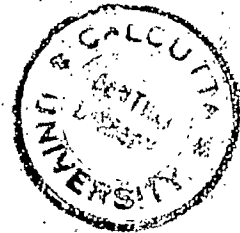


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1. If author's name is in the text, follow it with year in parentheses. ["... Duncan (1959) has proven that ..."] If author's name is not in the text, insert at an appropriate point the last name and year, separated by comma. ["... some have claimed (cf. Gouldner, 1963) that ..."]
2. Pagination (without "p." or "pp.") follows year of publication, separated by colon. ["... it has been noted (Lipset, 1964:61–64) that ..."] Incorporate within parentheses any brief phrase associated with reference. ["... have claimed that this is so (but see Jones, 1952:99 for a conflicting view.)"]
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List all items alphabetically by author and, within author, by year of publication, in an appendix, titled "REFERENCES." Use no italics and no abbreviations. For typing format, see the following examples:

Davis, K.

1963a "The theory of change and response in modern demographic history." *Population Index* 29(October):345–366.

1963b "Social demography." Pp. 204–221 in Bernard Berelson (ed.), *The Behavioral Sciences Today*, New York: Basic Books.

Goode, W. J.

1967 "The protection of the inept." *American Sociological Review* 32(February): 5–19.

Moore, Wilbert E., and Arnold S. Feldman.

1960 *Labor Commitment and Social Change in Developing Areas*. New York: Social Science Research Council.

Sanford, Nevitt (ed.)

1962 *The American College*. New York: Wiley.

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ITEMS

■ This issue of *ASR* marks the retirement of one editor, Karl F. Schuessler, and the initial efforts of another, James F. Short, Jr. The issue is, in fact, a product of our joint efforts, since evaluation and editorial decisions concerning three articles herein published were made in Bloomington; while the remainder are my responsibility. We are grateful to Professor Schuessler for his short "backlog" of articles accepted; and for his assistance in many ways, including especially his wise counsel during this transition period. He will indeed be a hard act to follow. Future issues of *ASR* doubtless will enjoy the Schuessler editorial touch, since his assignments to readers, suggestions to "revise and resubmit," and other editorial matters extend to many articles still in the process of review and revision.

■ This issue also marks the beginning of yet another chapter in history of the American Sociological Association publications, for it is the first *ASR* to be published without book reviews. We welcome the advent of *Contemporary Sociology*, ASA's new journal of reviews, and Editor Dennis Wrong. *ASR* began publication in February, 1936, some thirty-one years after the founding of the American Sociological Society in 1905. That first issue also included "Official Reports and Proceedings," "Persons and Positions," "Current Items" concerning departments, regional societies, and organizations of special interest, "Books Received" and notes on "Periodical Literature"—all functions since assumed by *The American Sociologist* and/or its companion vehicle from the Executive Office of ASA, the *Socio-Log*. These developments, and the publication by ASA of the specialized journals, *Sociometry*, the *Journal of Health and Social Behavior*, and *Sociology of Education*, and of the annual volume, *Sociological Methodology*, reflect changes within the discipline and the profession, and influence greatly the future of both. Inevitably, these changes also influence the character of *ASR*. We share the hope voiced by the Editorial Board of the first *ASR* that it will be "one of the most important working tools of American [and other] sociologists." Its function is disciplinary, in the broadest and, hopefully, the best sense. This we take to be our mandate.

■ We are happy to announce the appointment by the Publications Committee of the following As-

sociate Editors, whom we welcome with thanks: Charles Bowerman, Edgar G. Epps, Reynolds Farley, Travis W. Hirschi, Lewis M. Killian, Gwynn Nettler, Charles B. Perrow, Seymour Spilerman, and Harriet Zuckerman.

■ And, most important to the smooth functioning and quality of work of the editor's office, we are happy to introduce a new Deputy Editor: Lois B. DeFleur; a new Copy Editor: Loretta Anawalt; and a new Office Manager and Secretary: Sherry Bye.

■ This issue begins with "Tenting with Malinowski," by Murray L. Wax, Professor of Sociology at the University of Kansas (Lawrence). Stimulated by his wife's research into field work, Wax has been engaged in historical investigation and in the study of contemporary American Indian affairs, particularly the interactions between Indian communities and institutions of formal education.

■ Joel H. Levine currently is a Research Fellow at Harvard University and Visiting Scholar in Sociology at Dartmouth College. His current research interests include social networks (as in "The Sphere of Influence") and models of social stratification and mobility. Leo A. Goodman is the Charles L. Hutchinson Distinguished Service Professor of Sociology and Statistics at the University of Chicago, and Research Associate at the Population Research Center of the University. His current research includes work on a general model for survey analysis and on the further development of a variety of demographic topics. D. W. Britt and Omer R. Galle ("Industrial Conflict and Unionization") are Assistant Professor of Sociology and Social Psychology at Florida Atlantic University and Associate Professor of Sociology at Vanderbilt, respectively. Both are interested in comparative analyses of conflict. Britt also is engaged in longitudinal and experimental analysis of social control; while Galle's research interests include the relationship between population density and human behavior, and migration between urban areas.

■ Janice B. Lodahl is a doctoral student at the New York State School of Industrial and Labor Relations at Cornell and Associate and Co-director of the project on University Organization. "The

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ITEMS

April, 1971, ASR

■ Among the Items relating to the transition of editorship, I neglected to mention in February that our bright new cover is a product of designer Jerry Torrey. Mr. Torrey gave generously of his time and talents, for very little compensation. We thank him and hope our readership will approve.

■ Another change, the result of fiscal considerations, was the decision of the Council, upon recommendation of the Publications Committee, to replace the recently adopted sewn binding with a "Perfect" binding. The "Perfect" process is a considerable improvement over older processes, and it is a good deal less expensive than sewn binding. The printer indicates that this change will save the Association in excess of \$10,000 annually.

■ This issue includes studies of social organization and social participation, historical demography, political sociology, achievement aspirations and orientation, the economic and cultural determinants of aggression, and fundamental developments in children's interpersonal relations. Comments and rejoinders concerning one of the papers published in this issue and two published earlier conclude the issue.

■ The issue begins with two papers which seek to disentangle structural from social psychological influences, on organizational performance and on career attainments, respectively. **Stanley Lieberman** is Professor of Sociology and Associate Director of the Population Research Center, University of Chicago. He is currently comparing blacks and South-Central-Eastern European groups in the United States during the period from 1880 to the present. His co-author, **James F. O'Connor** is finishing the requirements for the Ph.D. in sociology at the University of Washington. His current research deals with the survival of American Federation of Teachers local unions. **David L. Feather-**

man is Assistant Professor of Rural Sociology and an associate of the Center for Demography and Ecology, University of Wisconsin-Madison. He has recently completed a monograph, *Socioeconomic Background and Achievement*, with O. D. Duncan and Beverly Duncan, and is analyzing unpublished data from the 1962 "Occupational Changes in a Generation" survey. With R. M. Hauser, he will be replicating and extending the Blau-Duncan OCG study.

■ Aspirations (for a college education) are the dependent variables in a third article, by **Joel I. Nelson**, Associate Professor of Sociology at the University of Minnesota. Professor Nelson has just returned from Montpellier, France, where he was a senior Fulbright lecturer in sociology at the Université Paul Valéry. His current research interests are on the structural sources of economic conflict in post-industrial societies. The present paper is one of a series of attempts to test at a microscopic level the utility of structural explanations.

■ **Gerry E. Hendershot**, Assistant Professor of Sociology at Vanderbilt, is studying the organization of family planning activities in a local community. **Thomas F. James**, Ph.D. candidate in Sociology at Vanderbilt, is studying the organizational structure of hospitals.

■ **William L. Parish, Jr.** is Assistant Professor of Sociology at the University of Chicago. He is completing a study of primary groups and industrialization in Taiwan and will soon begin a study of the informal organization of the Communist Chinese bureaucracy. **Moshe Schwartz**, Lecturer in sociology at Tel Aviv University, is completing a study of police and community relations in Chicago. **Gudmund Hernes** is Professor of Sociology at the University of Bergen. His major research interests are in the areas of collective decisions and social indicators.

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ITEMS

June, 1972, *ASR*

■ This issue includes papers on topics ranging broadly over social psychology, social organization, and research methods. Specific topics are similarly varied, as are comments and replies concerning four previously published papers. Authors of this array of edification and controversy are as follows:

■ **Joseph Berger** has recently completed a monograph on expectation-states theory with co-authors Thomas Connor (Michigan State) and Hamit Fisek (now in the army in Turkey). The monograph reviews a broad research program on the subject. **Bernard P. Cohen** has recently completed a monograph with Hans Lee (Michigan State) covering new models in the Asch experimental situation. And **Morris Zelditch, Jr.** is working on the effect of expectation-states on stability of authority in experimentally-created bureaucratic organizations, extending an earlier experiment with William Evan to nonprofessional organizations. The three co-authors have recently completed a monograph on the material described in the paper published in this issue of *ASR*, with additional detail on analysis of tests of the theory. All three are on the faculty of Stanford University.

■ **Yeheakel Hasenfeld** is Assistant Professor at the University of Michigan, School of Social Work. Currently engaged in a comparative study of juvenile correctional programs, his research interests are in the organizational determinants of clients' careers in human service organizations.

■ **Lawrence E. Hazelrigg** is Assistant Professor of Sociology at Indiana University. His current research interests include cleavage structures and cleavage-group identities in the context of political behavior, and occupational attainment processes in 19th century U.S. cities. His co-author, **Joseph Lopreato**, is Professor and Chairman of the Department of Sociology at the University of Texas (Austin). The Hazelrigg-Lopreato paper continues

Lopreato's long standing interest in social mobility and political attitudes.

■ **Seymour Spilerman** has spent the past academic year as a Project Director at the Russell Sage Foundation. He will return to the Department of Sociology at the University of Wisconsin (Madison) where he will be involved in analyses of data from the New Jersey Negative Income Tax Experiment and with additional materials related to social and racial violence. A third "mobility" paper is authored by **Roland K. Hawkes**, Assistant Professor of Sociology at Southern Illinois University (Carbondale). His research interests include multivariate models of the stratification process and the development of statistics for ordinal measures.

■ **Michael Armer** is Associate Professor of Sociology at Indiana University, and **Allen Schnalberg**, Assistant Professor of Sociology at Northwestern University. They are presently engaged in a replication of the research reported in this issue using data from rural and urban samples of Costa Rican males. In addition, they are using the data from these studies to investigate substantive questions and to illustrate certain problems in measurement evaluation with social survey data. Schnalberg is also engaged in research on the interrelation between social stratification and environmental processes.

■ **Marvin E. Olsen** is Associate Professor of Sociology and Director of the Institute of Social Research at Indiana University. During the 1971-72 academic year he has been on sabbatical leave at Uppsala University, Sweden. In addition to teaching and writing there, he is gathering survey data on political participation with which to replicate the research reported in this issue. He is also studying ways in which interest organizations attempt to influence political decision making and administration in Sweden.

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ITEMS

August, 1972, ASR

■ As this issue is put to bed we look forward to the ASA meetings in cool New Orleans, and the opportunity to meet with Associate Editors and many others whose efforts in behalf of the discipline make it possible to publish ASR. As I write in early July, summer seems already foreshortened. Before New Orleans, like many of you, I hope to accomplish a bit of research and writing, and vacation.

This issue features empirical studies of alienation and work, deviance, role differentiation, organizational structure and change, and science and economic development, as well as theoretical papers on exchange as symbolic interaction and methodology. It concludes with an exchange of views concerning cumulative knowledge in sociology.

■ The issue begins with a paper by **Melvin Seeman**, Professor of Sociology at the University of California (Los Angeles). Seeman is continuing comparative research on alienation, and the development of quasi-experimental studies in natural settings. He will re-interview his sample of French and American workers in the coming year to determine patterns of participation and attitude change since the French events of May, 1968.

■ **Walter D. Connor** is Assistant Professor of Sociology and an associate of the Center for Russian and East European Studies, University of Michigan. Besides deviance and dissent, he is currently concerned with stratification and mobility in socialist societies—interests he will pursue while on leave in 1973, dividing his time between the Russian Research Center at Harvard, and Moscow State University.

■ Two social psychological papers are next. **Peter Singelmann** is an Assistant Professor at the University of Missouri, Kansas City. His current research involves an exchange-theoretical analysis of *campesino* movements in Latin America. **Gordon H. Lewis** is Associate Professor of Sociology at Carnegie-Mellon University. His current interests

are in interpersonal hierarchies, social class perception, and decision-making problems.

■ **Marshall W. Meyer** teaches in the New York State School of Industrial and Labor Relations and the Department of Sociology at Cornell University and is continuing his research on government bureaucracies. Data on organizational size, structure, technology, environment, and effectiveness are being gathered in extensive interviews with officials of finance departments. Since comparable information was obtained in 1966, longitudinal analysis will be possible. **Ronald Corwin** is Professor of Sociology, Ohio State University. His article on organization change grows out of previously published research on organizational conflict and reflects a convergence of his interests in complex formal organizations and the sociology of education. The article is part of a larger study on organizational change which is being published by Wiley-Interscience in February. He is planning a comparative study of inter-organizational and client-organization relationships.

■ **Marion Blute** is a Ph.D. candidate at the University of Toronto. Her research interests center on topics in the fields of economic development and social stratification, and on the possibilities for a sociology of mathematics.

■ **Kenneth Wayne Taylor**, Assistant Professor at the University of Manitoba, Winnipeg, Canada, teaches methodology and industrial sociology. He has recently completed a study of organizational growth and development using a goal specificity variation model. He is currently researching the growth and development of industrial structures in hinterland areas. **James Frideres** is Assistant Professor at the University of Calgary, Alberta, Canada. Methodological research under way includes a study of the consequences of misapplications of survey research techniques. He is currently also researching Canadian ethnic relations and participation of women in the Canadian labor force.

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ITEMS

October, 1972, ASR

■ A broad range of fundamental problems of human relations—their forms, correlates and possible explanations—are featured in this issue. We begin with **William J. Goode's** presidential address before the annual ASA meetings in New Orleans, a searching inquiry into "The Place of Force in Human Society." Goode, Professor of Sociology at Columbia, and among the most honored and admired of sociologists for his accomplishments "on and off the field," to use another journalistic mode, continues his interest in control systems. His paper is followed by two papers describing patterns of violence in France and in selected cities in the U.S. Authors of these and other papers report the following:

■ **Charles Tilly** is Professor of Sociology and History at the University of Michigan, where his co-author, **David Snyder**, is a doctoral student. Tilly continues his long standing interests in structural change and political conflict in Western Europe. Snyder is working on models of industrial conflict in Europe and the United States over the past century. **Richard A. Berk** is Assistant Professor of Sociology and Urban Affairs at Northwestern. With Peter Rossi and Bettye K. Eidson, he is completing a book (*The Roots of Urban Discontent*) on the politics of city services. Other research includes a study of the politics of passage of penal codes, theoretical work on collective violence, and collection and analysis of data for progressive Chicago groups. **Howard Aldrich** is Assistant Professor of Organizational Behavior in the New York State School of Industrial and Labor Relations, at Cornell. His current research is in two areas: interorganizational relations in the manpower training system and the relationship between small businesses and their urban environment. He is studying the employment impact of white owned businesses on the black ghetto and, with Albert J. Reiss, Jr., the survival and ownership turnover of white owned businesses in the black ghetto.

■ **Edna Bonacich**, Assistant Professor of Sociology at the University of California, Riverside, teaches ethnic relations and sociology of conflict. Her current research includes a study of three generations of Japanese Americans (the Japanese American Research Project at UCLA), a theoretical analysis of middleman minorities, and work on simulation. **Marvin E. Olsen** is Associate Professor of Sociology at Indiana University. He is working on a monograph based on interviews with organizational and governmental elites in Sweden, focusing on ways in which interest organizations influence political decision making in this society. **Judy Corder Tully**, Assistant Professor of Sociology, University of Texas (Austin), is analyzing data on the relationship of sex roles to academic performance and career aspirations of junior high school age children. She is also interested in reactions to and effects of the women's movement.

■ **Lewis A. Coser** is Distinguished Professor of Sociology at the State University of New York, Stony Brook. He is presently working on a series of studies on "greedy organizations," i.e., organizations which wish to absorb the total personality of their personnel rather than rest content with specific role obligations. In addition, he is preparing the ground work for a large-scale study of the publishing industry as a "gate keeper of ideas."

■ **Ephraim Yuchtman** is Senior Lecturer in Sociology at Tel-Aviv University and for the past year he has been Visiting Associate Professor in the Department of Sociology and the Institute for Social Research at the University of Michigan. His chief interest is in reward structures in small groups and larger organizations, the determinants of preferences among alternative principles of reward distribution and their consequences for the individual and the organization. He is also interested in social stratification and mobility and in organizational behavior and effectiveness. **Bill Tudor** is Assistant Professor of Sociology at Central Michigan Uni-

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O'DONNELL, JOHN A.
 OFSHEE, RICHARD J.
 OGLES, RICHARD H.
 OHR, MICHAEL J.
 OLESEN, VIRGINIA L.
 OLMSTED, DONALD W.
 OLSEN, MARVIN E.
 OLSEN, STEPHEN M.
 OLSON, PHILIP G.
 PAIGE, JEFFERY M.
 PALMER, STUART
 PALMORE, JAMES A., JR.
 PATCHEN, MARTIN
 PAVALKO, RONALD M.
 PELLEGRIN, ROLAND J.
 PERRUCCI, ROBERT
 PILIAVIN, IRVING M.
 PITTMAN, DAVID J.
 PIVEN, FRANCES
 PLOCH, DONALD R.
 POLOMA, MARGARET M.
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 QUARANTELLI, ENRICO L.
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 REISS, ALBERT J., JR.
 REISS, IRA L.
 REISS, PAUL J.
 RICHARDSON, ARTHUR H.
 RILEY, MATILDA W.
 ROBERTS, LYNN
 ROGERS, EVERETT M.
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 ROSEN, BERNARD C.
 ROSENBERG, BERNARD
 ROSOW, IRVING
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 SARTLIOS-ROTHSCHILD, CONSTAN-
 TINA
 SAKODA, JAMES M.
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 SCHMID, CALVIN F.
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 SCHNAIBERG, ALLAN
 SCHNEIDER, LOUIS
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 SCHOENHERR, RICHARD A.
 SCHRAG, CLARENCE C.
 SCHUESSLER, KARL F.
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 SCHWARTZ, NEENA B.
 SCHWARTZ, SANDRA K.
 SCOTT, W. RICHARD
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 SEGAL, DAVID R.
 SEWELL, WILLIAM H.
 SEXTON, PATRICIA
 SHARP, LAWRENCE J.
 SHOSTAK, ARTHUR B.
 SIEGEL, PAUL M.

SILLS, DAVID L.
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SIMON, WILLIAM
SIMPSON, MILES E.
SIMPSON, RICHARD L.
SINGELMANN, PETER
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SMELSER, NEIL J.
SMIGEL, ERWIN O.
SMITH, PHILIP C.
SMITH, ROBERT B.
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STORER, NORMAN W.
STRATTON, JOHN R.
STRAUS, MURRAY A.
STREIB, GORDON F.
STRONG, MARY S.
SUELZLE, H. MARIJEAN
SWANSON, GUY E.
SWARTZ, PEPPER
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TAEUBER, KARL E.
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TILLY, CHARLES
TITTLE, CHARLES R.
TUDOR, BILL
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TURNER, RALPH H.
TURNER, STEPHEN
UDRY, J. RICHARD
UDY, STANLEY H., JR.
USEEM, RUTH H.
VAN ARSDOL, MAURICE D., JR.
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VINTER, ROBERT D.
VOLKART, EDMUND H.
VON BROEMSEN,
MAXIMILIAN H.
VOSS, HARWIN L.
WALBERG, HERBERT J.

WARREN, DONALD IRWIN
WASHBURN, NORMAN F.
WAX, MURRAY L.
WAXLER, NANCY E.
WEBSTER, MURRAY A., JR.
WEIGERT, ANDREW J.
WEINSTEIN, EUGENE A.
WEITZMAN, LENORE J.
WHITE, HARRISON C.
WHITE, MERVIN F.
WHYTE, WILLIAM FOOTE
WILEY, JAMES A.
WILKINS, JAMES L.
WILLER, DAVID E.
WILLIAMS, GERALD W.
WILLIAMS, JAMES L.
WILLIAMS, ROBERT
WILLIAMS, WALTER
WILSON, THOMAS P.
WINCH, ROBERT F.
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ZEITLIN, IRVING M.

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TENTING WITH MALINOWSKI *

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MURRAY L. WAX

Department of Sociology, University of Kansas

Using his recently published personal diaries and his monographs, as well as the ethnographic reports of Melanesia produced by other scholars, I examine the circumstances by which Bronislaw Malinowski was led to the discovery of intensive personal fieldwork. Crucial features of the situation in which he worked include the colonial situation with its caste barrier and exploitation of natives and the Cargo Cults which were emerging among the latter. By settling within a Melanesian society unusual in its stratified character, Malinowski was able to avoid some of the problems of parity between anthropologist and native people. Nevertheless, it is hard to classify his work as participant observation, although he did move the next generation of anthropologists toward that style of fieldwork.

I shall invite my readers to step outside the closed study of the theorist into the open air of the anthropological field, . . . There, paddling on the lagoon, watching the natives under the blazing sun at their garden work, following them through the patches of jungle, and on the winding beaches and reefs, we shall learn about their life (Malinowski, 1926/1955:99).

An iconoclast all his life, Malinowski has in this gross, tiresome, posthumous work (the diaries), destroyed one final idol, and one he himself did much to create: that of the fieldworker with extraordinary empathy for the natives (Geertz, 1967:12).

IN the development of a science, an improvement in the quantity and quality of data has far-reaching effects. Such has been the case with anthropology, which was

markedly stimulated in its growth by the discovery early in this century of the personal field-trip intensively focused on a single people. This anthropological discovery also influenced sociology, reinforcing the development of a research technique already practiced by people like Paul Göhre and Beatrice Potter Webb.

Like many other scientific discoveries of major impact, the intensive field-trip appears an obvious step in retrospect. By now, anthropologists take its presence so much for granted that they sometimes attribute the discovery to simple accident. They argue that Malinowski was an enemy alien in British colonial territories during the first World War, and not allowed to travel. Having to remain in residence among the Melanesian natives, he thereby discovered the advantages of intensive fieldwork. This explanation as we shall see is false to fact and diminishes the nature and significance of his discovery.

Social and cultural anthropologists today expect that each professional will engage in intensive fieldwork and base his scholarly writing directly on privately gathered data. Yet, it is not at all obvious that he should do so. In many sciences the theoretician works

* Sabbatical leave granted by the University of Kansas provided me with the opportunity to compose this essay. Conversations with Karl Heider furnished some valuable background information about Malinowski. In initial drafts, the essay was read and criticized by George W. Stocking, Jr., who is himself engaged in research on that controversial figure; I have profited greatly from his comments. In a number of ways the present essay develops further the ideas about the history of social science and its field techniques outlined in the writings listed in the bibliography of Rosalie H. Wax and myself.

with data obtained by those who specialize in experimenting and observing. Indeed, much of the anthropological theorizing of the 19th and early 20th centuries was built on the observations of missionaries, traders, explorers, and government administrators who happened to have contact with exotic peoples. Necessarily, these data were uneven, and often obtained under erratic conditions by careless or unreliable observers. To modern students of anthropology it comes as something of a shock to realize that scholars as renowned in their time as Edward B. Tylor, James G. Frazer, Lucien Lévy-Bruhl, or Edward Westermarck should have based their arguments solely on data obtained this way.

As with any major scientific advance, historians could engage in a complex analysis of just which individual should be credited with which feature of the fieldwork discovery and in what paradigm. We have noted that some explorers, adventurers, and missionaries served as observers in field situations, and one could argue that some of their work might qualify as "intensive personal fieldwork among a single people." Charles Doughty, Arminius Vámbéry, and Vladimir G. Bogoraz, to name an outstanding few, could well sustain intensive historical study. Yet, despite the excellence and interest that still attaches to their reports, they did not influence the conduct of anthropological scholars. The case of the Torres Straits Expedition of 1898-99 is instructive, as in the judgment of Evans-Pritchard (1951:73), it

... had many weaknesses. However well the men who carried it out might have been trained in systematic research in one or another of the natural sciences, the short time they spent among the peoples they studied, their ignorance of their languages, and the casualness and superficiality of their contacts with the natives did not permit deep investigation.

The acquiescence of renowned scholars to this kind of superficial fieldwork with exotic peoples cannot be explained by their ignorance of science or indifference to its canons. Something more is involved. For we must account for the fact (Evans-Pritchard 1951:71-72) that

... with the exception of Morgan's study of

the Iroquois, not a single anthropologist conducted field studies till the end of the nineteenth century. It is even more remarkable that it does not seem to have occurred to them that a writer on anthropological topics might at least have a look, if only a glimpse, at one or two specimens of what he spent his life writing about. William James tells us that when he asked Sir James Frazer about the natives he had known, Frazer exclaimed, 'But Heaven forbid!'

Together with other historians of anthropology and sociology, I would suggest that fieldwork in the 19th century was inhibited by the ideology and politics of the time. Ideologically, the doctrine of social evolutionism predominated. Leading social thinkers, like August Comte, Herbert Spencer, or Edward B. Tylor looked to the exotic or in current parlance "technologically primitive" peoples of the world to learn about the evolution of man. These exotic peoples were "savages," "Stone Age men," who bore the remnants of primitive antiquity. They merited study, not in their own right as human beings, but because such study might illuminate the origins of civilized, European Society and might help explain such puzzling, less civilized, features of that society as its religious institutions.

Discoverer and Exemplar

In the final analysis, the major credit for discovering the technique of intensive personal fieldwork among a single people must go to Bronislaw Malinowski (1884-1942).¹

¹ Because Franz Boas (1858-1942) was such a forceful critic of armchair theorizing on anthropological data of poor quality, he is sometimes credited with being the initiator of intensive fieldwork. But except for his youthful field trip to Baffinland in 1883-84 as an "anthro-geographer," his own field research was composed of a large number of brief visits touching on a multiplicity of areas (for further details see Stocking 1969: chap. 7; Rohner 1966). Boas perceived that the technique of working through interpreters was less than satisfactory, and he (1911:60) advocated that fieldworkers learn the native language, "because much information can be gained by listening to conversations and taking part in their daily life." However, he also recognized that this ideal usually could not be realized. Boas and his early students, e.g. Alfred L. Kroeber and Robert H. Lowie, conducted their field researches under severe pressures of time and money and in the face of a despair that native cultures were rapidly disappearing. In

His researches among the Trobriand Islanders during the years 1916-18 yielded a series of epochal volumes which revolutionized the content and practice of anthropology. He was well aware of the novelty of what he had done. In chapter 1 of his first commercially published monograph, *Argonauts of the Western Pacific* (1922), he discusses at length how to conduct anthropological field research; and, as if to dramatize the point, places first in the book a photographic plate of "The Ethnographer's Tent on the Beach of Nu'agasi."²

Throughout *Argonauts* and his subsequent books, Malinowski emphasized his method of research in the Trobriands, and polemical against the theorist in "the closed study." Ironically, the quoted passage which opens this essay appears in an address delivered by Malinowski in honor of Sir James Frazer, a model closeted theorist. Malinowski's eloquence in book and lecture decisively influenced the next generation of anthropologists. A number of his students accomplished heroic and ingenious feats of personal fieldwork, out of which flowed a series of splendid monographic reports. One of the first and most eminent of his students (Evans-Pritchard 1951:74-75) pays tribute to his influence:

It can be fairly said that the comprehensive field studies of modern anthropology directly or indirectly derive from his [Malinowski's] teaching, for he insisted that the social life of a primitive people can only be understood if it is studied intensively, and that it is a necessary part of a social anthropologist's training to carry out one such intensive study of a primitive society.

Though we may agree that Malinowski must be credited with pioneering personal in depth field study of a single people, the process by which he developed this pro-

cedure is by no means clear. He tells the reader of *Argonauts* that he pitched his tent in the village of Omarkana in the Trobriand Islands, but it is not clear how he came to live there rather than, as was more customary, in the household of a missionary or trader.

One would expect an enthusiastic reception for diaries born of such intimate circumstances as Malinowski's with the Trobriand natives. Instead in 1967 when two diaries covering portions of the years 1914-18 were translated from the Polish and published, they were greeted with a chorus of anthropological dismay (e.g. Hogbin 1968; Hoebel 1967; and a more balanced appraisal in Stocking 1968). Reviewers expressed shame and regret that a hero of cultural anthropology should have revealed himself as "a crabbed, self-preoccupied, hypochondriacal narcissist" (Geertz 1967) and racist. In defense of her teacher, Hortense Powdermaker (1967) contended that some of the offensive terminology could be an artifact of translation. Unfortunately, for her thesis, the polyglot Malinowski used words and phrases from many languages in these diaries, and "nigger" was among them (1967: xxi, 154).³

³ With the exception of Stocking (1968:194) none of the reviewers or commentators complained because the original text was not provided or because "a few extremely personal observations" (Malinowski 1967:viii) had been deleted from the published text. The practice of ethnographic field research is difficult, and many anthropologists have found it hard to be as frank about their experiences as scientific disclosure would require. But editorial deletions at this date from the journals of a man of this stature only suggest fear of damaging an idol wobbly on his pedestal. Also, considering the care and devotion of the translator and annotator, the absence of any sort of index to the volume is inexcusable.

⁴ The diaries do remind us that he twice journeyed from New Guinea to Australia and that on the second of these occasions he remained there from about May 1916 to September 1917. This travel should deflate the myth that he had to do fieldwork because as an enemy alien he was confined to the area of New Guinea. Conceivably, he might have located himself in some Australian university setting for the remainder of the war, rather than returned to a tent in the Trobriands.

Of course, it is true that as an Austrian subject, he was technically an enemy alien. But as Marett reminisces (cited in Symmons-Symonolewicz 1959: 20): "Nothing, however, could have been more generous than the treatment by the Australian

a sense, Malinowski's researches among the Trobriands translated into dramatic reality what Boas had earlier seen as an almost unattainable idea.

² For those readers who think of Malinowski's Melanesian researches as having been confined to the village of Omarkana (Kiriwina District, Boyowa Island, Trobriand Archipelago), this plate is a helpful reminder that his place of residence was not quite so stationary. As his diaries reveal, he traveled a great deal, and especially during his second trip to the Trobriands, made journeys to such neighboring areas as the Amphlett Islands, where Nu'agasi is located.

To a reader curious about Malinowski's field experiences, the diaries appear strangely bare. Crucial events are seldom indicated, and no diary has been located for the strategic period when he first began work among the Trobriand Islanders.⁴ Nonetheless, the diaries are helpful if viewed in their cultural and historical context and addressed with the right questions. When we treat his discovery as a thing born of chance and bred of desire, when we give him heroic stature as the first and finest of ethnographic fieldworkers, we are making myth and not rightly understanding his achievement. To understand the process of Malinowski's discovery, we must discard the image of fieldwork as the unilateral action of a researcher independent of people and circumstance, and instead study the interaction of the man and "his people" within a larger context. That larger perspective is the focus of this essay.

Europeans and Melanesian Natives

By 1914 when Malinowski arrived in New Guinea to undertake his researches, the relationships between the European Colonial administrators and the natives of Melanesia had evolved into a rigid caste structure. In European eyes, the natives were "Niggers" to be missionized, educated, ultimately civilized, and in the interim, employed for menial labor in households and plantations. Only the very lowest ranks of the British civil service were open to natives. All other positions were reserved for Europeans. Even as late as 1947 a Melanesian as talented, experienced, and influential as Yali could not be promoted to the rank of Patrol Officer and admitted by the British to the status of social equality (Lawrence 1964:157-159).

Melanesian societies had been thoroughly disrupted and their populations reduced by European contact, trade, governance, and missionizing. In the British areas, a system of indirect rule had been imposed, so that in each village native officials were responsible to the central administration for executing British laws. Some of these laws were reasonable, but many the natives perceived as

irrational and impracticable. Though it had been a highly ritualized affair integral to the religious and status systems of many societies, native warfare had been terminated. Taxation had been imposed whose avowed intention was to force native men into plantation labor. In some cases, according to Worsley, as much as 20 percent of the male population were forced from their local residence, setting askew local systems of food production and social organization. Europeans had preempted native lands for plantations or mines. Missionaries spread throughout establishing styles of living which elevated them socially and politically above their flock. Corporal punishment reinforced British law.

Working conditions for Melanesian laborers were sometimes appalling. In the Mambare goldfields, the death rate was 21% in 1903-4 and in 1906 it was still 17.7% (Worsley 1968:57). By law the minimum wage at Mambare was ten shillings per month. In Rabaul in 1929 the average laborer earned six shillings per month, and when the natives struck non-violently, the employers retaliated with corporal punishment and long sentences to hard labor (Worsley 1968:47). The British administration made some attempts to regulate conditions, but even these reveal the general attitude of mind. Note Malinowski's diary comment of October 1914, (1967:17): "I had little opportunity to work with Ahuia, for he was busy with the trial of Burnesconi who had hung up a native for five hours."

These severe disturbances to the organized lives of their communities triggered a series of radical "Cargo Cult" movements among the Melanesians. To comprehend these movements, we should bear in mind that the Melanesians had become dependent on trade with the Europeans. Thus the cargoes deposited by ships on the local wharves assumed great importance. The Europeans took possession of these cargoes by a system the natives regarded as magical, and they doled them out to the Melanesians in a manner that appeared arbitrary and stingy. What rankled deepest was the European's denial to the natives of parity, that is, of social equality, fraternity and common humanity. The social movements or "Cargo Cults" thus triggered among the natives prophesied

authorities of the young scholar, for they not only granted him a *libera custodia* so that he could explore where he chose within their vast territories but actually supplied him with the funds to do so."

a day when the ships would come bringing cargo to the natives and not the Europeans.

My guess would be that Cargo Cult activities were underway in Mailu, where Malinowski first did his researches.⁵ I suggest this because Mailu was close to New Guinea and to commercial centers such as Port Moresby and because a missionary lived there whom Malinowski portrays as quite obnoxious. Such movements could account for his frustrated attempt to conduct research there and his later reference to "my Mailu failure." In any case, his diary reveals that during this first expedition, he met some clearly antagonistic responses: natives laughed at his inquiries, stared at him, lied to him, and were rowdy in dealing with him (cf. 1967:31, 43).

On the other hand, most authorities emphasize that Trobriand Island society was differently organized from societies elsewhere in Melanesia. It was less egalitarian and contained lineages of such differential rank that Malinowski could speak (1929: chap. 2) of some natives as being aristocratic, and it contained men of such differential ranking that one was recognized as "Paramount Chief." A native society that acknowledged principles of hierarchy and centralized political leadership, might not feel the assertion of British governance as being as unfamiliar and disorganizing as might the egalitarian communities described by Burridge, for example. Nevertheless, for the Paramount Chief himself, the results were traumatic (Worsley 1968:51-52):⁶

Sir William MacGregor, the new Administrator of British New Guinea, was very concerned to ensure that "the rapacious acts of the benighted heathen" gave place to "law and good order under the munificent and wise

guidance of a beloved Administrator." Sir William was determined to enforce recognition of the new authority of the Queen and her representative. In one of the most politically developed societies of New Guinea, he enforced his authority in a typical manner. During a visit to the Trobriand Islands, he noted the Paramount Chief seated on his high platform. This custom ensured that nobody stood higher than the chief, and made it possible for his subjects to pass by him without their having to crawl on the ground. Sir William "saw the chief sitting on high, in his seat of honour, walked straight up to him, seized him by the hair, and dragged him to the ground, and took his seat himself. "No one," said Sir William, "shall sit higher in New Guinea than I."

Subsequently, in 1915 when Malinowski arrived in Omakana, he found (1935/1965:I, 84) that the Paramount Chief, To'uluwa was

... a shrewd, well-balanced man, but his pride had been broken by the European invasion, and he had retired from most of his offices.⁷

Malinowski Leaves the Community of White Colonists

When he arrived in New Guinea, Malinowski would thus have found himself in a difficult situation for social research. Between the world of the Melanesian natives and of the British colonial administration was a steep barrier of status, wealth, and power. This barrier was augmented by cultural and linguistic differences. Few of the

⁷ "When To'uluwa, the paramount chief of the Trobriands, was put in jail by the resident magistrate, the latter, mostly, I am afraid because he wanted to humiliate his native rival, forbade the commoners incarcerated with the chief to crouch before him" (Malinowski 1929/1969:33 n.2).

Malinowski comments (1935/1965:I, 479-80) that the eclipse in the power of the Paramount Chief modified the organization of Trobriand society:

The paramount chief and his peers in other districts are no longer the only people or even the main people who wield power and of whom one has to be afraid. There is a resident magistrate who can put you in gaol, fine you, or even—as has happened once or twice—hang you. His law has to be obeyed. There are the missionaries who moralise, pester and shame you into doing this or abstaining from that. There are the traders who exercise a different but not less powerful influence by giving or withholding things which have become almost a necessity. . . . The empirical facts which the ethnographer has before him in the Trobriands nowadays are not natives unaffected by European influences but natives to a considerable extent transformed by these influences.

⁵ I have not attempted to research the ethnohistorical literatures for signs of Cargo cult activities in the areas where Malinowski worked (principally Mailu Island (1914-15, the Trobriands 1915-16 and 1917-18). The problem is that a negative finding would be far from conclusive (Worsley 1968:93):

... there seems little doubt that many movements have flourished secretly without ever coming to the attention of the authorities. But the records which do exist show that an extraordinary number of cults did spring up over the whole of Melanesia, and that they shared many features in common.

⁶ Worsley seems here to be citing from the writings of J. P. Thompson (1892) and of Sir Hubert Murray (1928), but I find his references at this point confusing.

colonists spoke native languages or understood native customs, and conversely few of the natives spoke English or understood the customs of Europeans. Born into the Polish gentry and educated at leading universities, Malinowski would have been expected to find his basic social ties with the colonists and to study the natives as might zoologists their animal specimens. Association with natives on a level of parity would have been regarded as a challenge to the caste structure and a betrayal of upper caste solidarity. One can read Malinowski's diaries as a testament of his difficulties with this social situation and of uncertainty about his proper social role.⁸

Malinowski's writings, both then and later reveal a strong antipathy to the structure of colonial rule. From the outset the diaries express open outrage against the missions. The diaries also indicate that he found some of his colonial associates distasteful and others "loathesome" in their lack of finer sensibilities:

This [missionary] disgusts me with his [white]⁹ superiority (1967:16, October 21, 1914).

Beautiful moonlit nights on the veranda at Mr. and Mrs. McGrath's—I am filled with dislike for these ordinary people who are incapable of finding a glimmer of poetry in certain things which fill me with exaltation (1967:21, October 5, 1914).

Compared to his colonial associates, Malinowski was a remarkably well-educated man. He finished his Ph.D. in 1908 in Physics and Mathematics, at the University of Cracow, and did further graduate study at Leipzig and the London School of Economics. He spoke and read several European languages. His diaries revealed a man who continually sought refuge in reading novels of all levels, from classics to trash, in English and other languages. Later in life, he was once to remark to Mrs. B. Z. Seligman, "Rivers is the Rider Haggard of anthropology; I shall be the Conrad!" (R. Firth

1957/1964:6). In the light of such education and predispositions, his antipathy to the colonists is not surprising.

In the *Argonauts*, Malinowski further criticizes the colonists (1922/1961:5-6) for their subjective and unserious view of native life:

Information which I received from some white residents in the district, valuable as it was in itself, was more discouraging than anything else with regard to my own work. Here were men who had lived for years in the place with constant opportunities of observing the natives and communicating with them, and who yet hardly knew one thing about them really well . . . Moreover, the manner in which my white informants spoke about the natives and put their views was, naturally, that of untrained minds, unaccustomed to formulate their thoughts with any degree of consistency and precision. And they were for the most part, naturally enough, full of the biased and pre-judged opinions inevitable in the average practical man, whether administrator, missionary, or trader, yet so strongly repulsive to a mind striving after the objective, scientific view of things. The habit of treating with a self-satisfied frivolity what is really serious to the ethnographer; the cheap rating of what to him is a scientific treasure, that is to say, the native's cultural and mental peculiarities and independence—these features, so well known in the inferior amateur's writing, I found in the tone of the majority of white residents.

The diaries for his first field-trip reveal that Malinowski was often miserable in New Guinea and that on this first trip he lived most of his time in conventional residences, slept in conventional beds, and walked to the villages to conduct his ethnographic labors. True, the information in the diary is slender, but certainly comments such as that for January 23, 1915 (1967:72), "Yesterday I walked to the village at 7," or, that for the following day, "I could barely drag myself to the village," testify to a residence outside the native quarters. Furthermore, on boat trips, his gear did not include a tent. In some instances, (December 12, 1914; 1967:53) he reports that he "went to the *dubu* [clan clubhouse], . . . to spend the night. Very bad night." In others (December 22, 1914), he stayed with the missionaries or (February 2, 1915) in the M.G. (Mission Government) station.

The appeal to Malinowski of a tent residence in a village is suggested in the lan-

⁸ For example, of a conversation (November 23, 1917) with Judge J. H. P. Murray and his nephew, Leonard, important officials in the region, Malinowski remarks in his diary (1967:128): "I tried to control myself and to remember that I worked with immortality in view and that paying attention to this crew simply banalizes my work."

⁹ This word is inserted by the editor of the diary.

guage of his first diary when he comments on February 22, 1915, in the course of a trip to Dikoyas, on a stay in a village on the north shore of Woodlark Island, (1967:88):

I am in a tent of palm leaves pieced together over a shaky floor made of sticks. The open side faces the village about 60 m. away, below me, as on the palm of my hand. . . .

If I interpret his narrative correctly, on this same trip we encounter all together his ecstatic praise of the local landscape, his delight in native company and his customary testy comments on exchanges with the Europeans.

Diary references to his illness, lethargy, and irritation with fellow Europeans occur often in his comments on the trip to Woodlark Island: "Never before in N.G. [New Guinea] have I felt so low." Often, as in this instance his despondency appears (1967:88-89) in counterpoint to his esthetic pleasure with the landscape: "After landing in this village, I took a marvelous walk through the tall luxuriant jungle, à la Kandy—I felt fine again, in my element." And, as far as I have noted for the first time (1967:92) we encounter an explicit expression of his preference for life with the natives:

Then stop in the woods. The boys [New Guinea servants] carried my things. Marvellous, enormous wilderness . . . Like *Lady Horton's drive* in Kandy. The candelabra of ferns on the trees; the enormous trunks of the gigantic trees . . . After some time we entered a dry jungle. I was madly happy to be alone again with N.G. boys. Particularly when I sat alone in a hut, gazing out at the village through the betel [palms].

In the *Argonauts* (1922/1961:6) he tells the reader:

. . . in my first piece of Ethnographic research on the South coast, it was not until I was alone in the district that I began to make some headway; and, at any rate, I found out where lay the secret of effective field-work. . . .

Referring the reader to the photographic plates which show the location of his tent within the native village, he then instructs him in "The Proper Conditions for Ethnographic Work":

These, as said, consist mainly in cutting oneself off from the company of other white men, and remaining in as close contact with the

natives as possible, which really can only be achieved by camping right in their villages.

Compared then to living among the Europeans, Malinowski found living among the natives personally more pleasant and scientifically more productive. However, such preferences do not exactly explain the tent. The natives lived in what he described as "huts," and the administrators and missionaries had their fixed residences, the "stations." Conceivably, Malinowski, had he wished to settle in some village for an intensive lengthy study, would have requested the natives to construct a dwelling for him or might have asked the administration for an unoccupied mission station. However, at the time that Malinowski embarked on his second field-trip, he was not sure where he was going to work or settle, hence the necessity for a tent (1935/1965:I, 453):

When in my second expedition I arrived in the Trobriands (June 1915) I had not prepared myself for work in that language, because I did not intend to settle in that district for any length of time.

The tent testifies to the fact that prior to his stay in the Trobriands, Malinowski had not yet discovered the full "secret of effective field research." He had learned that he could do better research by being alone with the natives, but he had not yet hit upon the overwhelming advantages of *living intimately and for a prolonged period of time within a single native community whose language he had mastered*. This was to dawn upon him as he settled in the village of Omarkana, learned its language, and observed the lives of its inhabitants.

Malinowski among the Savages

Anthropologists have experienced some difficulty comprehending Malinowski's fieldwork by reading into it a portrait of fieldwork as it subsequently evolved. By 1966, Hortense Powdermaker was referring to anthropological fieldwork as "participant observation." That she should do so is of special significance, since she was among Malinowski's earliest students, beginning her work under him at the London School in 1925, and like a true disciple, conducting her first field researches among a Melanesian people in the villages of Lesu, New Ireland, 1929-30.

While anthropologists or other social-scientists have not agreed on the exact referent of such phrases as "ethnographic field research" or "participant observation," the latter epithet carries a denotation quite absent in the former, especially as ethnography was practiced by Malinowski's predecessors early in the century. The word "participation" implies that the fieldworker seeks involvement in the daily activities of the group he is studying. To some romantics, and some green students of anthropology, the notion suggests "going native," immersing oneself in the life of the group, becoming indistinguishable from a native. In the case of research among a people as exotic as the Melanesians, such an expectation is absurd. In the time at his disposal and given his scholarly goals, no fieldworker can hope to become a native; and no native could ever be led to conceive of him as such. On the other hand, in research focusing on Euro-American domestic institutions, it is sometimes possible for a fieldworker to immerse himself and become an ordinary participant. Many social institutions, such as prisons, mental asylums, or army basic training, are explicitly designed to ingest outsiders and transform them, willy nilly, into inmates, soldiers, or insiders. Similarly, something of the connotation of "participant observation" derives from the sort of transformation of the self that can occur in the process of domestic research (cf. R. H. Wax 1971).

In the case of research among exotic peoples, participation implies social parity, a recognition of common humanity, in which distinctions of ethnicity or skill are not to be taken as symbols of superordination.¹⁰ Participation further implies willingness to share the experiences and hazards of life. Without participation and parity, the interaction between the educated professional researcher and, for example, a colonized group of Melanesian natives, rapidly degenerates into the relationship of a plantation manager to his laborers. The manager may think he knows his men, he may even speak a few of their phrases and perceive some of their

¹⁰ I take the term "parity" from the insightful analysis of Cargo Cults by Burridge (1960). The term is utilized and developed further in relationship to participant observation by Rosalie H. Wax (1971).

customs, but he never seeks to know in depth their hopes and fears, their acts and avoidances, their picture of the world.

In the generation after Malinowski, the fieldworking anthropologist learned that some effort at establishing parity was essential to a mutually satisfactory relationship with an exotic people (cf. Powdermaker 1966:263-264; Burridge 1960:1-13). To attempt participation was one of the most convincing symbols of the fieldworker's desire for parity. Particularly in colonial situations, the anthropologist was met with the mask presented by the native to the Euro-American administrators, traders, and missionaries. By entering and sharing in native life, the anthropologist hoped to build a different kind of relationship, to get a fuller and more human kind of knowledge.

But, we should not read the same desire for parity and participation into Malinowski's labors, especially at their onset, nor presume that, because he came to engage in intensive personal research, while living in a Trobriand village, he thereby too pursued social equality with the natives.¹¹ Throughout his life Malinowski referred to the Trobrianders as "savages" and "Stone Age Men." Unlike his fellow colonists, Malinowski did view the Trobrianders as men as complete as the Europeans and equally endowed with reason and morals. Indeed, in his monographs, Malinowski is so concerned to make this point that he sometimes sacrifices accuracy, likening the natives to Europeans far more than was the case. However, Malinowski's insistence on the natives' human stature should not be misinterpreted to mean

¹¹ As one of his students has remarked (Leach 1965:viii):

In 1914, ethnographers, Malinowski included, still viewed their subject matter with considerable contempt. They studied "the manners and customs of primitive savages" and "primitive" meant not only simple and childlike but primeval. Primitive peoples were thought of as zoological specimens; the behavior of a New Guinea native was expected to throw some light upon our own Stone Age ancestors but he was not regarded as interesting as a human being in his own right. The standard methods of ethnographic research were such that the social superiority of the investigator was being constantly emphasized. The "native" was a specimen to be measured and photographed and interviewed—through an interpreter.

that he was indifferent to the criteria of civilization. Sometimes, Malinowski (1935/1965:211-212) speaks of Trobriander, Briton, and Pole as being equally savage:

Cricket, which to an Englishman has become a synonym for honour and sportsman-like behaviour, is to a Kiriwian [Trobriander], a cause for violent quarreling and strong passion, as well as a newly invented system of gambling; while to another type of savage, a Pole, it remains pointless—a tedious manner of time-wasting.

Or, (1922/1961:21):

Again, in this type of work, it is good for the Ethnographer sometimes to put aside camera, note book and pencil, and to join himself in what is going on. He can take part in the natives' games, he can follow them on their visits and walks, sit down and listen and share in their conversations. I am not certain if this is equally easy for everyone—perhaps the Slavonic nature is more plastic and more naturally savage than that of Western Europeans—but though the degree of success varies, the attempt is possible for everyone.

Malinowski could consider himself, the British colonists, and the Trobriand natives as equally human in the anthropological abstract, but in the concreteness of daily interaction, he considered himself a member of an intellectual and esthetic elite, superior to both. When he speaks of himself as a Slavonic savage, he speaks in mock-deprecation, and it is with this understanding in mind that we must confront the statements in the diaries (e.g. Malinowski 1967:372—May 11, 1918) so disconcerting to anthropological reviewers:

At 10 I went to Teyava, where I took pictures of a house, a group of girls, and the *wasi* [exchange of food], and studied construction of a new house. On this occasion I made one or two coarse jokes, and one *bloody nigger* made a disapproving remark, whereupon I cursed them and was highly irritated. I managed to control myself *on the spot*, but I was terribly vexed by the fact that this *nigger* had dared to speak to me in such a manner [italics signify originals in English; other text in Polish].

What I would like to suggest about the foregoing passage and others like it is that anthropological readers have been interpreting its meaning within the context of their

own time and place rather than Malinowski's.¹²

What induced Malinowski to settle in Omakana? Perhaps the very fact that the society of the Trobriand archipelago was stratified. As a descendant of the gentry and a member of an intellectual elite, he may well have found this untypical hierarchical Melanesian society congenial. It is also possible that the Paramount Chief and his peers welcomed this strange Polish scholar as an ally in their struggle with the British resident magistrate and the colonial system. Here was a European who appreciated their esoteric lore, respected their status, and was eager to be instructed. Paramount Chief and anthropologist encountered each other as intelligent fellows, both devoted to specialized intellectual skills, and both having difficulties adapting to the self-righteous and autocratic rule of the British colonists.

Then, too, given "the magical orientation" of the Melanesians, Malinowski might have seemed the bearer of great mystical power and lore. Anthropologists since Malinowski (e.g. Lawrence 1964) have discovered the Melanesian belief that the secret of "the Cargo" lay in the Christian Scriptures. Some Melanesians hoped and expected that the missionaries would make good their assertions of kinship and transmit to them in the nature of these magical secrets.¹³ It is not too much to hazard that the Paramount Chief and his peers, each of whom possessed important kinds of socially necessary magic, might have hoped that Malinowski in turn had a magical power to assist them with the British. Such an expectation would ex-

¹² A major exception among anthropological readers is Stocking who has the insight (1968:190-191) to compare the situation of Malinowski to that of Kurtz in Joseph Conrad's *Heart of Darkness* explaining that while "Malinowski was far from being Kurtz . . . like Kurtz, he was alone with his instincts in the heart of darkness."

¹³ ". . . the natives' interpretation of the Scriptures was no mere academic explanation of the origin of cargo. It had its practical aspects. It was assumed that the friendliness of the missionaries was evidence that, whatever other Europeans might feel and do, they at least remembered and would honour their ties of brotherhood with the natives as common descendants of Noah. They would reveal the ritual secrets which would en-

plain his ability to elicit from them vast quantities of magical texts which otherwise might have been withheld. By the very principle of reciprocity Malinowski himself was to emphasize (1926) as vital to the normal operations of native institutions, he would have been expected to reciprocate with lore in manipulating the European system.¹⁴

However, the unhappy truth in Malinowski's diaries is that he neither participated in the ordinary run of the native life nor was able to reciprocate native trust and honor by helping them deal with the British administrators, traders, and missionaries. To these latter, Malinowski was an uninfluential enemy alien. He did speak up in defense of the natives, and later in life he was to wage a literary campaign against the more brutal of colonial practices, but at this time he lacked the stature requisite for making the British take seriously his criticisms of their policies. Again, Malinowski seems scarcely to have sought participation in Trobriand life, and in any case he was a sickly man and hence unfit to engage in their strenuous routines.

In his "Preface" to the *Argonauts*, Sir James Frazer says (1922/1961:vii) "Malinowski lived as a native among the natives

for many months together," a description of Malinowski's fieldwork repeated by others (e.g. Kardiner and Preble 1961: 163, 171-2). As nearly as I can find, Malinowski never described his fieldwork in these words. Any serious reading of Malinowski's Trobriand monographs would reveal that only by marrying a Trobriand woman, entering the web of kinship obligations and reciprocities, and engaging with the other men of the village in intensive agriculture would he have lived as a Trobriand. Instead, Malinowski established himself in the heart of native villages in the style of a petty lord attended by a large retinue of personal servants. Compared to Britons elsewhere, he had few funds, but in the Trobriand archipelago the monies he did control went a long way.¹⁵ So situated, housed, and served, he was able to observe and gossip with the natives about village events, though he scarcely participated in them. From his diaries, it is evident that the greater part of his researches involved quiz-

sure that God sent the natives supplies of cargo."

The foregoing is the essence of what Lawrence (1964:76-77) denominates as the "Third Cargo Belief" (1914-33, Southern Madang District of New Guinea). Conceivably, Malinowski encountered natives who held this belief during the course of his fieldwork.

¹⁴ It is entirely possible that the Paramount Chief finally came to the judgment that Malinowski's powers were negative, as evidenced by the chief's blaming Malinowski for the failure of the *kula* in which Malinowski attempted to participate. (cf. 1922/1961:479, 1967:153). If so, this may account for the fact that on his second trip Malinowski did not return to Omarkana but worked elsewhere in the Trobriand archipelago. Certainly, much of importance is implicit in Malinowski's diary account (1967:153) of the meeting with the chief, at the start of his third expedition (December, 1917), after his absence from the Trobriands for more than a year and a half: "To'uluwa came. We greet each other as friends. He spoke about me and praised me. Despite everything there is a certain residue of sympathy. He stood over me with a half-ironical, half-indulgent smile, telling about my exploits. Joke about our *kula*."

¹⁵ "The male term for old age, *tomwaya*, can also denote rank or importance. I myself was often so addressed, but I was not flattered, and much preferred to be called *toboma*, literally 'the tabooed man,' a name given to old men of rank, but stressing the latter attribute rather than the former" (Malinowski 1929/1969:61). Malinowski was only in his early thirties, but as he was unmarried and presumably unmarriageable, yet a person of mature status, it would have been natural for him to be placed in the category of those past marriage and child-rearing.

As for his finances, he received about £250 per year for the six years which included his field-trips: "I defrayed out of this, not only all the expenses of travel and research, such as fares, wages to native servants, payments of interpreters, but I was also able to collect a fair amount of ethnographic specimens" (Malinowski 1922: Acknowledgments). Malinowski mentions (1935/1965:I, 41) that he "used to supply the [Paramount] chief with about half a stick of tobacco a day, and usually with a small bunch of betel nut." This would evidently have been the price, or "tax," that Malinowski paid for his presence in the village including presumably the location of his tent and other amenities. The value which the Trobrianders placed on tobacco in ceremonial presentations is shown elsewhere in his work (e.g. 1929/1969:94). In any case, judging from the number of personal servants or "boys" to whom Malinowski refers in his diaries and elsewhere (e.g. Ginger, Ogisa, Tomakapu, and others, cf. 1967:168), he lived like a petty lord or "sahib."

ing strategic informants; that is, his field technique closely resembled earlier ethnographic inquiry, with the important difference that Malinowski's inquiry took place in the heart of the native habitat and could be tested. In villages like Omarkana, this technique worked very well. Malinowski apparently established relationships with the area's aristocratic and priestly leaders which yielded reams of texts of native myth and ritual. Elsewhere—as the diaries make painfully evident—his key informants thwarted him. He complains (1967:234, 236) irritably that the natives are lying to him. In such a situation, a modern participant-observer fieldworker would try to discover the meaning of the natives' attempt to deceive him. Indeed, he might even inquire why earlier they had been more truthful. But Malinowski's naive image of his status among the Trobrianders precludes such inquiry. His response is to be angry with the natives for lying to him.

Similarly, a modern participant-observer ethnographer hopes to establish parity with native people in economic matters. This is never easy since most native peoples seem poor in relation to ethnographers, though in many instances the natives possess the food and shelter on which the ethnographer depends. The diaries indicate that in this respect also Malinowski had his troubles, for he lived like a petty European lord. On some occasions (e.g. 1967:216, 239), he worries about the theft of personal belongings; on others, he complains because the natives help themselves to his stores of supplies. He speaks (1967:188) of "being irritated by the impudence of Navavile and the Wawela fellows [they had eaten too much betel-nut]" while he was absent from the tent. Presumably he paid the natives in betel-nut for their work. To help themselves to his supply was impudence from the European perspective. Yet to withhold his supply was for the natives failure to acknowledge native norms of generosity. The native view is evident in Malinowski's comment (1935/1965: 40-41) on the Paramount Chief: were his stores of tobacco or betel-nut "exposed to public gaze, he would, on the principles of

noblesse oblige, have to distribute them among the surrounding people." The diary passage is particularly striking in that Navavile was among those consuming the betel-nut. Navavile was the local specialist in garden magic rituals and the equivalent of an aristocrat and leader, hence, a vital informant to Malinowski, one whose favor an anthropologist would ordinarily take pains to cultivate, whatever the cost in betel-nut. Thus, the passage exposes Malinowski's European biases and reveals how little he shared the perspective of a Trobriand aristocrat.

Malinowski's Achievement in Fieldwork

The significance of the foregoing analysis is more than historical. It suggests the complexities of a relationship now taken for granted by anthropologists in modern, participant-observer fieldwork. Malinowski himself did not practice this kind of fieldwork, but he moved a great distance toward it. He discovered the incomparable scientific value of intensive personal field research, in which the researcher learns the native language and lives for many months among a single people. He was perhaps fortunate in that the people he settled with were willing to tolerate his presence as European sahib and to confide in him all manner of ritually important lore. But he merited some luck, for he was diligent, linguistically apt, and deeply dedicated to his scholarly task.

Yet to assess his achievement we must consider, not merely his accomplishment in the Trobriands, but his influence on his students and other anthropological readers. His emphases in his monographs draw readers toward the notion of participant-observer fieldwork. Again and again, he describes the Trobriand natives, and by implication all natives, as rational moral human beings in whose society a civilized man would be content to live. Thus the monographs generate the idea of parity between European and native, ethnographer and native. Moreover, the monographs imply that Malinowski was personally involved with native life. So much is this the case that, at the publica-

tion of the diaries, some anthropological readers felt deceived and betrayed. Whether or not they ought to have felt so is beside the point. If Malinowski in the field relied more heavily on questioning than participant-observation, his books emphasize the dubiousness of the former procedure compared to the latter.¹⁶

Malinowski (1922/1961:7-8) seems to have been far more an observer than a participant in Trobriand life and one of the most obtrusive type, who had to be tolerated because he was white and wealthy:

... as they knew that I would thrust my nose into everything, even where a well mannered native would not dream of intruding, they finished by regarding me as part and parcel of their life, a necessary evil or nuisance, mitigated by donations of tobacco.

Were we to divide fieldworker roles into interrogator, observer, and participant, then Malinowski far more often played the first two than the last. At one point only late in the diaries (1967:280-281; May 24, 1918), does he remark: "To encourage them to play . . . I began to *kasaysuya* [a round dance and game] myself. I needed exercise, moreover I could learn more by taking part personally."

If the account I have presented of Malinowski's fieldwork experiences is correct, it clarifies the particular nature of his achievement as a cultural anthropologist. The abundant materials collected through his fieldwork enabled him to describe many important features of Trobriand culture. But the social distance remaining between Malinowski and the Trobrianders led to his profound misreading of such important features

of Trobriand life as their system of magic and religion. Despite the quantities of materials he collected on their ritual practices, he portrayed them as Europeans in dusky skins. Similar criticisms have been leveled at other of his theoretical interpretations of Trobriand culture (cf. Geertz 1967; and R. Firth ed. 1957). The import of this critical assessment is that Malinowski acquired a poor understanding of the Trobrianders. That this should then be the message of his diaries should reassure rather than confound his anthropological readers. Much valuable information and analysis can be conducted in the absence of deep understanding. Most important, Malinowski's own experiences led to the discovery of intensive participant-observer fieldwork.

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¹⁶ Perhaps most striking is a lengthy and amusing passage in *The Sexual Life of Savages* (1929/1969:504-510) where Malinowski compares the realities of European sexual practices with what a man from Mars might elicit from a European informant about them, and then transfers the analogy to the Trobriand situation. In this playful contrast, Malinowski is not counterpointing participation but emphasizing that "it is necessary to become acquainted directly with the behaviour of the native; and this can be done only through a knowledge of the language and through a prolonged residence among the people."

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THE SPHERE OF INFLUENCE *

JOEL H. LEVINE

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University of Michigan, Dartmouth College

This is a study of network representation: The data represent a set of interlocked directorates—specifically, the network in which the boards of major banks are interlocked with the boards of major industrials. The problem is to represent this network so as to organize it and describe its major outlines. Using an unfolding variant of smallest space analysis, the problem is solved by a spherical map: the "Sphere of Influence." The sectors of the sphere represent similarly-linked corporations, and the relations among the sectors represent the relations among bank-industrial communities.

THIS is a study of a network of interlocked directorates—specifically, a study of the network in which we find the boards of major banks, e.g., Chase Manhattan or Morgan Guarantee Trust interlocked with the boards of major industrials such as General Motors or Ford. As a sociologist I approach this network as a problem in sociometry. My problem is to "understand" a large network in the crude, almost a-theoretical sense of being able to represent it, to discern its major outlines, and to distinguish important links from those which are not.

This paper will explore techniques to facilitate this understanding. And it will describe two results, one anticipated and one wholly unanticipated. The anticipated result is a "map" that represents proximities in the network. The unanticipated result is the *configuration* revealed in the map, which—partly for whimsy but mainly because of the data—I will call the "Sphere of Influence."

* This work has been greatly facilitated by public documents of the House Subcommittee on Domestic Finance; I am indebted to Representative Wright Patman and his staff. I am indebted to James C. Lingoes for considerable aid during the preliminary work of this analysis, to Roger Shepard for seminars that introduced me to the concepts of smallest space analysis, and to Stephen Waite who has also given generously of his time. I am indebted to colleagues of the FASR-structural studies seminar at Harvard, and—for causing me to attempt this analysis—I would like to thank Leslie Howard and his students, Phillip Bonacich (Bonacich, 1971a, 1971b), Stephen Berkowitz (Berkowitz, 1971), James Haney, Edward O. Lauermann, William D. Phelan, Jr., William Taylor, and Harrison C. White (Lorrain and White, 1971).

This particular network (a subset of data published by Rep. Patman's House Subcommittee on Banking, 1966) is a web of 84 nodes and 150 links. Its size makes it important methodologically; moreover its content makes it a network of putatively immense social importance. These twin forces, the size and the importance of the network, create problems.

First, the size creates problems that are present, but readily solved for smaller networks. In particular, how does one represent a network? A simple network: A is the "friend of" B, B "friend of" C, C . . . D, D . . . E, can be represented as a single straight line from A to B to C . . . to E. This representation is so natural that we barely note it as an object in its own right, distinct from both the data and from the analysis. Yet the representation organizes the data, it enables one to "see" relations, it codes the pattern in a way that can be remembered and thought about, and it suggests attributes such as centrality and distance. All these properties are beyond the data but below the level of formal hypotheses: The value (or deceptiveness) of a representation lies in what it suggests, in its ability to stimulate thought.

For a complex network, one needs the same advantages, but for a complex network this particular kind of line-drawing representation degenerates. For example, suppose I begin to represent my network by drawing the largest industrial (by sales), G.M., as a point and then drawing lines to the banks it connects. Among the fourteen banks used here (the sampling criterion is de-

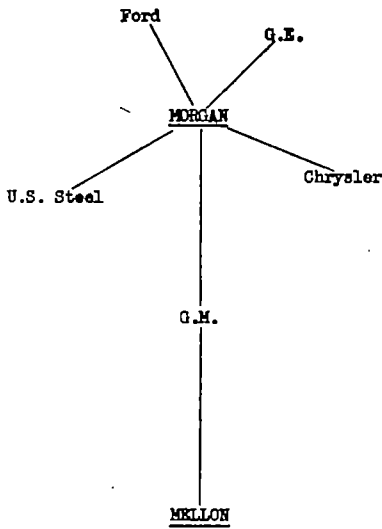


FIGURE 1

Seven Points of the G.M. Network

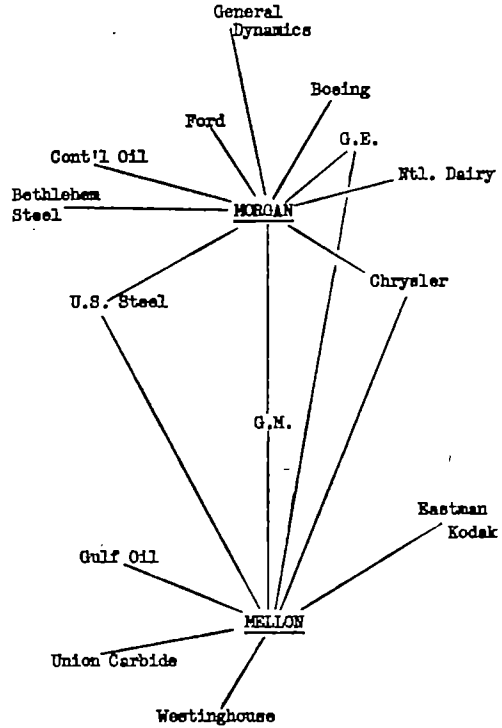


FIGURE 2

Sixteen Point Expansion of Figure 1

scribed below), G.M. reaches Morgan Guarantee Trust (three links) and Mellon National Bank (one link). Now, Morgan itself connects to U. S. Steel, Ford, G.E., and Chrysler, Figure 1, which is still a tractable picture (seven points). But Morgan is also linked to sixteen more corporations in this sample and Mellon to fourteen, Figure 2, some of which are the same as those for Morgan. This brings in thirty-two points and begins to be an unmanageable thicket of information. Yet it is only a fraction of the full network. Should I continue, I might succeed in making a spectacular picture, but probably not in making one that is useful. What I need is a representation that helps to order and clarify the large network, as a line-drawing representation helps with a small one. The representation should prepare me for theory and higher matters, but the immediate problem is to make this thicket minimally intelligible. The larger the network, the more difficult the problem and, for that very reason, the greater the need.

The representation problem is introduced

by the size of the network. The importance of this particular network makes the problem more intense. Subjectively important data, data that are ideologically potent, demand the utmost objectivity. Their representation (much less their analysis) must be derived by clearly reproducible methods. Too much is "known" about these nodes, new information is broadcast each week in publications from *Barrons* to *Ramparts* and in scholarly journals as well. This familiarity, combined with the complexity, creates the temptation to simplify by using hunches: X is important, let us record its connections. Or, X is powerful, let us document its dominion. Without commenting on the politics of such methods, they are helpful because they help one parse a mass of data. Faced with the knowledge that you are unable to handle the whole, prior "knowledge" helps one select. But such procedures are scientifically treacherous. Such hunches provide no overview, no knowledge of the context within which the "selected" facts exist. One must ask: Would I have reached the same

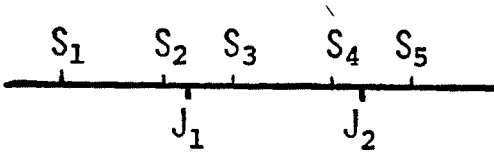


FIGURE 3. ONE-DIMENSIONAL JOINT SPACE ("TRUE SCALE" ORDER: 1, 2, 3, 4, 5)

overview had I not "known" the participants' names?

Multidimensional Scaling of Networks

This paper addresses both the representation problem and the problem of objectivity by the use of nonmetric multidimensional scaling. In particular I am using a nonmetric unfolding analysis (see Shepherd 1962, Coombs 1964) implemented for smallest space analysis by Guttman and Lingoes (1970).¹ What is a "nonmetric unfolding analysis" and how does it apply to networks? In the classical application by psychologists, the technique is applied to a set of "judges" and a set of "stimuli." Let us assume some attribute, e.g., political liberalism/conservatism on which both the judges and the stimuli are located as in Figure 3. The real scale defines a "joint space" in which both judges and stimuli are scored. The purpose of the unfolding analysis is to recover this joint space from the judges' preference orderings. Judge 1 would prefer

¹ Sociological use of similar smallest-space technique will be found in Laumann and Guttman (1966) and Laumann (1969), although these applications do not use disjoint sets of points, as the banks are disjoint from the industrials.

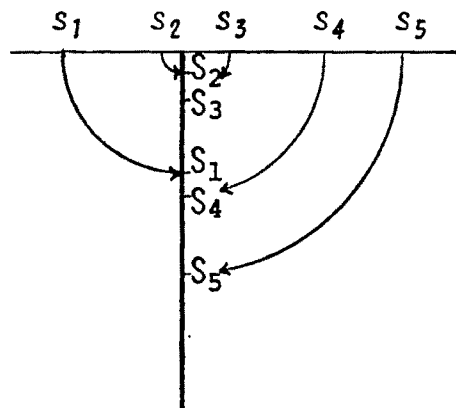


FIG. 4A. Folded Scale of Preferences Reported by Judge 1 (Order: 2, 3, 1, 4, 5)

S₂ over S₃, S₁, S₄, S₅—in order of their proximity to his own position, Figure 4a. Judge 2 would prefer S₄, S₅, S₃, S₂, S₁, Figure 4b. In actual use, of course, the joint space is unknown, except as it is found by "unfolding" the preferences.

We transfer the unfolding paradigm to networks by identifying banks as "judges" and industrial corporations as "stimuli." And we interpret number of interlocked directors as "preferences": Mellon National Bank being closer to Gulf Oil (four links) than it is to ALCOA (three links), or Westinghouse (two links), or G.M. (one link), or Standard Oil of New Jersey (zero links). (We could just as well identify industrial corporations as judges and banks as stimuli. As long as the identification is consistent, it makes no formal difference). What we ask of the unfolding analysis is that it assemble the "preferences" of many different banks into one joint space, describing the whole network in one picture.

Overview of the Data

For many years Representative Wright Patman's committees have been assembling an impressive collection of data on U. S. industrial and financial organization. (See brief selections in Zeitlin, 1970.) One volume, *Commercial Banks and Their Trust Activities* (1968), records several kinds of links (director interlocks, fund management, and stockholding) between major U. S. industrial corporations and a selection of forty-nine banks. The network analyzed

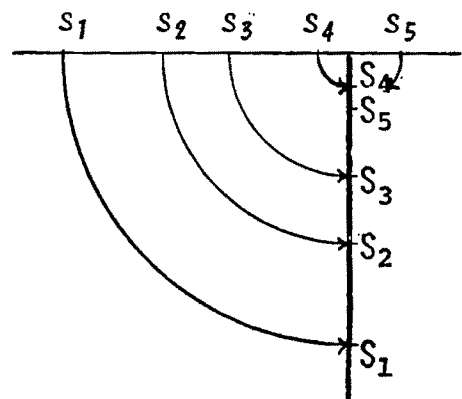


FIG. 4B. Folded Scale of Preferences Reported by Judge 2 (Order: 4, 5, 3, 2, 1)

FIGURE 4. TWO FOLDED SCALES DERIVED FROM THE SAME JOINT SPACE.

in this paper is a subset of Patman's data: using seventy industrials, fourteen banks and one kind of link, shared directors. I chose a subset in order to simplify the problem: If the method were tested on too complex a problem, one could not evaluate the result. Thus, I restricted the sample to three cities: New York, Pittsburgh, and Chicago.² At a minimum, the joint space should recover this regionalism (see Warner, Unwalla, and Trimm, circa p. 140).

The seventy industrials and fourteen banks are a completely connected network, Figure 5; (that is, each corporation leads directly or indirectly to all the others). The network is not only connected (which was guaranteed by the selection procedure) but tightly connected (which was *not* guaranteed), making for a redundancy of direct and indirect ties. For example, one can gain an upper bound on a Milgram-style small world analysis (Milgram, 1967) by noting the number of steps from bank to bank: For eight banks, (1, 2, 3, 4, 5, 6, 9, 10) each is connected to any other through exactly one intermediary, e.g., *Chase-G.E.-Morgan, Chase-International Paper-Bankers Trust, Chase-S.O.N.J.-First Natl. City*. The next shortest path detectable in these data (bank to bank) is bank-industrial-bank-industrial-bank. All the remaining bank-bank links are of this length. As we shall see, this kind

of network redundancy is methodologically useful. It makes representation for complex networks in some ways easier than for simple ones.

What I require is a joint space for these eighty-four banks and industrials. All network similarities among banks will be inferred *indirectly* from their links to the industrials; and all network similarities among industrials will be inferred *indirectly* from their links to the banks.

First Result: A Map of the Network

Figure 6 is the three-dimensional joint space (two dimensions are shown, a third dimension separates the banks from the industrials).³ First, note that this three-dimensional representation does satisfy the minimal requirement: It recovers the regional groupings. That is, based on the pattern of interlocks, one finds regional clusters. Region *per se* is not recorded in Figure 5—region is a variable that I withheld from my "computer" so that I would have a minimal criterion for checking the sense of my results. One finds First National Bank of Chicago at the lower right (and forward among the banks) and nearby, Northern Trust, Harris Trust, American National, and Continental Illinois. Region *per se* is only a geographical fact. The map of directorates shows that the fact is also social. Near these banks (but to the back—among the industrials) are, for example, Standard Oil of Indiana, Armour, Inland Steel, and International Harvester—a Chicago cluster as a *network* (not a geographical) fact. Between the center and the right of the three-dimensional picture, there is a band of Pittsburgh banks and industrials. Finally, occupying half the space at the left and bottom, we find the New York banks.⁴

So we have a reasonable picture. The technique was bound to give us some "picture," i.e., something had to come out of the computer. The regionalism gives me some assur-

² These three cities were chosen according to the size of their largest banks. The largest bank (by Patman's ranking on trust assets) in the sample is in New York City. The largest non-NYC bank in the sample is in Pittsburgh. And the largest non-NYC, non-Pittsburgh bank in the sample is in Chicago. Having chosen these three cities, I have used the fourteen banks in these cities (among Patman's original forty nine). Thus I am excluding several large banks in Boston, Cleveland, Detroit, and Philadelphia while including smaller banks in the three cities—e.g., including American National Bank and Trust (trust assets, 1967, 603 million) because it is in Chicago, but excluding Old Colony Trust of Boston (assets 4.2 billion dollars). (Patman's sample itself excludes important banks, e.g., U.S. Trust, and is limited to Eastern banks (Chicago-East) excluding South, Southwestern, and Western banks.)

Having chosen the fourteen banks, I selected industrials both interlocked with one or more of the banks and included in *Fortune's* top one hundred. Thus I am reduced to fourteen banks and seventy industrials. In addition, three industrials have not been mapped due to irremediable errors in the data processing.

³ See the discussion of dimensionality below and the discussion of the "picture" obtained in higher dimensions.

⁴ My emphasis here is methodological. But substantively related work can be found in the social ecology (and social cartography) work in Duncan (1960), Duncan and Lieberman (1970) and Lieberman (1971).

	MORGAN GUARANTY TRUST, NYC	CHASE MANHATTAN, NYC	BANKERS TRUST, NYC	FIRST NATIONAL CITY, NYC	MELLON NT'L. B&T, PITT.	MANUFACTURERS HANOVER, NYC	FIRST NATIONAL BANK, CHIC.	CONTINENTAL ILLINOIS, CHIC.	CHEMICAL BANK, NYC	NORTHERN TRUST CO., CHIC.	HARRIS TRUST & SAVINGS, CHIC.	PITT. NT'L BANK, PITT.	UNION NT'L BANK, PITT.	AMERICAN NT'L B&T, CHIC.
1. General Motors	3	•	•	•	1	•	•	•	•	•	•	•	•	•
2. Ford	2	•	•	1	•	•	•	•	•	•	•	•	•	•
3. S.O.N.J.	2	•	•	1	•	•	•	•	•	•	•	•	•	•
4. G.E.	3	1	•	1	1	•	•	•	•	1	•	•	•	•
5. Chrysler	1	1	•	1	4	•	1	•	•	•	•	•	•	•
6. Mobil Oil	•	•	1	•	2	•	•	•	•	•	•	•	•	•
7. Texaco	•	•	•	•	•	•	•	2	•	•	•	•	1	•
8. U.S.Steel	2	•	•	1	1	•	•	2	•	•	•	•	•	•
9. I.B.M.	•	2	•	1	•	•	•	•	•	•	•	•	•	•
10. Gulf Oil	•	•	•	4	•	•	•	•	•	•	•	•	•	•
11. Western Elec.	•	•	2	•	1	•	•	1	•	•	1	•	•	•
12. DuPont, E.I. DeNemours	•	•	•	•	•	•	•	1	•	•	•	•	•	•
13. Swift Co.	•	•	•	•	•	•	1	2	•	1	1	•	•	•
14. Shell Oil	•	•	•	•	•	•	1	•	•	•	•	•	•	•
15. Std. Oil, Indiana	1	•	•	•	•	•	2	1	•	1	•	•	1	•
17. Beth. Steel	•	•	•	•	•	•	•	•	•	•	•	•	•	•
18. Int. Harvester	2	•	•	•	•	•	2	2	•	•	•	•	•	•
19. Westinghouse	•	•	•	1	2	•	1	5	•	3	3	•	1	•
21. Goodyear T&R	•	1	•	•	•	•	•	•	1	1	1	•	•	•
23. Boeing	1	•	•	1	•	•	•	•	•	•	•	•	•	•
24. Armour	•	•	•	•	•	•	•	•	•	•	•	•	•	•
25. Nt'l. Dairy	1	2	•	•	2	•	2	•	•	•	•	•	•	2
26. Procter & Gamble	1	•	•	1	•	•	•	•	•	•	•	•	•	•
27. Union Carbide	•	1	•	1	3	•	•	•	•	1	•	•	•	•
28. I.T.&T.	1	•	•	1	•	•	•	•	•	•	•	•	•	•
32. General Dynamics	•	•	•	•	•	•	•	•	•	•	•	•	•	•
33. Cont'l. Oil	1	•	•	•	•	•	•	1	•	•	•	•	•	•
34. Eastman Kodak	2	2	•	•	•	•	•	•	•	•	•	•	•	•
36. United Aircraft	•	•	•	1	•	•	•	•	•	•	•	•	•	•
37. Monsanto Co.	1	•	•	2	•	•	•	•	•	•	•	•	•	•
	•	•	•	•	•	•	•	•	•	•	•	•	•	•

FIGURE 5 Bank-Industrial Interlocked Directorates

(The number in cell i, j is the number of directors common to the board of industrial i and bank j . The number of interlocks is in every case less than or equal to five.) Source: Commercial Banks and Their Trust Activities; Emerging Influence on the American Economy, Staff Report for the Subcommittee on Domestic Finance, Committee on Banking and Currency, July 8, 1968, U.S. Government Printing Office.

THE SPHERE OF INFLUENCE

19

Bank-Industrial Directorate Interlocks (cont'd)

38. General Foods	1 2	2	1 1 2		
39. Borden			2	1	
40. Caterpillar Tractor	1 2	2			
41. International Paper					
42. Sinclair Oil					
43. ALCOA		3			
44. Burlington Ind.	1				
45. American Can	2	2			
46. Union Oil, Calif.			1	1	
48. Continental Can	1				
49. Uniroyal			3 1		
51. Cities Service	1				
53. Grace, W.R., Co.		3 1			
54. Allied Chem.		1			
55. Anaconda	2	2			
56. Armco Steel		1			
63. Reynolds, R.J. Tobacco	1				
64. Atlantic Richfield	1				
65. Deere			1		
67. Gruman Aircraft Engrg.	1				
68. Inland Steel			2 1		1
70. National Steel			1	3	
71. Singer	1 1				
72. Corn Products			1		
75. Goodrich, B.F.	1 1	1			
77. Celanese	1 1				
78. Jones & Laughlin Steel		2		1	
79. FMC Corp.			1		
81. Wilson Co. Inc.	1				
82. Coca Cola	1				
83. American Cyanamid	1				
84. Pitt. Plate Glass		3			
85. Colgate Palmolive	1	1	1 1		
87. Honeywell				1	
88. Borg Warner		1	3		
89. American Home Prod.		2			
90. Owens Illinois		1			
91. Nat'l Cash Reg.		3 1			
93. TRW Inc.		1			
96. Reynolds Metals		1			

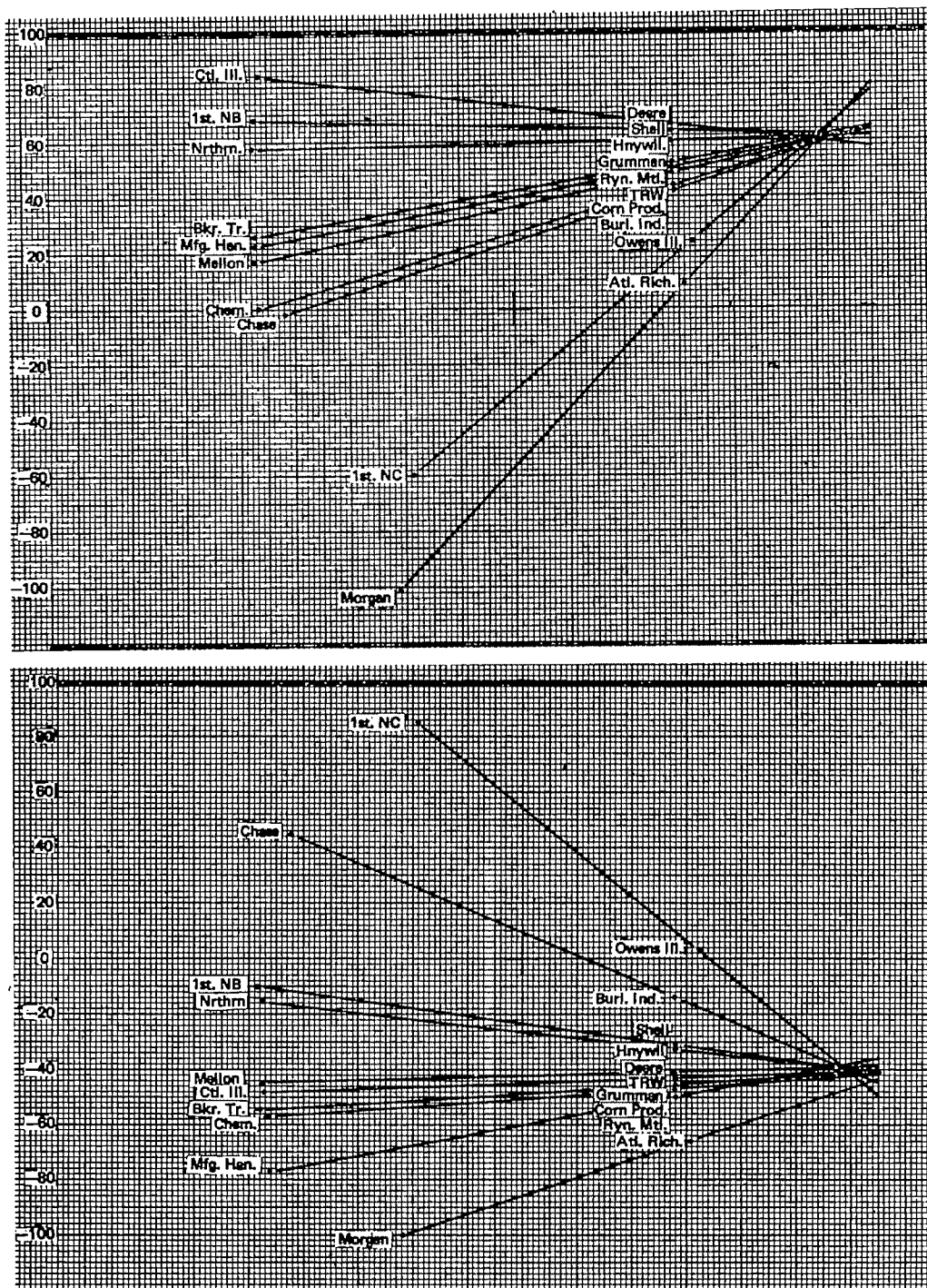


FIGURE 7. PAIRS OF BANKS AND ONCE-LINKED INDUSTRIALS: VIEWS OF DIMENSIONS 1 AND 2 AND OF DIMENSIONS 1 AND 3

to Gulf though both are linked to Chemical Bank N.Y., off to their left, and neither is linked to Pittsburgh. Note that #48, Continental Can, and #64, Atlantic Rich-

field, linked only to Morgan, appear at the right, far from Morgan. Note also that #44, Burlington Industries, appears near the center; whereas Chase is to the upper left. These

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"discrepancies" in the second and third dimensional map show that a bank's "cluster" does not have the same second and third dimensional coordinates as the bank.

But the location of these "clusters" has a pattern. In Figure 7a an arrow connects Morgan to Atlantic Richfield, showing where to "read" Morgan bank's industrial "cluster." A similar arrow connects Chase to Burlington, Mellon to TRW, etc. Each of these ten industrials is linked once to the bank at the end of the arrow and to no other bank. (There are no exclusive industrials for the other four banks.) The curious thing about the ten arrows is where they point. Figure 7 shows the first- and second- and the first- and third-dimensional views of Figure 6. The ten arrows converge to (approximately) one point: The joint space has a center; the industrials and the banks lie on concentric shells of a sphere.⁵

The Sphere of Influence

Who is at the center? No one: no bank, no industrial. The center is empty. It is where an industrial would be were it linked to no one (and such unlinked industrials were eliminated from the data). From this unlinked center one diverges in the direction of Morgan (if one is Atlantic Richfield), or in the direction of Chase (if one is Burlington Industries), or one splits the angle if he is linked to both, e.g., Singer. The separate clusters are organized around one center. Viewed spherically, there is a Morgan *sector*, a Chase *sector*, a Mellon *sector*. Radii indicate the strength of the link, angles indicate direction.

This is a strong result. Eighty-four corporations with 150 links, and 730 non-links, have obligingly arranged themselves in a sphere.⁶ And phrases like "sphere of in-

fluence" or "sector" which no one takes literally have been reified. The data are telling us what our metaphors should have suggested. Suppose bank B_1 is at the center of its "sphere of influence." For example, Morgan: Morgan is three-linked to G.M. and G.E. so that—if more links imply less distance—G.M. and G.E. are near to the center of the Morgan "sphere." Ford, U.S. Steel, Bethlehem Steel, and Continental Oil are a little "farther" away, being twice linked. Chrysler, Boeing, Natl. Dairy, Procter and Gamble, etc. are still farther being once-linked, and Standard Oil of N.J., Mobil Oil, Texaco, IBM, Gulf Oil, etc. are far away from Morgan, by being unlinked to Morgan. But these unlinked corporations do have a distance because, if not directly, then indirectly they are linked. If we draw the Morgan sphere, they are far from its center—we do not know *where* to draw them, but we know they are at some (mean) distance R from the center: They are somewhere on a sphere (or circle, in 2 space) of radius R around Morgan. Now suppose Chase Manhattan is also at the center of its sphere and that those on the periphery of its realm are far away. And suppose that "far away" is approximately R , as in the previous case. Where is the industrial that is far from *both* B_1 and B_2 ?⁷ It is limited. If it is simultaneously on *both* peripheries— R -away from B_1 *and* approximately R -away from B_2 —then, in two-space, there are at most two points where it can be located (See Figure 8, points C_1 and C_2).

Suppose an industrial is R -away from Morgan and Chase and R -away from every bank. Then, by definition, the industrial that is far away from every center is itself the center of a sphere of radius R , around which the other centers are arrayed.⁸ A sphere is

items (Figure 5) while the output is approximately 230 items (three coordinates for each of the eighty four points). Particularly for a smallest space program (which only "knows" that an input "three" is greater than an input "two"—not how much greater), this is a low ratio of input to output.

⁷ Differences in R should tend to diffuse the "center" of Figure 9.

⁸ The truly unlinked industrial (not linked directly or indirectly) may be the better meaning for the center. In this case we would have an unresolved anomaly: It would be in the center because of its symmetrical position with respect to all of the banks. But, being in the center, there would be a limit to its distance from all the other points—a

⁵ Recall the banks and industrials—the separate shells—were separated at the outset, since I used data on links *between* banks and industrials.

⁶ The smallest space computations herein were executed with the Guttman/Lingoes smallest space programs (updated June, 1970) implemented on the IBM 360 at the University of Michigan. Several runs with these data, using different starting points for the three-dimensional solution and different numbers of dimensions, have pointed to the same converging configuration. The results remain exploratory. Much more work is needed to examine their stability: The input is roughly one thousand

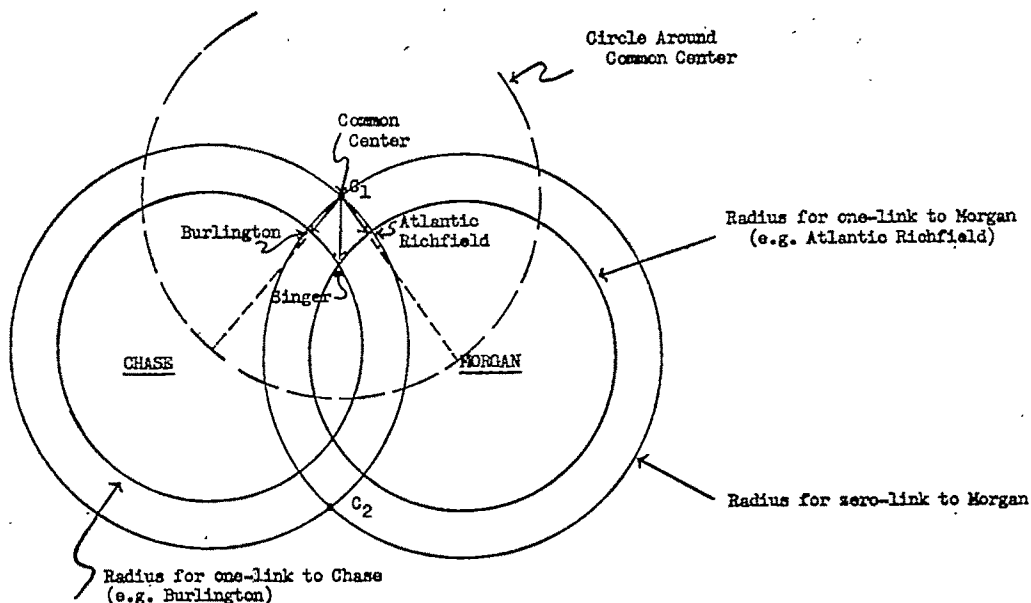


FIGURE 8. SCHEMATIC DRAWING OF THE RELATION AMONG THREE INDUSTRIALS, TWO BANKS, AND THE COMMON CENTER

the locus of points (banks) that are R-distant from one point (the simultaneously distant industrial). Considered separately, we may examine the Morgan-centered network, Chase-centered network, etc. But considered simultaneously the natural and simple view is to subsume them all into one sphere in which each group is a sector. Thus Burlington is at a distance from the common center, diverging from the common center on a radius to Chase. And because Singer is approximately as close to Chase as Burlington is, (1) each is one-linked to Chase (1) and as close to Morgan as is Atlantic Richfield, Singer is on a radius *between* the Chase and Morgan sectors. And although Singer is as close to Chase as is Burlington, Singer is farther from the common center, Figure 8. Thus, in the larger Sphere, the angle establishes the pattern of connection, i.e., the sector to which one belongs. (The length of the radius corresponds to the degree of connection; e.g., an industrial linked once to Chase and once to Morgan should appear approximately in the same sector as an industrial linked twice to each, but should be closer to the common center.⁹⁾

condition which is anomalous because it would be in no way linked to them.

⁹ Assuming that the one-distance from Chase is to the two-distance from Chase as the one-distance from Morgan is to the two-distance from Morgan.

What we have here is a primitive analogue to a Copernican change of reference. If one switches from reference to Morgan, or Chase, or some one corporation ("geo"-centric view) to a view where each is arrayed around a common center ("helio"-centric) the data do not change, but the overview changes and becomes simpler.¹⁰

Social Cartography: Gnomonic Map

The apparent discrepancies in Figure 6 were not so much true errors as errors of three-dimensional parallax, using rectangular coordinates when spherical coordinates were more appropriate. Figure 6 is the view of the configuration from infinity. It is a parallel projection of a sphere into a plane—which is a poor way of viewing a spherical object.

¹⁰ Actually there may be two spheres: One is the sphere described in the text. The second may be a sphere whose center is the bank that is linked to no industrial. In this second sphere the banks diverge toward one or more industrials. As there are few banks in this sample as weakly linked as some of the industrials, the second sphere is either absent or suppressed by the fit criterion—in favor of the first sphere. The “stress” between these two possible configurations (logically incompatible for finite-radius Euclidean spheres) should affect the result. One line that may be explored is a change to the so-called “taxicab” metric wherein a single surface *can* lie on two spheres drawn from two different centers.

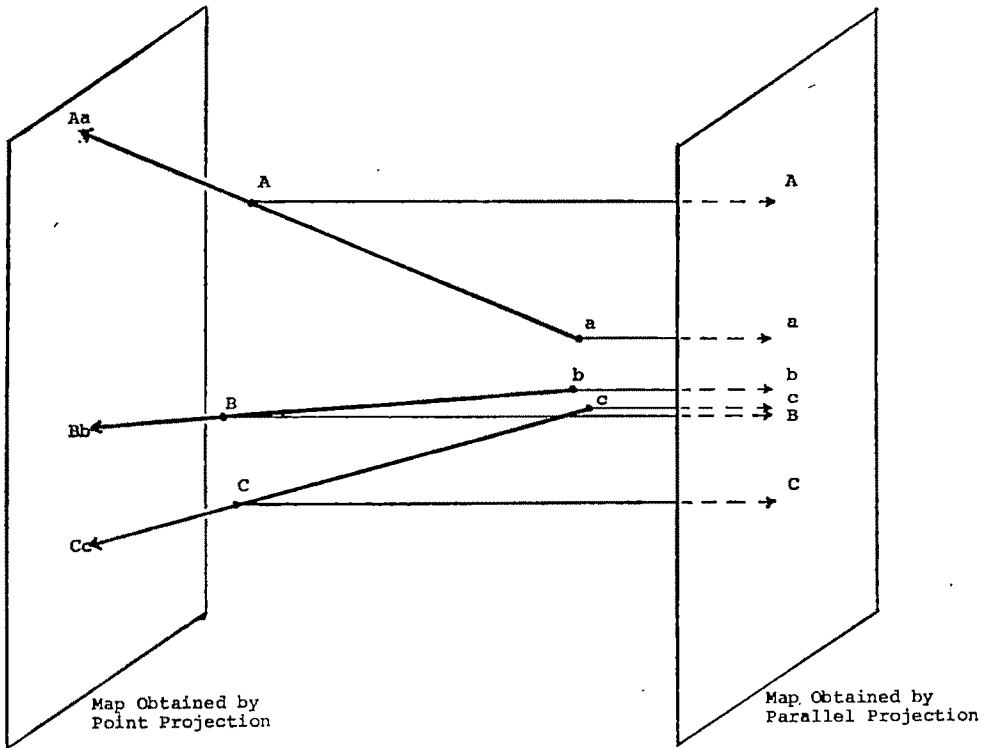


FIGURE 9. TWO ALTERNATIVE PROJECTIONS OF THE SAME SIX POINTS—ILLUSTRATING HOW THE *Apparent* CONFIGURATION IS AFFECTED BY THE PROJECTION. TO THE RIGHT IS A PARALLEL PROJECTION ONTO A PLANE (PRODUCING ONE PLANAR MAP WITH THE ORDER A, a, B, b, C, c). TO THE LEFT IS A POINT PROJECTION ONTO A PLANE (PRODUCING A PLANAR MAP THAT IS SERIOUSLY ALTERED AND REORGANIZED COMPARED TO THE PARALLEL PROJECTION).

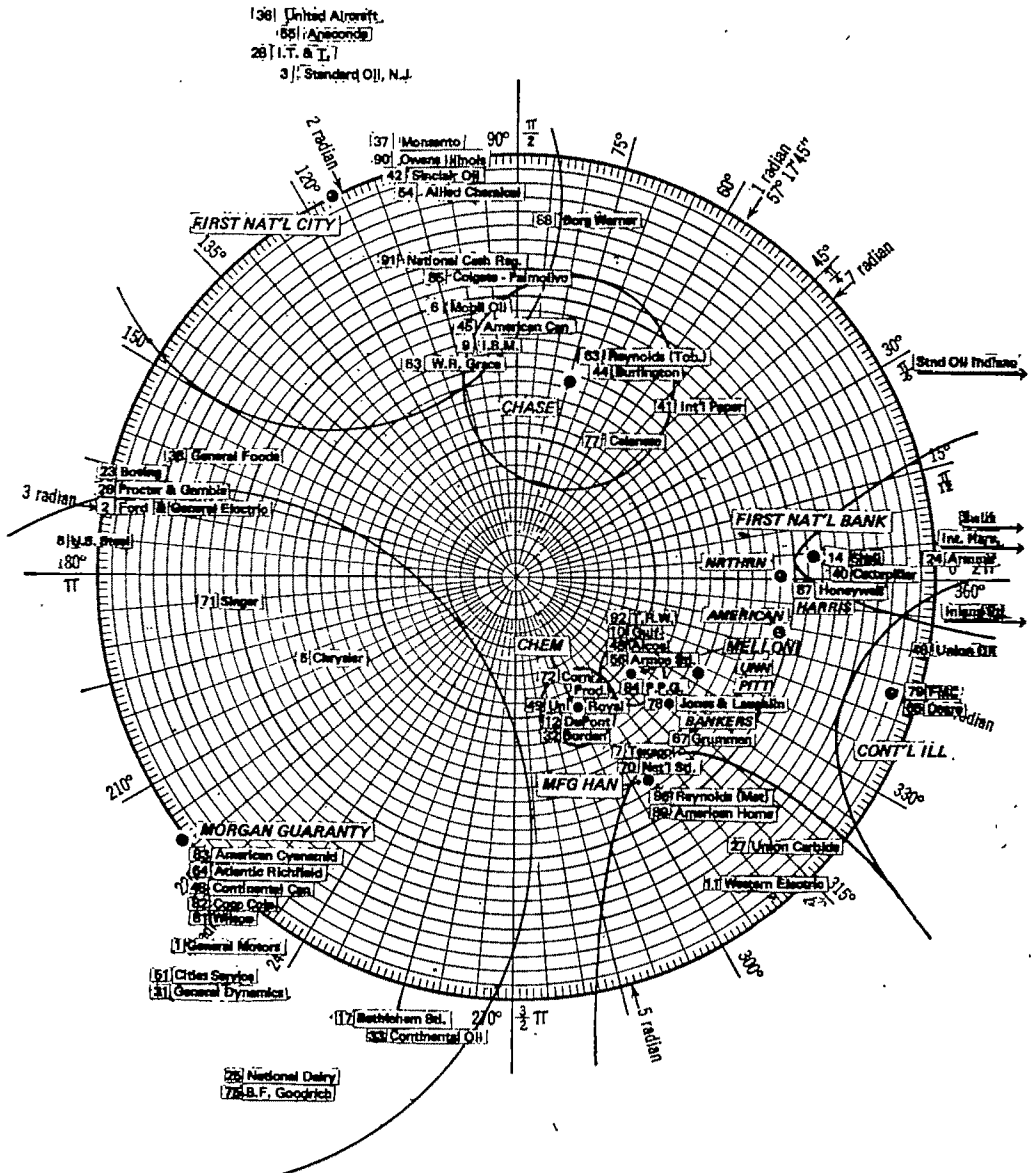
and the cause of the apparent discrepancies. The parallel projection has two flaws. First it distorts angular distance terribly. For example in Figure 9 a parallel projection onto a map separates a from A , and c from C , and puts c closer to B (much as DuPont was separated from Chemical Bank and appeared close to Mellon). Second, the parallel projection in Figure 6 is an unpredictable combination of sector effects and number of links. Thus two points in separate sectors could appear close together because both are close to the center. Or, two points in the same sector could appear far apart because their radii were different.

To the extent that it can be done, geographers are the men who can map a sphere onto a printed page. Thus let us engage in a little "social cartography," remapping Figure 6. To emphasize angular separation, we need a projection from the center point, rather than parallel projection. Thus, borrowed from the physical cartographers, Figure

10 is a "gnomonic projection" of the Sphere of Influence.¹¹

Any two-dimensional view of a truly three-dimensional object has both distortions and advantages. The gnomonic projection has the advantage that all points on the same radius are mapped into one point. Thus DuPont is properly represented near Chemical Bank. And Gulf Oil, Jones and Laughlin Steel, and Pittsburgh Plate Glass are properly in the Pittsburgh group. The Morgan sector is near Morgan, etc. Thus, by mapping each radius into a single point, it corrects the major representational flaws of Figure 6. The gnomonic projection has the second advantage that it maps great circle routes into straight lines. Thus Ford, which is linked

¹¹ The graph-paper-grid represents the point of tangency (plane to sphere) at the center of the graph. The grid of circles represents sines of the angles: point-of-tangency to center-of-sphere to point-being-projected. The circular grid only facilitates plotting *in the plane*, and does not directly represent the sphere itself.



to Morgan and First National City, appears near (but not on) the straight line between Morgan and Mellon (magnitudes of the angles are distorted). Singer is near (but not on) the straight line (great circle route) between Morgan and Chase. Thus the spherical map gives a quick approximate overview of the relations in the network.

Data Versus Representation of Data

The gnomonic map is a large sociogram whose sectors indicate relatively well-linked

bank-industrial ties. But the representation is not the *same* as the ties, and it is important to emphasize the difference between the network *data* and the two-dimensional, or three-dimensional or however-many-dimensional representation. The network itself is fourteen dimensional and no fewer number of dimensions will describe it fully; yet I have used three dimensions. The choice of the "correct" number of dimensions is—for this network analysis—a somewhat different problem from the choice of the "correct" number in a typical factor analysis. When

one uses fewer dimensions than necessary to describe the data, one always destroys some facts. Typically one chooses enough dimensions to describe the facts that are real: Eliminating some of the dimensions destroys detail, but the detail it destroys is presumed to have been the result of randomness and error. Thus, in a typical factor analysis the economy of dimensions sacrifices detail, but probably increases accuracy. But in the present analysis, six dimensions or three, or one—anything short of fourteen dimensions would suppress some of the *real* links. And suppression is exactly what I want—in order to “see the forest.” Which facts will the technique suppress? It will suppress links that are not backed up by a network of redundant linkages: The technique *must* misrepresent something, but it tries to minimize errors. Therefore, if the technique misrepresents the isolated fact, then the error cost is small. But if a bank is close to an industrial through a full network of direct and many indirect links as well, then the technique will place them close together, since not to do so would be to misreport many links. By minimizing error, the technique represents the major, the most redundant outlines, which are exactly what we set out to find. Thus Ford, which is close to Morgan (two links) and First National City (one link) is close to General Electric, General Foods, and U.S. Steel which, while linked to Morgan and First National City, are linked to other banks as well. The representation “suggests” that the Morgan, First National City pair predominates: These industrials, e.g., Ford and G.E., differ in detail; but in gross, low-dimensional outline the representation suggests they are similar in effect. Detail is suppressed—we do not want all the detail, not yet.¹² To begin the analysis, really pre-analysis of a social network, we must, as scientists, be willfully simple-minded. That is the kind of assistance which we required of a system of representation.

Conclusion

We have examined a sociogram of banks and industrials looking for a method that

provides for a large network the kind of overview that point-line sociograms provide for small ones. The method used was multi-dimensional unfolded scaling which is able to compress into two, three, or four dimensions much of the information in the original fourteen dimensional data. Before we venture—or venture with *confidence*—into more substantive analysis, we will need to continue to examine the technique itself. For example: How stable is the configuration? How much leeway should one allow; that is, how well-determined are the angles in Figure 7? I would make a rough allowance of 5 to 10 degrees. But 5 to 10 degrees is a guess subject to much exploration of rounding errors,¹³ of algorithms, of “initial configurations,” and even subject to exploration of alternate geometries.¹⁴

For the present, the (Guttman/Lingoes/Coombs) unfolding analysis has been applied to a set of fourteen banks whose boards or directors are interlocked with the boards of seventy large U.S. industrials. The banks and industrials have been mapped into a joint space in which distance corresponds to interlock: the industrials most strongly linked to a bank are near it in the space. While the technique attends to these details of each bank and industrial, it also creates an overall pattern de-emphasizing the neighborhood of any one node. The overall picture is a sphere: the center is empty, one shell of the sphere is filled with industrials and the outer sphere is filled with banks. In the overview, each bank or industrial occupies a “sector” of the overall network, each industrial falling into the appropriate combination of sectors. Reducing the number of dimensions enables one to visualize the major grouping while suppressing non-essential features of the network.

Further analysis will examine both lower and higher dimensions. Here, in three dimensions, Figure 10 is a roughly triangular array with “vertices” Morgan, First National City, and Continental Illinois (New York-Morgan, New York-First Natl. City/Chase, and Chicago-Continental Illinois). In four dimensions the detail is richer, with Mellon emerg-

separate directions. See comments in the text in the discussion of “proper” dimensionality.)

¹³ Some rather large matrices have had to be inverted.

¹⁴ See footnotes 8 and 10.

¹² In four dimensions the cluster begins to break-up; General Foods drifts toward Chicago and Chemical Bank, General Electric toward Mellon. (in four-dimensional detail these “drifts” are

ing as a vertex in its own right—clarifying a region which is crowded and confused in the three-dimensional figure. (Preliminary analysis shows that the two, four, and six dimensional maps are also spherical.) Further analysis will examine a larger universe of data, more cities, more banks, and more industrials. Further analysis will consider the robustness of the “picture”; i.e., how much of the shape and detail of this picture would be affected by missing data—can we reconstruct unreported interlocks? We will examine trends: these 1967 data surely differ in detail from new data for 1972. But how, if at all, would the 1972 representation differ from 1967? Do the dozens of new details outline slow-moving, progressing trends? The “sphere” is designed to complement sociometric technique and answer these questions.

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A MODIFIED MULTIPLE REGRESSION APPROACH TO THE ANALYSIS OF DICHOTOMOUS VARIABLES *

LEO A. GOODMAN

University of Chicago

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To illustrate the models and methods of the present article, we shall reanalyze those data in the famous study of The American Soldier by Stouffer et al. (1949), subsequently analyzed by Coleman (1964), Zeisel (1968), and Theil (1970). The methods we present reveal how the odds pertaining to a given dichotomized variable (e.g., the odds that a soldier would prefer a Northern to a Southern Camp assignment) are related to other dichotomized variables (e.g., (a) the soldier's race, (b) his region of origin, (c) his present camp location). The usual regression analysis methods do not suit the case considered here, where the dependent variable is the odds pertaining to a given dichotomous variable. Nor do the usual methods suit the case where the dependent variable is a proportion pertaining to the dichotomous variable. This article presents some relatively elementary models and methods suitable for analysing the odds (or a proportion) pertaining to the given dichotomous dependent variable. Applying these models and methods to the data referred to above, new insights are obtained.

LET US begin by describing the data that will be analyzed here for illustrative purposes. These data, which are based on the earlier data first presented by Stouffer et al. (1949), appear in Table 1 below. This four-way table cross-classifies soldiers by the following four dichotomous variables: (A) race (Negro or white); (B) region of origin (North or South); (C) location of present camp (North or South); and (D) preference as to camp location (North or South).

Table 1 shows that for say, a Negro Northerner in a Northern camp the odds are 387 to 36 that he will prefer a Northern camp. This table also shows that for, say, a white Southerner in a Southern camp, the odds are 91 to 869 that he will prefer a Northern camp. In the next section, we present a model that describes quantitatively how these and the other odds in Table 1 are affected by (A) race, (B) region of origin, and (C) present camp location. We show how to test whether the model fits the data, and we measure how well the model fits using an index that is analogous to the usual multiple correlation coefficient of re-

gression analysis. We also show how to assess the statistical significance of the contribution made by certain parameters in the model, and we measure the contribution's magnitude with indices that are analogous to the usual partial and multiple-partial correlation coefficients of regression analysis.

With the model that will be described in the next section, and with the more general model that follows it, we can estimate how the odds for preferring a Northern camp are changed by the "main effects" of race, region or origin, and present camp location, as well as by certain "interaction effects" among these variables. With each model considered here we can also estimate what the expected frequencies in Table 1 would be if the model were true. We can compare these estimated expected frequencies with the corresponding observed frequencies to determine if the model fits the data. For the model that will be described in the next section, Table 2 gives the expected frequencies estimated under the assumption that the model is true. Under this model, the estimated odds are 390.64 to 32.36 that the Negro Northerner in a Northern camp will prefer a Northern camp; and the estimated odds are 91.74 to 868.26 that the white Southerner in a Southern camp will prefer a Northern camp. When we compare the corresponding entries in Tables 1 and 2

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Table 1. Cross-Classification of Soldiers With Respect to Four Dichotomized Variables: (A) Race, (B) Region of Origin, (C) Location of Present Camp, and (D) Preference as to Camp Location

Variable A Race	Variable B Region of Origin	Variable C Location of Present Camp	Variable D Number of Soldiers Preferring Camp* In North In South	
Negro	North	North	387	36
Negro	North	South	876	250
Negro	South	North	383	270
Negro	South	South	381	1712
White	North	North	955	162
White	North	South	874	510
White	South	North	104	176
White	South	South	91	869

*The numbers in this table were recalculated from the percentage table in Stouffer et al. (1949, p. 553). These numbers are consistent with the percentages in the 1949 table, but they may differ somewhat from the actual observed frequencies due to rounding of the percentages. A related percentage table was given also by Coleman (1964, p. 198) and Theil (1970, p. 104). "Preference for camp in North" includes (a) those who prefer to move to a specific camp located in the North, and (b) those whose present camp is in the North and who prefer to stay there. Similarly, "preference for camp in South" includes (a) those who prefer to move to a specific camp located in the South, and (b) those whose present camp is in the South and who prefer to stay there.

(by methods that will be described later herein), we find that the model fits the data well.

Using the expected frequencies estimated under the given model (see Table 2), we can also estimate the expected *proportion* preferring a Northern camp (as well as the *odds* referred to above), for the individuals in each row in Table 2, under the assumption that the model is true. The models considered herein, which describe how the *odds* for preferring a Northern camp are changed by certain specified "main effects" and "interaction effects," can also be used to describe how the *proportion* preferring a Northern camp is changed by these effects.

In order to understand how the dependent variable (preference as to camp location) is related to the other three variables (race, region of origin, and present camp location), we began with the four-way table (Table 1). Is it necessary to use a four-way table to describe how the dependent variable is related to the other three variables, or can this relationship be summarized adequately

using the information contained in tables of smaller dimension (e.g., two-way and/or three-way tables)? The methods presented in the present article can be used to answer this question. To estimate the relationship between the dependent variable and the other three variables, the model that will be presented in the next section will actually use only the information contained in (1) the two-way table describing the relationship between the dependent variable and race, and (2) the three-way table describing the relationship between the dependent variable, region of origin and present camp location. Since that model fits the data well, we find that the relationship between the dependent variable and the other three variables can be summarized adequately using only the information contained in the particular two-way and three-way tables noted above. This topic will be discussed more fully later herein when Table 5 is presented.

We propose to analyze the data in Table 1 by methods quite different from those used in earlier analyses of these data. Our model

Table 2. Estimate of the Expected Frequencies in the Four-Way Contingency Table (Table 1), Under the Model in Which the Odds for Preferring a Northern Camp Depend on Race, Region of Origin, Location of Present Camp, and on the Interaction Between Region of Origin and Location of Present Camp

Variable A Race	Variable B Region of Origin	Variable C Location of Present Camp	Variable D Number of Soldiers Preferring Camp In North In South	
Negro	North	North	390.64	32.36
Negro	North	South	879.31	246.69
Negro	South	North	376.79	276.21
Negro	South	South	380.26	1712.74
White	North	North	951.36	165.64
White	North	South	870.69	513.31
White	South	North	110.21	169.79
White	South	South	91.74	868.26

fits the data better than Coleman's (1964) and we present a more parsimonious explanation of these data than he does. With the estimated parameters in our model, we can explain, in a more comprehensive and compact way, various interesting features of these data noted by Zeisel (1968). Some of the models considered in the present article are related to those in Theil (1970), but the methods we use are easier to apply than his. In the final section herein, we shall compare more fully ours with earlier methods.

A MODEL FOR ANALYZING THE ODDS

The symbols A, B, C, and D denote the four dichotomized variables in the four-way table (Table 1): (A) race, (B) region of origin, (C) location of present camp, and (D) preference as to camp location. For variable A, we use numbers 1 and 2 to denote Negro and white. For variables B, C, and D, we use numbers 1 and 2 to denote North and South. Each of Table 1's sixteen cells can be designated (i, j, k, l), where $i=1$ or 2; $j=1$ or 2; $k=1$ or 2; $l=1$ or 2. For example, entry 387 is in cell (1, 1, 1, 1), and is a case where variables A, B, C, D all take on value 1; entry 36 is in the cell (1, 1, 1, 2), with variable A, B, and C taking on value 1 and variable D value 2; entry 876 is in cell (1, 1, 2, 1) with variables A, B, and D taking on value 1 and variable C

value 2; entry 250 is in cell (1, 1, 2, 2) with variables A and B taking on value 1 and variables C and D value 2.

Let f_{ijkl} denote the observed frequency in cell (i, j, k, l) of Table 1. For example, $f_{1111} = 387$, $f_{1112} = 36$, $f_{1121} = 876$, $f_{1122} = 250$, etc. Note that each row of Table 1 can be described by the triplet (i, j, k). For example, the first row is (1, 1, 1); the second (1, 1, 2); etc. Let n_{ijk} denote the total observed frequency in a row (i, j, k). In other words, we can write n_{ijk} as

$$n_{ijk} = f_{ijk1} + f_{ijk2}. \quad (1)$$

For example, $n_{111} = 423$, $n_{112} = 1126$, etc.

For those in row (i, j, k) the observed odds in favor of a preference for a Northern camp (i.e., the odds that variable D will take on value 1) can be written as

$$\omega_{ijk} = f_{ijk1}/f_{ijk2}. \quad (2)$$

For example, $\omega_{111} = 10.75$, $\omega_{112} = 3.50$, etc. In other words, when variables A, B, and C all take on value 1, the odds are 10.75 to 1 that variable D will take on that value. When variables A and B take on value 1 and variable C value 2, the odds are 3.50 to 1 that variable D will take on value 1.

For row (i, j, k) in Table 1, let p_{ijk} denote that row's observed proportion of observations for which variable D takes on value 1. In other words, we can write p_{ijk} as

$$p_{ijk} = f_{ijk1}/n_{ijk}. \quad (3)$$

For example, $p_{111} = .91$, $p_{112} = .78$, etc. We also let q_{ijk} denote the observed proportion

of observations in row (i, j, k) for which variable D takes on value 2. Thus,

$$q_{ijk} = f_{ijk2}/n_{ijk} = 1 - p_{ijk}. \quad (4)$$

From (2)–(4), we see that

$$\omega_{ijk} = p_{ijk}/q_{ijk}. \quad (5)$$

We can also express the p_{ijk} and q_{ijk} in terms of the observed odds ω_{ijk} :

$$\begin{aligned} p_{ijk} &= \omega_{ijk}/(1 + \omega_{ijk}), \\ q_{ijk} &= 1/(1 + \omega_{ijk}). \end{aligned} \quad (6)$$

From (3) and (6) we see that the observed frequencies f_{ijk1} can be expressed in terms of the observed odds ω_{ijk} and the n_{ijk} :

$$\begin{aligned} f_{ijk1} &= n_{ijk}\omega_{ijk}/(1 + \omega_{ijk}) \\ f_{ijk2} &= n_{ijk}/(1 + \omega_{ijk}). \end{aligned} \quad (7)$$

Let F_{ijk1} denote the expected frequency in cell $(i, j, k, 1)$ under some specified model. For example, for the model referred to at the end of the preceding section, we see from Table 2 that F_{1111} and F_{1112} are estimated as 390.64 and 32.36, respectively. (The calculation of the entries in Table 2 will be commented upon later herein after we have presented the material in Table 5.) Letting Ω_{ijk} denote the odds based on the expected frequencies, we see that

$$\Omega_{ijk} = F_{ijk1}/F_{ijk2}. \quad (8)$$

Formula (8) corresponds to (2). In addition, corresponding to (7), we have the following:

$$\begin{aligned} F_{ijk1} &= n_{ijk}\Omega_{ijk}/(1 + \Omega_{ijk}) \\ F_{ijk2} &= n_{ijk}/(1 + \Omega_{ijk}). \end{aligned} \quad (9)$$

(For a related matter, see (44) later herein.) Thus, from the F_{ijk1} , we can calculate the “expected odds” Ω_{ijk} ; and from the Ω_{ijk} and n_{ijk} , we can calculate the F_{ijk1} .

Our models will express the Ω_{ijk} in terms of a set of parameters that describe the “main effects” of variables A , B , and C , and certain “interaction effects” among these variables, in a way that is somewhat analogous to the corresponding effects in the usual analysis of variance model. In the present section, we shall present a particular model that fits the data (Table 1) well; and in the next section, we shall present a more general model, namely, a “saturated model” for analyzing the odds, that can help determine the various “unsaturated models” that should be examined further.¹

¹ The saturated model, which we present in the next section, can also be described as a full model or an unrestricted model. The unsaturated models can also be described as restricted models. The various models we consider, which assume that the expected odds Ω_{ijk} are subject to certain multi-

Our analysis of this saturated model led us to the particular unsaturated model that we shall present now. For expository reasons we present the unsaturated model first.

Consider the following model:

$$\Omega_{ijk} = \gamma \gamma^A \gamma^B \gamma^C \gamma^{BC}_{jk} \quad (10)$$

where

$$\begin{aligned} \gamma^A_1 &= 1/\gamma^A_2, \gamma^B_1 = 1/\gamma^B_2, \gamma^C_1 = 1/\gamma^C_2, \\ \gamma^{BC}_{11} &= \gamma^{BC}_{22} = 1/\gamma^{BC}_{12} = 1/\gamma^{BC}_{21}. \end{aligned} \quad (11)$$

Parameters γ , γ^A_1 , γ^B_1 , and γ^C_1 describe the “main effects” on Ω_{ijk} of the general mean² and variables A , B , and C , respectively; and parameter γ^{BC}_{11} describes the “interaction effect” of variables B and C on Ω_{ijk} .³

Formula (10) describes the effects of the parameters on Ω_{ijk} , expressing Ω_{ijk} explicitly in terms of the model's parameters. These parameters can also be explicitly expressed in terms of Ω_{ijk} . From (10)–(11), we obtain the following expressions for the parameters in terms of Ω_{ijk} :

$$\gamma = \left[\prod_{i=1}^2 \prod_{j=1}^2 \prod_{k=1}^2 \Omega_{ijk} \right]^{1/8}, \quad (12)$$

$$\gamma^A_1 = [\Omega_{1jk}/\Omega_{2jk}]^{1/4} \quad (\text{for } j = 1, 2; k = 1, 2)$$

$$= \left[\prod_{j=1}^2 \prod_{k=1}^2 (\Omega_{1jk}/\Omega_{2jk}) \right]^{1/8}, \quad (13)$$

$$\gamma^B_1 = \left[\prod_{i=1}^2 \prod_{k=1}^2 (\Omega_{i1k}/\Omega_{i2k}) \right]^{1/8}, \quad (14)$$

$$\gamma^C_1 = \left[\prod_{i=1}^2 \prod_{j=1}^2 (\Omega_{ij1}/\Omega_{ij2}) \right]^{1/8}, \quad (15)$$

$$\gamma^{BC}_{11} = [(\Omega_{111}\Omega_{122})/(\Omega_{112}\Omega_{121})]^{1/4} \quad (\text{for } i = 1, 2)$$

$$= \left[\prod_{i=1}^2 [(\Omega_{i11}\Omega_{i22})/(\Omega_{i12}\Omega_{i21})] \right]^{1/8}. \quad (16)$$

plicative main and interaction effects (see, e.g., formulas (10) and (29)), are quite different from models of the kind appearing in, for example, Coleman (1964) and Boudon (1968). For further comment, see the final section of the present article.

² Since γ is somewhat analogous to the main effect of the general mean in the usual model for the analysis of variance (i.e., the constant term in that model), we refer to γ as the main effect of the general mean on the Ω_{ijk} . γ actually equals the geometric mean of the Ω_{ijk} corresponding to the eight possible values of (i, j, k) obtained when $i=1, 2; j=1, 2; k=1, 2$. For further details, see formula (12) below.

³ The relationship between the model described above by (10)–(11) and the usual model for the analysis of variance will be clarified when we discuss formulas (20)–(22).

From (12), we see that γ is actually the geometric mean of the eight Ω_{ijk} . From (13), we see that γ^A_1 is the square-root of the odds-ratio $\Omega_{1jk}/\Omega_{2jk}$. From (12)–(16), we see that all the γ parameters can be expressed in terms of Ω_{ijk} .⁴ Since Table 2 presents the estimated values of F_{ijk} (under model (10)), we can use these to estimate first, Ω_{ijk} (see (8)) and second, the γ parameters (see (12)–(16)). Table 3 gives the estimated values of the γ parameters.

To emphasize the fact that odds Ω_{ijk} pertain to variable D, and that the γ parameters describe the main and interaction effects on these odds, we could replace the symbols Ω_{ijk} , γ , γ^A_1 , γ^B_j , γ^C_k , γ^{BO}_{jk} in (10)–(16) by Ω^D_{ijk} , γ^D , γ^{AD}_1 , γ^{BD}_j , γ^{CD}_k , γ^{BOD}_{jk} , respectively. This notation was used in Table 3 and later in Table 4, where each of the above parameters is identified by its superscript. From Table 3, we see that the estimated main effect of each variable (A, B, C) is positive (i.e., the estimates of γ^{AD}_1 , γ^{BD}_1 , γ^{CD}_1 are all larger than 1); but the estimated interaction effect between variables B and C is negative (i.e., the estimate of γ^{BOD}_{11} is less than 1). This means, among other things, that the estimated effect on Ω^D_{ijk} of being a Northerner in a Northern camp is less positive (due to the multiplicative factor of 0.86 pertaining to γ^{BOD}_{11}) than might be surmised simply by combining the main effect of being a Northerner with the main effect of his being in a Northern camp. More precisely, after taking account of the model's various main effects, we must multiply the estimate of Ω^D_{ijk} by the factor 0.86 for a Northerner located in a Northern camp, to account for the interaction effect γ^{BOD}_{11} between variables B and C (i.e., the effect on Ω^D_{ijk} of the interaction between region of origin and present camp location).⁵ By applying the numeri-

Table 3. Estimate of the Main Effects and Interaction Effects of the Three Variables (A, B, C) on the Odds Ω_{ijk} Pertaining to Variable D in the Four-Way Contingency Table (Table 1), Under Models (10) and (20)

Variable	γ Effects in Model (10)	β Effects in Model (20)
D	1.31	.27
AD	1.45	.37
BD	3.45	1.24
CD	2.14	.76
BCD	0.86	-.15

cal values of Table 3 to formula (10), we see, for example, that

$$\Omega^D_{111} = (1.31)(1.45)(3.45)(2.14)(0.86) = 12.07. \quad (17a)$$

(All calculations in this paper were carried out to more significant digits than are reported here.)

Further insight into the meaning of the γ parameters can be gained by noting how the estimated value of these parameters affect the estimate of Ω^D_{ijk} , for $i = 1, 2$; $j = 1, 2$; $k = 1, 2$. By applying the numerical values of Table 3 to formulas (10)–(11), we find that the Ω^D_{ijk} can be estimated by (17a) and as follows:

$$\Omega^D_{211} = (1.31) \left(\frac{1}{1.45} \right) (3.45) (2.14) (0.86) = 5.74, \quad (17b)$$

$$\Omega^D_{121} = (1.31)(1.45) \left(\frac{1}{3.45} \right) (2.14) \left(\frac{1}{0.86} \right) = 1.36, \quad (17c)$$

$$\Omega^D_{112} = (1.31)(1.45)(3.45) \left(\frac{1}{2.14} \right) \left(\frac{1}{0.86} \right) = 3.56, \quad (17d)$$

et cetera. Comparing (17a) with (17b), we

those whose region of origin is the same as their present camp location (viz., Northerners in a Northern and Southerners in a Southern camp); and it must be divided by the factor 0.86 for those whose region of origin differs from their present camp location (viz., Northerners in a Southern and Southerners in a Northern camp). For further comments, see the final section herein.

⁴ The relationship between formulas (12)–(16) and certain formulas in the analysis of variance will be clarified when we discuss formulas (23)–(27).

⁵ From the relationship between γ^{BO}_{11} , γ^{BO}_{21} , γ^{BO}_{12} and γ^{BO}_{22} described by formula (11), we see that, after taking account of the various main effects in the model, the estimate of the expected odds Ω^D_{1jk} favoring a preference for a Northern camp must be multiplied by the factor 0.86 for

see the effect of $\gamma^{\overline{AD}}_1$. Comparing (17a) with (17c), we see the effect of $\gamma^{\overline{BD}}_1$ and $\gamma^{\overline{BD}}_{11}$. Comparing (17a) with (17d), we see the effect of $\gamma^{\overline{OD}}_1$ and $\gamma^{\overline{OD}}_{11}$.

We used the superscript $\overline{D}$ in the preceding two paragraphs to emphasize the fact that the odds Ω_{ijk} pertain to variable D, and that the γ parameters describe the main and interaction effects on these odds. To simplify notation we will delete this superscript hereafter, in all but one section of the paper.

Formula (10) expresses Ω_{ijk} as a product of certain main and interaction effect parameters. This formula can also be expressed in an additive form via logarithms. First, corresponding to Ω_{ijk} , we let Φ_{ijk} denote the natural logarithm of Ω_{ijk} ; i.e., we define Φ_{ijk} as

$$\Phi_{ijk} = \log \Omega_{ijk}, \quad (18)$$

where "log" denotes the natural logarithm. Second, corresponding to formula (10)'s set of parameters ($\gamma, \gamma^A_1, \gamma^B_j, \gamma^C_k, \gamma^{BO}_{jk}$), we define a new set as follows:

$$\begin{aligned} \beta &= \log \gamma, \quad \beta^A_1 = \log \gamma^A_1, \\ \beta^B_j &= \log \gamma^B_j, \text{ etc.} \end{aligned} \quad (19)$$

Then from (10) and (18)–(19) we see that

$$\Phi_{ijk} = \beta + \beta^A_1 + \beta^B_j + \beta^C_k + \beta^{BO}_{jk}. \quad (20)$$

From (11) and (19) we see that

$$\begin{aligned} \beta^A_1 &= -\beta^A_2, \quad \beta^B_1 = -\beta^B_2, \quad \beta^C_1 = -\beta^C_2, \\ \beta^{BO}_{11} &= \beta^{BO}_{22} = -\beta^{BO}_{12} = -\beta^{BO}_{21}, \end{aligned} \quad (21)$$

which can also be expressed as follows:

$$\begin{aligned} \sum_{i=1}^2 \beta^A_i &= 0, \quad \sum_{j=1}^2 \beta^B_j = 0, \quad \sum_{k=1}^2 \beta^C_k = 0, \\ \sum_{j=1}^2 \beta^{BO}_{jk} &= 0 \quad (\text{for } k = 1, 2), \end{aligned} \quad (22)$$

$$\sum_{k=1}^2 \beta^{BO}_{jk} = 0 \quad (\text{for } j = 1, 2).$$

Parameters $\beta, \beta^A_1, \beta^B_1$, and β^C_1 describe the main effects on Φ_{ijk} of the general mean⁶ and variables A, B, and C; and parameter β^{BO}_{11} describes the interaction effect of variables B and C on Φ_{ijk} . The model described by formula (20), which expresses

⁶ Since β is somewhat analogous to the main effect of the general mean in the usual model for the analysis of variance (i.e., the constant term in that model), we refer to β as the main effect of the general mean on Φ_{ijk} . β actually equals the arithmetic mean of the Φ_{ijk} corresponding to the eight possible values of (i, j, k) obtained when $i=1, 2$; $j=1, 2$; and $k=1, 2$. For further details, see formula (23) below.

Φ_{ijk} in terms of five parameters ($\beta, \beta^A_1, \beta^B_1, \beta^C_1, \beta^{BO}_{11}$), is equivalent to that described by formula (10), which expresses the corresponding Ω_{ijk} in terms of the corresponding five parameters ($\gamma, \gamma^A_1, \gamma^B_1, \gamma^C_1, \gamma^{BO}_{11}$).

We noted earlier that model (10)'s parameters could be expressed explicitly in terms of Ω_{ijk} (see formulas (12)–(16)). Similarly, the parameters in model (20) can be expressed explicitly in terms of Φ_{ijk} . From (20)–(22), we obtain the following expressions for these parameters in terms of Φ_{ijk} :

$$\beta = \left[\sum_{i=1}^2 \sum_{j=1}^2 \sum_{k=1}^2 \Phi_{ijk} \right] / 8, \quad (23)$$

$$\beta^A_1 = [\Phi_{1jk} - \Phi_{2jk}] / 2 \quad (\text{for } j = 1, 2; k = 1, 2) \quad (24)$$

$$= \left[\sum_{j=1}^2 \sum_{k=1}^2 (\Phi_{1jk} - \Phi_{2jk}) \right] / 8,$$

$$\beta^B_1 = \left[\sum_{j=1}^2 \sum_{k=1}^2 (\Phi_{11k} - \Phi_{12k}) \right] / 8, \quad (25)$$

$$\beta^C_1 = \left[\sum_{i=1}^2 \sum_{j=1}^2 (\Phi_{ij1} - \Phi_{ij2}) \right] / 8, \quad (26)$$

$$\beta^{BO}_{11} = [\Phi_{111} + \Phi_{122} - \Phi_{112} - \Phi_{121}] / 4 \quad (\text{for } i = 1, 2) \quad (27)$$

$$= \left[\sum_{i=1}^2 [\Phi_{i11} + \Phi_{i22} - \Phi_{i12} - \Phi_{i21}] \right] / 8.$$

Formulas (23)–(27) are equivalent to the corresponding formulas (12)–(16).⁷ Formula (23) states that β is the arithmetic mean of Φ_{ijk} (corresponding to the eight possible values of (i, j, k)). Formula (24) states that β^A_1 can be expressed both as one-half the difference $\Phi_{1jk} - \Phi_{2jk}$ (for $j = 1, 2; k = 1, 2$), and as one-half the arithmetic mean of the differences $\Phi_{1jk} - \Phi_{2jk}$ corresponding to the four possible values of (j, k) obtained when $j = 1, 2; k = 1, 2$. Formula (25) states that β^B_1 equals one-half the arithmetic mean of the differences $\Phi_{11k} - \Phi_{12k}$ corresponding

⁷ Indeed, instead of obtaining (23)–(27) from formulas (20)–(22), we could also have obtained (23)–(27) from formulas (12)–(16) and (18)–(19). Similarly, we could have obtained (12)–(16) from formulas (23)–(27), making use of formula (28) below and the fact that $\Omega_{ijk} = \exp \Phi_{ijk}$.

to the four possible values of (i, k) obtained when $i = 1, 2$; $k = 1, 2$. Formula (26) can be similarly expressed, and formula (27) also has a somewhat similar interpretation.

Since Table 2 presents the estimated values of F_{ijk} (under model (10) or the equivalent model (20)), we can use these values to estimate first, Ω_{ijk} and Φ_{ijk} (see (8) and (18)) and second, the β parameters (see (23)–(27)). Table 3 includes the β parameters' estimated values. We can also use these values to calculate the γ parameters' estimated values since the relationship between the β and γ parameters can be expressed by (19) or by the following equivalent set of formulas:⁸

$$\gamma = \exp \beta, \gamma^A = \exp \beta^A, \gamma^B = \exp \beta^B, \quad (28)$$

etc., where "exp" denotes the exponential function.⁹

Earlier we discussed Table 3's γ parameter estimated values. Now let us examine the estimated values of β , β^A , β^B , β^C , β^{BC} , also given in Table 3. In line with our earlier discussion of Table 3, in examining the estimated β parameters, we note that the estimated main effect of each variable (A, B, C) is positive (i.e., the estimates of the β^A , β^B , β^C , which could have been written as $\beta^{A\bar{0}}$, $\beta^{B\bar{0}}$, $\beta^{C\bar{0}}$, are all positive); but the estimated interaction effect between variables B and C is negative (i.e., the estimate of β^{BC} , which could have been written as $\beta^{BC\bar{0}}$, is negative).¹⁰

⁸ For expository purposes, we discussed the γ before the β parameters. Since Table 3 already provided the γ parameters' estimated values, we could have used them in turn to estimate the β parameters (see (19)). Actually, rather than calculate the estimated β from the estimated γ parameters, calculated earlier from the estimated Ω_{ijk} (see (12)–(16)), it is easier to calculate the estimated γ from the corresponding estimated β parameters (see (28)), which can be calculated from the estimated Φ_{ijk} (see (23)–(27)).

⁹ The exponential function is the inverse of the natural logarithm. Comparison of (19) and (28) should make this point clear. For example, for a given γ value, we can calculate β from (19) using a table of natural logarithms; and for a given β value, we can calculate γ either from (19), with the natural-logarithm table used now in so to speak, inverted order, or equivalently from (28) using a table of the exponential function.

¹⁰ The fact that the estimate of β^{BC} is negative corresponds to the fact that the estimate of γ^{BC}

Later we shall show how to assess the statistical significance of the contribution made by certain parameters (e.g., γ^{BC}) in model (10), and by certain parameters (e.g., β^{BC}) in model (20), and we also show how to measure this contribution's magnitude.

Our models express the expected odds Ω_{ijk} in terms of the γ parameters (see (10) and also (29) below); or they express the expected log-odds Φ_{ijk} in terms of the β parameters (see (20) and also (35) below). These two forms of expression are equivalent. Since the expected frequencies F_{ijk} can be expressed in terms of the Ω_{ijk} (see (9)), our models can also be used to express F_{ijk} in terms of the γ parameters. In addition, letting P_{ijk} and Q_{ijk} denote the expected proportions F_{ijk1}/n_{ijk} and F_{ijk2}/n_{ijk} , respectively (see (3)–(4)), note that our models can also be used to express P_{ijk} (and Q_{ijk}) in terms of these γ parameters.

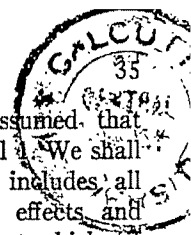
Before closing this section, we should note the relationship between model (20) and the usual models for (a) the analysis of variance and (b) the analysis of the "logit" pertaining to variable D.

Model (20) and the usual model for the three-way analysis of variance may be compared in several ways. (Note that the Φ_{ijk} in (20) can be presented in a three-way array, while the F_{ijk} are presented in a four-way table.) In the usual three-way analysis of variance, one must assume homoscedasticity, i.e., that each observation in the three-way table has the same variance. On the other hand, for our kind of data, the homoscedasticity assumption would be contradicted in a way that could not be ignored.¹¹ Our data also contradict the assumption in the usual analysis of variance that each observation has a normal distribution.¹²

is less than 1 (see (19) and (28)). We interpreted this fact in footnote (5). For further comment, see the final section herein.

¹¹ In the present context, we note that the variance of the observed proportion p_{ijk} (see (3)) will depend both on the magnitude of n_{ijk} (see (1)) and the expected proportion $P_{ijk} = F_{ijk1}/n_{ijk}$. A similar remark applies to the variance of the observed odds ω_{ijk} (see (2) and (5)) and the variance of the logarithm of ω_{ijk} . (The logarithm of the ω_{ijk} is of interest here since it corresponds to Φ_{ijk} in the same sense that ω_{ijk} corresponds to Ω_{ijk} ; see (2), (8), (18).)

¹² On the other hand, when n_{ijk} is large, the



Note also that formulas (20)–(22) and (23)–(27) are similar to formulas appearing in the usual analysis of variance. However, to estimate the β parameters under model (20), we use the estimated values of the expected frequencies F_{ijk} under the model (see Table 2) to estimate first Ω_{ijk} and Φ_{ijk} (see (8) and (18)); and then we use these estimated values of Φ_{ijk} in (23)–(27) to estimate the β parameters. In contrast, in the usual analysis of variance (assuming homoscedasticity), the quantity corresponding to the estimated Φ_{ijk} in formulas (23)–(27) is replaced by the observation in cell (i, j, k) ; and formulas (24) and (27) are replaced simply by the corresponding expressions on the second line of these two formulas.

Now let us consider the usual model for analyzing the logit pertaining to variable D. This logit is usually defined as being $\Phi_{ijk}/2$ (see, e.g., Fisher and Yates 1963). Model (20) states that this logit (multiplied by 2) can be expressed as a sum of parameters $\beta, \beta^A_i, \beta^B_j, \beta^C_k, \beta^{BC}_{jk}$ (i.e., the main effects of the general mean and of variables A, B, C, and the interaction effect between variables B and C). We can rewrite this model as a regression model expressing variable D's logit as a linear function of dummy variables pertaining to the main effects of variables A, B, C and the interaction effect between variables B and C; but homoscedasticity can not be assumed in this model. Later we shall test the statistical significance of the contribution made by certain parameters in this model, and we shall measure the contributions' magnitude by applying methods proposed in Goodman (1970, 1971a). For some related material, see also Dyke and Patterson (1952), Bishop (1969), Theil (1970), and the final section below.

A GENERAL MODEL FOR ANALYZING THE ODDS

Model (10) included the main effect on Ω_{ijk} of all three variables (A, B, C), but only one of three possible two-factor interaction effects (viz., γ^{BC}_{jk}); and it did not include the three-factor interaction effect

observed proportion p_{ijk} will be approximately normally distributed (as long as the expected proportion P_{ijk} differs sufficiently from the extreme values of 0 and 1). A similar remark applies to the observed odds ω_{ijk} and the logarithm of ω_{ijk} .

(viz., γ^{ABC}_{ijk}). This model assumed that $\gamma^{AB}_{ij}, \gamma^{AC}_{ik}$, and γ^{ABC}_{ijk} all equal 1. We shall now consider the model that includes all possible main and interaction effects and that makes no assumptions about which (if any) of these effects equals 1. Instead of model (10), we now have the following "saturated" model:

$$\Omega_{ijk} = \gamma \gamma^A_i \gamma^B_j \gamma^C_k \gamma^{AB}_{ij} \gamma^{AC}_{ik} \gamma^{BC}_{jk} \gamma^{ABC}_{ijk}, \quad (29)$$

where

$$\begin{aligned} \gamma^A_1 &= 1/\gamma^A_2, \dots, \gamma^{AB}_{11} = \gamma^{AB}_{22} = \\ &1/\gamma^{AB}_{12} = 1/\gamma^{AB}_{21}, \dots, \\ \gamma^{ABC}_{111} &= \gamma^{ABC}_{221} = \gamma^{ABC}_{212} = \gamma^{ABC}_{122} = \\ &1/\gamma^{ABC}_{112} = 1/\gamma^{ABC}_{121} = \\ &1/\gamma^{ABC}_{211} = 1/\gamma^{ABC}_{222}. \end{aligned} \quad (30)$$

Formula (29) describes the effects of the γ parameters on Ω_{ijk} . It expresses Ω_{ijk} explicitly in terms of the model's γ parameters. These parameters can also be expressed explicitly in terms of Ω_{ijk} . From (29)–(30), we obtain the following expressions for the parameters in terms of the Ω_{ijk} :¹³

$$\gamma = \left[\prod_{i=1}^2 \prod_{j=1}^2 \prod_{k=1}^2 \Omega_{ijk} \right]^{1/8}, \quad (31)$$

$$\gamma^A_1 = \left[\prod_{j=1}^2 \prod_{k=1}^2 (\Omega_{1jk}/\Omega_{2jk}) \right]^{1/4}, \quad (32)$$

$$\gamma^{AB}_{11} = \left[\prod_{k=1}^2 (\Omega_{11k}\Omega_{22k})/(\Omega_{12k}\Omega_{21k}) \right]^{1/4}, \quad (33)$$

$$\gamma^{ABC}_{111} = [(\Omega_{111}\Omega_{221}\Omega_{212}\Omega_{122})/(\Omega_{112}\Omega_{121}\Omega_{211}\Omega_{222})]^{1/8}. \quad (34)$$

For the saturated model (29)–(30), we can estimate the γ parameters by formulas (31)–(34), replacing the expected odds Ω_{ijk} in these formulas by the corresponding observed odds ω_{ijk} .¹⁴ With the saturated

¹³ Formulas (31)–(34) for the saturated model (29)–(30) correspond to formulas (12)–(16) for the unsaturated model (10)–(11).

¹⁴ In contrast to this procedure for the saturated model, note that for an unsaturated model (e.g., model (10)) we use the estimated values of the expected frequencies F_{ijk} under the model (see Table 2) to estimate Ω_{ijk} (see (8)); then we can use these estimated values of the Ω_{ijk} in (31)–(34) to estimate the γ parameters. (When these estimated values of the Ω_{ijk} are used in (31)–(34), we

Table 4. Estimate of the Main Effects and Interaction Effects of the Three Variables (A,B,C) on the Odds Ω_{ijk} Pertaining to Variable D in the Four-Way Contingency Table (Table 1), Under the Saturated Models (29) and (35)

Variable	γ Effects in Model (29)	β Effects in Model (35)	Standardized Value
D	1.28	.25	6.96
AD	1.44	.37	10.21
BD	3.43	1.23	34.36
CD	2.10	.74	20.65
ABD	0.96	-.04	-1.11
ACD	1.00	.00	0.00
BCD	0.86	-.15	-4.31
ABCD	0.97	-.03	-0.86

model's γ parameters thus estimated, the observed data fit perfectly. (For further comments on this point, see footnote 19 later herein.) Based on Table 1's data, the γ parameters' estimated values are given in Table 4. Note that, for Table 1's data, Table 4's estimated γ 's are quite similar to the corresponding quantities of Table 3.

Having replaced the unsaturated (10) with the saturated model (29), we can also replace the unsaturated (20) with the following saturated model:

$$\Phi_{ijk} = \beta + \beta^A_1 + \beta^B_j + \beta^C_k + \beta^{AB}_{ij} + \beta^{AC}_{ik} + \beta^{BC}_{jk} + \beta^{ABC}_{ijk}, \quad (35)$$

where

$$\begin{aligned} \beta^A_1 &= -\beta^B_2, \dots, \beta^{AB}_{11} = \beta^{AB}_{22} = \\ &= -\beta^{AB}_{12} = -\beta^{AB}_{21}, \dots, \\ \beta^{ABO}_{111} &= \beta^{ABO}_{221} = \beta^{ABO}_{212} = \beta^{ABO}_{122} \\ &= -\beta^{ABO}_{112} = -\beta^{ABO}_{121} = \\ &= -\beta^{ABO}_{211} = -\beta^{ABO}_{222}. \end{aligned} \quad (36)$$

obtain the same results as when they are used in (12)-(16). For an unsaturated model (e.g., model (10)), the entries in Table 2 are the maximum-likelihood estimates of the F_{ijk} under the model, and they are calculated by an iterative procedure which we shall comment upon later herein after we have presented the material in Table 5. The observed frequencies f_{ijk} are the maximum-likelihood estimates of the F_{ijk} under the saturated model, but *not* under an unsaturated model. Similarly, the observed odds ω_{ijk} are the maximum-likelihood estimates of the Ω_{ijk} under the saturated model, but *not* under an unsaturated model.

Model (35)-(36) is, of course, equivalent to model (29)-(30). Similarly, formulas (31)-(34) are equivalent to the following set of formulas:

$$\beta = \left[\sum_{i=1}^2 \sum_{j=1}^2 \sum_{k=1}^2 \Phi_{ijk} \right] / 8, \quad (37)$$

$$\beta^A_1 = \left[\sum_{j=1}^2 \sum_{k=1}^2 (\Phi_{1jk} - \Phi_{2jk}) \right] / 8, \quad (38)$$

...

$$\beta^{AB}_{11} = \left[\sum_{k=1}^2 (\Phi_{11k} + \Phi_{22k} - \Phi_{12k} - \Phi_{21k}) \right] / 8, \quad (39)$$

...

$$\begin{aligned} \beta^{ABO}_{111} &= [\Phi_{111} + \Phi_{221} + \Phi_{212} + \Phi_{122} \\ &- \Phi_{112} - \Phi_{121} - \Phi_{211} - \Phi_{222}] / 8. \end{aligned} \quad (40)$$

For the saturated model (35)-(36) we can estimate the β parameters by formulas (37)-(40), replacing the "expected log-odds" Φ_{ijk} in these formulas by the corresponding log ω_{ijk} .¹⁶ In addition, the variance of the estimated β parameters can be estimated by the following formula:¹⁶

$$S^2_{\hat{\beta}} = \left[\sum_{i=1}^2 \sum_{j=1}^2 \sum_{k=1}^2 \sum_{l=1}^2 (1/f_{ijkl}) \right] / 64. \quad (41)$$

By dividing each estimated β parameter by its estimated standard deviation $S_{\hat{\beta}}$, we obtain the corresponding "standardized value" of the estimate. Each standardized value can be used to test whether the corresponding β parameter is nil.¹⁷ Table 4 in-

¹⁶ Remarks similar to those in footnotes 8 and 14 would apply here as well.

¹⁷ Note should be taken of the fact that the estimation method presented herein for the saturated model can be improved upon by replacing f_{ijk} in (41) by $f_{ijk} + 1/2$, and replacing the ω_{ijk} that are used in (31)-(34) (or in (37)-(40)) by $\omega_{ijk} = (f_{ijk} + 1/2) / (f_{ijk} + 1/2)$. It should also be noted that formula (41) and some of the other results presented herein are applicable both in the case where the observed four-way table (Table 1) describes results obtained for a random sample of individuals cross-classified with respect to the four variables (A, B, C, D), and also in the case where the f_{ijk} s and f_{ijkl} s in row (i, j, k) of Table 1 describe results obtained with respect to variable D for a random sample of n_{ijk} individuals at levels i, j, k on variables A, B, C, respectively. For further details, see Goodman (1970) and Haberman (1970).

¹⁷ The term "standardized value" of a statistic is used here to mean the ratio of the statistic and

cludes the β parameters' estimated values, and their corresponding standardized values.

By examining the magnitudes of Table 4's standardized values, we find that the model in which β^{AB}_{ij} , β^{AC}_{jk} , β^{ABO}_{ijk} are set equal to zero in (35) should merit consideration. (Recall that these three parameters could also have been written as $\beta^{AB\bar{D}}_{ij}$, $\beta^{AC\bar{D}}_{jk}$, $\beta^{ABO\bar{D}}_{ijk}$, respectively.) But the model obtained when these particular parameters are set equal to zero in (35) is equivalent to model (20). Thus, for Table 1's data, examining the saturated models (29) and (35) leads to models (10) and (20).

HOW TO TEST WHETHER A MODEL FOR THE ODDS FITS THE DATA

To test whether the hypothesis H described by model (10) fits Table 1's data, we first estimate the expected frequencies F_{ijkl} under the hypothesis H (see Table 2), and then compare the observed frequency f_{ijkl} in Table 1 with the corresponding estimate of the F_{ijkl} in Table 2, by calculating either the usual chi-square goodness-of-fit statistic

$$\sum_{i=1}^2 \sum_{j=1}^2 \sum_{k=1}^2 \sum_{l=1}^2 (f_{ijkl} - F_{ijkl})^2 / F_{ijkl}, \quad (42)$$

or the corresponding chi-square based on the likelihood-ratio statistic; viz.,

$$2 \sum_{i=1}^2 \sum_{j=1}^2 \sum_{k=1}^2 \sum_{l=1}^2 f_{ijkl} \log [f_{ijkl} / F_{ijkl}]. \quad (43)$$

The chi-square value obtained from (42) or (43) can be assessed by comparing its numerical value with the percentiles of the tabulated chi-square distribution. The degrees of freedom for testing hypothesis H will be $8 - 5 = 3$ (since (a) there are eight observed odds in Table 1, and (b) there are five γ parameters estimated in model (10)).

Using (42), we obtain a goodness-of-fit

its estimated standard deviation. The same or similar words have also been used by other writers to denote other things with which the usage here should not be confused.

If a particular β parameter is nil, then the standardized value of the corresponding estimated β will be approximately normally distributed with zero mean and unit variance (when the sample size is large). For comments on related matters, see Goodman (1970, 1971a).

chi-square value of 1.46, and using (43), a likelihood-ratio chi-square value of 1.45. Since there were three degrees of freedom under H, the model fits the data well.

Model (10) is obtained from the saturated model (29) by making a specific set of its γ parameters equal to one.¹⁸ Model (20) is obtained from the saturated model (35) by making a specific set of its β parameters equal to zero. Models obtained this way from saturated models we call "unsaturated."¹⁹ Of course, all unsaturated models obtained from (29) or (35) are models for the odds Ω_{ijk} (or the log-odds Φ_{ijk}) pertaining to variable D. Thus, all these unsaturated models view the four-way table (Table 1) asymmetrically. In the four-way table for variables (A, B, C, D), we treated variable D as the dependent variable; i.e., we viewed the odds (or log odds) pertaining to variable D as depending on the level of variables (A, B, C).

When each individual in a sample is classified by four dichotomous variables (e.g., A, B, C, D), we obtain a four-way table (e.g., Table 1); and, in some contexts, any one of the four variables might be viewed as the dependent variable. For the four-way table, the expected frequencies estimated under a given unsaturated model that treats variable D as the dependent variable (see, e.g., Table 2) will usually differ from the corresponding expected frequencies estimated under a model that treats one of the other variables as the dependent variable.

In some contexts, the research worker will know which variable should be treated as the dependent variable; in others, any one of the four might be treated so. In still others, a different point of view would be appropriate. We could, for example, con-

¹⁸ Indeed, the three degrees of freedom used above to test model (10) correspond to the three γ parameters (viz., γ^{AB}_{11} , γ^{AC}_{11} , γ^{ABO}_{111}) in (29) that are set equal to one under model (10).

¹⁹ The number of degrees of freedom used to test a given unsaturated model will equal the number of γ parameters in (29) that are set equal to one under the unsaturated model. Since none of the γ parameters in (29) are set equal to one under that model (i.e., the number of γ parameters set equal to one is zero), there will be zero degrees of freedom under the saturated model. This corresponds to the fact that the observed data fit perfectly under the saturated model, since it includes all possible main and interaction effects (i.e., all possible γ parameters).

sider the case where none of the variables is the dependent variable, but where all are mutually related in some sense (see, e.g., Goodman, 1970). For the four-way table, Goodman (1970) described in his Table 4 a large class of models that would include as special cases models like our (10) and (20) which treat one variable as the dependent variable, as well as "unsaturated" models of a different kind where none of the variables is treated as the dependent variable but where some or all may be mutually related variables. Goodman's Table 4 (1970) contains fifty-three different "unsaturated models" in which one of the four variables is treated as the dependent variable and 113 different "unsaturated" models in which none of the four variables is viewed as the dependent variable. For the case where a given variable (say variable D) is the dependent variable, Goodman's Table 4 (1970) lists nineteen different unsaturated models.

Earlier herein we considered the case where a given variable is the dependent variable. Our models well suit this case (see (10), (20), (29), (35)). Many readers will find our exposition of this case easier to understand than the exposition of the more general case in Goodman (1970). Nevertheless, the more general models and methods of the earlier article also apply to the special case we considered. For each unsaturated model of the kind considered herein, and also for other kinds of "unsaturated" models, Goodman's Table 4 (1970) gave the corresponding degrees of freedom when each variable in the contingency table is dichotomous. He also described ways to calculate the degrees of freedom when some variables are polytomous but not necessarily dichotomous. A single computer program can be used to calculate the estimate of the F_{ijk1} , and the corresponding chi-square values (42) and (43), for any set of "unsaturated" models of the kinds considered herein and in Goodman (1970). For related material dealing with such models see, e.g., Bishop (1969), Goodman (1970, 1971a, 1972).

Let us reconsider model (10), which we obtained from the saturated model (29) by making some of its γ parameters equal to one. We can describe this unsaturated model

in any of the following equivalent ways:

- (1) By listing the γ parameters that are included in model (10); viz., $\gamma^{\bar{D}} \gamma^{\bar{A}\bar{D}}_1, \gamma^{\bar{B}\bar{D}}_j, \gamma^{\bar{C}\bar{D}}_k, \gamma^{\bar{B}\bar{C}\bar{D}}_{jk}$.²⁰
- (2) By listing the γ parameters in (29) that are set equal to one under the model; viz., $\gamma^{\bar{A}\bar{B}\bar{D}}_{ij}, \gamma^{\bar{A}\bar{C}\bar{D}}_{ik}, \gamma^{\bar{A}\bar{B}\bar{C}\bar{D}}_{ijk}$.
- (3) By listing the particular marginal tables that are fitted under the model—a topic we shall now discuss.

From our Table 1 we can determine n_{ijk} as defined by formula (1). In all unsaturated models obtained from the saturated model (29), the n_{ijk} are considered fixed; thus in these models the expected frequencies F_{ijk1} (under the model) will satisfy the following condition:

$$F_{ijk1} + F_{ijk2} = n_{ijk}. \quad (44)$$

By comparing the n_{ijk} from Table 1 with the estimated value of $F_{ijk1} + F_{ijk2}$ from Table 2, we see that condition (44) is satisfied. Since the n_{ijk} describe the three-way marginal table pertaining to variables (A, B, C), we shall use the symbol {ABC} to denote this table. Condition (44) states that the marginal table {ABC} is fitted under the model.

In addition to the marginal table {ABC}, two other marginal tables are fitted under model (10); viz., the two-way marginal table {AD} and the three-way marginal table {BCD}. Table 5 gives the three marginal tables fitted under model (10).²¹ In the preceding paragraph, we explained why the marginal table {ABC} was fitted. Under model (10), we also fit the marginal tables {AD} and {BCD} because it includes the parameters $\gamma^{\bar{A}\bar{D}}_1$ and $\gamma^{\bar{B}\bar{C}\bar{D}}_{jk}$, which pertain to the relationship between variables A and D (as displayed in the marginal table {AD})

²⁰ We return now to the notation used earlier where the letter $\bar{D}$ was included in the superscript of each γ parameter to emphasize the fact that the γ parameters describe the main and interaction effects on the odds pertaining to variable D. This notation will facilitate some of our present exposition. This notation's utility will become clearer two paragraphs below.

²¹ The four-way contingency table of observed data (Table 1) can be displayed as a $2 \times 2 \times 2 \times 2$ table, or an 8×2 table (as in Table 1); similarly the three-way marginal table {ABC} can be displayed as a $2 \times 2 \times 2$ table, or a 4×2 table (as in Table 5), or as a 8×1 table (as we would obtain if we present it as the marginal of the 8×2 table displayed in Table 1).

REGRESSION ANALYSIS

Table 5: The Three Marginal Tables That are Fitted When Models (10) and (20) are Applied to the Four-Way Contingency Table (Table 1)

I. Table {ABC}			
Variable A	Variable B	Variable C	
		North	South
Negro	North	423	1126
Negro	South	653	2093
White	North	1117	1384
White	South	280	960

II. Table {AD}			
Variable A	Variable D		
	North	South	
Negro	2027	2268	
White	2024	1717	

III. Table {BCD}			
Variable B	Variable C	Variable D	
		North	South
North	North	1263	286
North	South	764	1982
South	North	1829	672
South	South	195	1045

and to the joint relationship among variables B, C, and D (as displayed in the marginal table {BCD}).²²

The reader will find that the entries in the three marginal tables in Table 5, which were calculated from Table 1's data, equal the corresponding entries in the three mar-

²² When the three-way marginal table {BCD} is fitted, then the following two-way marginal tables will fit automatically: {BC}, {BD}, {CD}. Similarly, when a two-way marginal table, say, {AD} is fitted, then the two one-way marginals, {A} and {D}, will fit automatically. Corresponding to the superscript of each γ parameter in model (10) (with the letter D added to each superscript), a marginal table pertaining to that superscript will be included in the set of marginal tables fitted under the model. Under model (10), it will suffice to include {AD} and {BCD} (in addition to table {ABC}) in the set of fitted marginal tables, since then all the marginal tables corresponding to the model's γ parameters (viz., {D}, {AD}, {BD}, {CD}, {BCD}) will actually be fitted.

ginal tables calculated from Table 2's estimated F_{ijk} . The computer program, to which we referred in the fourth paragraph preceding this one, calculated the estimated values in Table 2 (viz., the maximum-likelihood estimates of the expected frequencies F_{ijk} under model (10)) by an iterative procedure which insured that the three marginal tables given in Table 5 would be fitted when Table 2's estimated F_{ijk} are used. For further details about the computing procedure, see, for example, the literature cited in the paragraph referred to above.

Although the three marginal tables (viz., {ABC}, {AD}, {BCD}) in Table 5 are fitted under model (10), we noted earlier that the reason for fitting {ABC} in the present context is somewhat different from the reason for fitting {AD} and {BCD}. The marginal table {ABC} is considered to be fixed under model (10); i.e., the n_{ijk} in (1) and (44) are viewed as constants. Aside from the n_{ijk} constants, to estimate the F_{ijk} under model (10), we use only the information contained in the observed marginals tables {AD} and {BCD}.

The above remarks pertain to model (10), but they can be extended in a straightforward way to a wide range of unsaturated models obtained from the saturated model (29) by setting certain specified γ parameters in (29) equal to one. Now let's apply several such unsaturated models to Table 1's data. Table 6 lists the chi-square values (42) and (43) obtained in testing these models. We include both chi-square values (42) and (43) in Table 6; but, in the present context, (43) has some advantages (see, e.g., Goodman 1968, 1970). In the remaining discussion, we shall use only the chi-square value based on (43).

Each model in Table 6 is described there by listing the marginal tables fitted under the model. For the sake of brevity, we actually list in Table 6 the "minimal set" of marginal tables fitted under the model, rather than the entire set of marginal tables that will in fact be fitted (see footnote 22 herein and Goodman 1970). For example, for model (10), which is presented as H_1 in Table 6, we list in Table 6 the following "minimal set" of marginal tables fitted under the model: {ABC}, {AD}, {BCD}. From this "minimal set" of marginal tables,

Table 6. Chi-Square Values for Some Models Pertaining to Table 1

Model	Fitted Marginals	Degrees of Freedom	Likelihood-Ratio Chi-Square	Goodness-of-Fit Chi-Square	γ Parameters Included in the Model
H ₁	{ABC},{AD},{BCD}	3	1.45	1.46	[D],[AD],[BD],[CD],[BCD]
H ₂	{ABC},{AD},{BD},{CD}	4	24.96	25.73	[D],[AD],[BD],[CD]
H ₃	{ABC},{BCD}	4	152.65	147.59	[D],[BD],[CD],[BCD]
H ₄	{ABC},{BD},{CD}	5	186.36	180.26	[D],[BD],[CD]
H ₅	{ABC},{AD},{CD}	5	2286.83	2187.71	[D],[AD],[CD]
H ₆	{ABC},{AD},{BD}	5	695.01	727.16	[D],[AD],[BD]
H ₇	{ABC},{D}	7	3111.47	2812.64	[D]
H ₈	{ABC},{ACD},{BCD}	2	1.32	1.34	[D],[AD],[BD],[CD],[ACD],[BCD]
H ₉	{ABC},{ABD},{BCD}	2	0.68	0.69	[D],[AD],[BD],[CD],[ABD],[BCD]
H ₁₀	{ABC},{ABD},{ACD}	2	17.29	18.73	[D],[AD],[BD],[CD],[ABD],[ACD]
H ₁₁	{ABD},{ACD},{BCD}	2	24.79	25.11	*
H ₁₂	None	15	5469.88	5989.11	*

*Models H₁₁ and H₁₂ cannot be expressed in terms of the γ parameters. See related discussion in the present article.

we find that the following marginal tables will in fact be fitted: {D}, {AD}, {BD}, {CD}, {BCD} as well as {ABC} and all the marginal tables formed from {ABC}. Variable D is included in five of the marginal tables listed above, and the model under consideration (i.e., model H₁ of Table 6) will include the following five γ parameters corresponding to these five marginal tables: $\gamma^{\bar{D}}$, $\gamma^{A\bar{D}}$, $\gamma^{B\bar{D}}$, $\gamma^{C\bar{D}}$, $\gamma^{BC\bar{D}}$.

Consider now hypothesis H₂ in Table 6. Since this model fits the marginals {ABC}, {AD}, {BD}, {CD}, it will include the following γ parameters: $\gamma^{\bar{D}}$, $\gamma^{A\bar{D}}$, $\gamma^{B\bar{D}}$, $\gamma^{C\bar{D}}$.²³ Similarly, hypothesis H₈ in Table 6 fits the marginals {ABC}, {BCD}; and thus that model includes the following γ parameters: $\gamma^{\bar{D}}$, $\gamma^{B\bar{D}}$, $\gamma^{C\bar{D}}$, $\gamma^{BC\bar{D}}$. Hypothesis H₄ in

Table 6 fits the marginals {ABC}, {BD}, {CD}; and thus that model includes the following γ parameters: $\gamma^{\bar{D}}$, $\gamma^{B\bar{D}}$, $\gamma^{C\bar{D}}$. Let us discuss these and other models in Table 6 further.

As we have already noted, model (10) is listed as H₁ of Table 6. If we now make $\gamma^{BC\bar{D}}$ equal to 1 in model (10), we get H₂ of Table 6. In model H₂, the odds pertaining to variable D are expressed in terms of the parameters $\gamma^{\bar{D}}$, $\gamma^{A\bar{D}}$, $\gamma^{B\bar{D}}$, $\gamma^{C\bar{D}}$, i.e., the main effects of the general mean and variables A, B, and C. To test whether the parameter $\gamma^{BC\bar{D}}$ in model (10) contributes in a statistically significant way, we can use the difference between the corresponding chi-square values for H₂ and H₁ as a chi-square statistic with one degree of freedom. (We get the one degree of freedom by subtracting the corresponding degrees of freedom for H₂ and H₁; i.e., 4-3=1.) From Table 6's chi-square values for H₂ and H₁, we see that $\gamma^{BC\bar{D}}$ does contribute to model (10) in a statistically significant way.

²³ For the reader who has difficulty determining which γ parameters are included in the model from the description of the model in terms of the marginal tables that are fitted, we include this information in Table 6's final column. In that column, we use the symbols [D], [AD], [BD], . . . , to denote $\gamma^{\bar{D}}$, $\gamma^{A\bar{D}}$, $\gamma^{B\bar{D}}$, . . . , respectively.

If we set γ^{AD_1} equal to 1 in model (10), we get H_3 of Table 6. To test whether the parameter γ^{AD_1} in model (10) contributes in a statistically significant way, we can use the difference between the corresponding chi-square values for H_3 and H_1 as a chi-square statistic with one degree of freedom. From Table 6's chi-square values for H_3 and H_1 , we see that γ^{AD_1} does contribute to model (10) in a statistically significant way.

If we set γ^{AD_1} and $\gamma^{BOD_{jk}}$ equal to 1 in model (10), we get H_4 of Table 6. If we set γ^{BD_j} and $\gamma^{BOD_{jk}}$ equal to 1 in model (10), we get H_5 . If we set γ^{OD_k} and $\gamma^{BOD_{jk}}$ equal to 1 in model (10), we get H_6 . Comparing the magnitudes of Table 6's corresponding three chi-square values, we see that the worst fitting model was H_5 , the next worst H_6 , and the least worst H_4 . In other words, by comparing the three models obtained from H_2 by deleting the main effect of one of the variables (A, B, C), we see that γ^{BD_j} contributes the most.

If we set γ^{AD_1} , γ^{BD_j} , γ^{OD_k} , and $\gamma^{BOD_{jk}}$ equal to 1 in model (10), we get H_7 of Table 6. In model H_7 , the odds pertaining to variable D depend on γ^D (the main effect of the general mean), but are unaffected by the level of variables A, B, and C. In other words, model H_7 states that variable D is independent of the joint variable A, B, C. From the chi-square value for H_7 in Table 6, we see that the data contradict this model.

If we set $\gamma^{ABD_{ij}}$ and $\gamma^{ABOD_{ijk}}$ equal to 1 in model (29), we get H_8 of Table 6. If we set $\gamma^{AOD_{ik}}$ and $\gamma^{ABOD_{ijk}}$ equal to 1 in model (29), we get H_9 . If we set $\gamma^{BOD_{jk}}$ and $\gamma^{ABOD_{ijk}}$ equal to 1 in model (29), we get H_{10} . Table 6 shows that H_8 and H_9 fit the data well, but H_{10} does not.

From the above description of H_8 and H_9 , we can express model H_1 as follows: Model H_1 states both that H_8 is true *and* that $\gamma^{AOD_{ik}}$ in H_8 equals 1. Model H_1 also states both that H_9 is true *and* that $\gamma^{ABD_{ij}}$ in H_9 equals 1. Thus, if H_1 is true, then H_8 and H_9 will also be true; but H_8 and H_9 can be true in cases where H_1 is not. H_1 implies models H_8 and H_9 .

Models H_{11} and H_{12} of Table 6 differ from H_1 to H_{10} in an important respect.

These last two models do not include the marginal {ABC} among the marginals that are fitted under the model. Therefore, the expected frequencies F_{ijk} estimated under models H_{11} and H_{12} will *not* satisfy condition (44) (except in some special cases). These two models cannot be expressed as unsaturated models obtained from the saturated model (29), except in cases where condition (44) is satisfied.

Model H_{12} of Table 6 is easier to describe than H_{11} , so I will describe it first. Model H_{12} states that the sixteen cells of Table 1 are equiprobable. From the chi-square value for H_{12} in Table 6, we see that the data contradict this model.

Now let us consider H_{11} of Table 6. As we noted above, this model cannot be expressed as one in which variable D is the dependent variable, since table {ABC} is not fitted under it. However, since the other three-way marginal tables (viz., {ABD}, {ACD}, {BCD}) are fitted under model H_{11} , we see that H_{11} is a model in which any one of the other variables (C, B, or A) can be viewed as the dependent variable. (Note that three of the four possible three-way marginal tables are fitted under H_{11} , and also under H_8 , and H_9 , and H_{10} .) From the chi-square value for H_{11} in Table 6, we see that the data contradict this model.

We noted earlier that Goodman's Table 4 (1970) included models in which one of the variables is treated as the dependent variable, and it included other kinds of models as well. To test whether any of these other kinds of models might fit the data in our four-way table, we would first consider H_{11} of Table 6; for this model assumes only that the F_{ijk} are not affected by the three factor "interaction effect" among the three variables A, B, C, nor by the four-factor "interaction effect" among variables A, B, D, C.²⁴ If this particular model does not fit

²⁴ Under model H_{11} , the only tables not fitted to the data are the three-way marginal table {ABC} and the four-way table {ABCD}; so the F_{ijk} (under model H_{11}) are not affected by the three-factor "interaction effect" among variables A, B, C (as displayed in the marginal table {ABC}) nor by the four-factor "interaction effect" among variables A, B, C, D (as displayed in the four-way table). We use the term "interaction effect" in the preceding sentence, and in the sentence to which this footnote applies, in a way

Table 7. Analysis of the Variation in the Odds Pertaining to Variable D in the Four-Way Contingency Table (Table 1)

Source of Variation	Degrees of Freedom	Chi-Square	Numerical Value
1. Total variation due to the "main effects" of variables A,B,C and "interaction effects" among these variables	7	$X^2(H_7)$	3111.47
1a. Due to variation unexplained by model H_1	3	$X^2(H_1)$	1.45
1b. Due to variation explained by model H_1	4	$X^2(H_7) - X^2(H_1)$	3110.02
<u>Partition of (1a)</u>			
1a.1. Due to variation unexplained by model H_9	2	$X^2(H_9)$	0.68
1a.2. Due to variation explained by the $\gamma_{ij}^{AB\bar{D}}$ parameter in model H_9	1	$X^2(H_1) - X^2(H_9)$	0.77

the data, then the data will also contradict any of the other kinds of "unsaturated" models that do not treat variable D as the dependent variable.²⁵ For examples of data that do not contradict these other kinds of "unsaturated" models, see, Goodman (1970, 1971a).

Before closing this section, we should note that some of the material discussed above could be presented in summary form in tables that are somewhat analogous to the usual analysis of variance tables. Table 7 is an example.

MULTIPLE AND PARTIAL CORRELATION COEFFICIENTS FOR MODELS FOR THE ODDS

In the usual multiple regression analysis for quantitative variables (predicting vari-

related to but different from the way we used it earlier. Earlier the term referred to the interaction effects of certain variables on the expected odds Ω_{ijk} pertaining to variable D; whereas above the term refers to the "interaction effects" among certain variables in the four-way table. For further details, see Goodman (1970, 1971a).

²⁵ Except for model H_{11} , any other unsaturated model that does not treat variable D as the dependent variable can be viewed as a model that states both that H_{11} is true and that some additional "interaction effects" (in addition to the particular three and four-factor "interaction effects" noted in sentence one of footnote 24) can be set equal to one. Thus, if any other unsaturated model (of the above kind) is true, then H_{11} will also be true. If H_{11} is not true, then none of the other unsaturated models (of the above kind) can be true. For related matters, see Goodman (1970, 1971a).

able Y from, say, variables X_1 and X_2), the quantity $R^2_{Y \cdot X_1 X_2}$, which is the square of the multiple correlation coefficient, can be interpreted as follows: It is the relative decrease in Y's "unexplained variation" obtained when comparing the case where X_1 and X_2 are not used to predict Y with the case where both are used. Similarly, the quantity $r^2_{YX_1 \cdot X_2}$, which is the square of the partial correlation coefficient, can be interpreted as follows: It is the relative decrease in Y's unexplained variation obtained when comparing the case where X_2 but not X_1 is used to predict Y with the case where both are used. The quantity $R^2_{Y \cdot X_1 X_2}$ is sometimes referred to as the coefficient of multiple determination, and the quantity $r^2_{YX_1 \cdot X_2}$ can be called the coefficient of partial determination. Goodman (1970, 1971a) introduced coefficients that are somewhat analogous to the usual coefficients of multiple and partial determination for analyzing the odds pertaining to a given variable in the four-way contingency table. We shall now illustrate their calculation.

For a given model in Table 6 (say model H_i , for $i = 1, 2, \dots, 12$), we shall use the symbol $X^2(H_i)$ to denote its chi-square value. In the preceding section, we noted, among other things, that the statistic $X^2(H_2) - X^2(H_1)$ could be used to test whether the parameter γ^{BC}_{jk} in H_1 contributed in a statistically significant way.²⁶ To

²⁶ To facilitate exposition in the preceding section, we included the letter $\bar{D}$ in the superscript of

measure the contribution's magnitude, we recommend the following coefficient, which we shall call the coefficient of partial determination between the odds ω_{ijk} and the parameter γ^{BC}_{jk} , when the other γ 's in model H_1 are taken into account: ²⁷

$$r^2_{\omega \gamma^{BC} \cdot H_1} = [X^2(H_2) - X^2(H_1)]/X^2(H_2). \quad (45)$$

From Table 6, we see that this coefficient equals .94 for Table 1's data.

We also noted in the preceding section that the statistic $X^2(H_3) - X^2(H_1)$ could be used to test whether the parameter γ^{A_1} in H_1 contributed in a statistically significant way. As in the preceding paragraph, we shall measure this contribution's magnitude by the following coefficient of partial determination: ²⁸

$$r^2_{\omega \gamma^{A_1} \cdot H_1} = [X^2(H_3) - X^2(H_1)]/X^2(H_3). \quad (46)$$

From Table 6, we see that this coefficient equals .99 for Table 1's data.

To measure how well model H_1 fits the data, we consider the following coefficient, which we call the coefficient of multiple determination between ω and the γ parameters in model H_1 :

each γ parameter; e.g., γ^{BC}_{jk} in (10) became $\gamma^{BC\bar{D}}_{jk}$. In the present section, we have no need for this more cumbersome notation and will not include the letter $\bar{D}$ in the superscript. The reader should, of course, keep in mind that, say, $\gamma^{BC\bar{D}}_{jk}$ here has the same meaning as $\gamma^{BC\bar{D}}_{jk}$ earlier, and that the various γ parameters describe the main and interaction effects on the odds pertaining to variable D .

²⁷ In the subscript of r^2 in (45), we changed the γ^{BC} notation to the γ^{BC} notation because of typographical considerations. This simple notational change should not confuse the reader. Similar notational changes will be made in other formulas in this section.

Since model H_1 includes the γ parameters γ , γ^{A_1} , γ^{B_1} , γ^{C_1} , γ^{BC}_{jk} , we could let

$$r^2_{\omega \gamma^{BC} \cdot \gamma \cdot \gamma^{A_1} \cdot \gamma^{B_1} \cdot \gamma^{C_1}}$$

denote the coefficient defined by (45). To test whether this coefficient differs significantly from zero, we use the statistic $X^2(H_2) - X^2(H_1)$ as noted earlier.

²⁸ Remarks like those in the second paragraph of footnote 27 can be applied to the coefficients defined by (46)–(49). For example, for (46), we could let

$$r^2_{\omega \gamma^{A_1} \cdot \gamma \cdot \gamma^{B_1} \cdot \gamma^{C_1} \cdot \gamma^{BC}}$$

denote this coefficient, and we could assess the statistical significance of this coefficient using the statistic $X^2(H_3) - X^2(H_1)$ noted earlier.

$$R^2_{\omega \cdot H_1} = [X^2(H_7) - X^2(H_1)]/X^2(H_7). \quad (47)$$

From Table 6, we see that this coefficient equals 1.00 (to two decimal places) for Table 1's data.

We might also consider the following coefficient, which we shall call the coefficient of multiple-partial determination between ω and the parameters γ^{A_1} and γ^{BC}_{jk} in model H_1 , when H_1 's other γ parameters (viz., γ , γ^{B_1} , γ^{C_1}) are taken into account.

$$R^2_{\omega (\gamma^{A_1}, \gamma^{BC}) \cdot \gamma, \gamma^{B_1}, \gamma^{C_1}} = [X^2(H_4) - X^2(H_1)]/X^2(H_4). \quad (48)$$

From Table 6 we see that the coefficient equals .99 (to two decimal places) for Table 1's data. Similarly, we can measure the contribution of γ^{B_1} and γ^{BC}_{jk} (using H_5 rather than H_4 in (48)) or the contribution of γ^{C_1} and γ^{BC}_{jk} (using H_6 rather than H_4 in (48)).

We can also use the above coefficients to measure the magnitude of the contribution made by the parameters in other models in Table 6. For example, to measure the magnitude of γ^{A_1} in model H_8 , we use the following coefficient of partial determination:

$$r^2_{\omega \gamma^{A_1} \cdot H_8} = [X^2(H_1) - X^2(H_8)]/X^2(H_1). \quad (49)$$

From Table 6, we see that this coefficient equals .09 for Table 1's data.

All of the r^2 and the R^2 coefficients given by (45)–(49) above took the general form $R^2 = [X^2(H'') - X^2(H')]/X^2(H'')$, (50) where the γ parameters in model H'' are also included among the γ parameters in model H' . We could also write each coefficient as follows:

$$\sum_{i=1}^2 \sum_{j=1}^2 \sum_{k=1}^2 \sum_{l=1}^2 F'_{ijkl} \log [F'_{ijkl}/F''_{ijkl}] \quad (51)$$

$$\sum_{i=1}^2 \sum_{j=1}^2 \sum_{k=1}^2 \sum_{l=1}^2 f_{ijkl} \log [f_{ijkl}/F''_{ijkl}]$$

where F'_{ijkl} and F''_{ijkl} denote the expected frequencies estimated under model H' and model H'' , respectively. The expression of the coefficient in the form (51) is somewhat analogous to the expression of the coefficients of multiple and partial determination, in the usual multiple regression analysis, as

a ratio of the "explained variation" (when model H' is used to "explain" the variation that was not explained by H") to the "unexplained variation" (when model H" is used).²⁹

COMMENTS ON SOME RELATED WORK

As we noted earlier, Coleman's model and methods differ from ours in several ways. His model does not fit the data as well, and his explanation of the data is less parsimonious. Indeed, he observes in his book (1964) that his model differs from the actual data in certain systematic ways, attributing these deviations to a supposed interaction for Negroes (but not whites) between their region of origin and present camp location.³⁰ In contrast, we find that (a) model (10) in the present article fits the data very well, (b) it does not require an ostensible interaction for Negroes (but not whites) of the kind considered by Coleman, (c) it includes an interaction effect γ^{BO}_{jk} between region of origin and present camp location, which applies equally to both Negroes and whites, (d) this interaction effect is statistically significant, and (e) it both reduces the expected odds favoring a preference for a Northern camp for those whose region of origin is the same as their present camp location, and increases these expected odds for those whose region of origin differs from their present camp location, after the various main effects in the model have been taken into account.³¹

²⁹ When H" is taken as H₇ of Table 6, then the denominator in (51) (i.e., the "unexplained variation" when model H₇ is used) corresponds to the "total variation" in the denominator of the usual coefficient of multiple determination in multiple regression analysis. For related matters, see Goodman (1970).

³⁰ Coleman (1964) did not provide methods for including interaction effects in his models, and so could not measure the magnitude of the ostensible interaction to which he referred, nor could he judge whether introducing the ostensible interaction would improve the fit of his model.

³¹ For further details, see footnote 5. In addition to the effect of γ^{BO}_{jk} described there, the effect can also be described as follows: For the estimate of the expected odds Ω^{B}_{1jk} in favor of a Northern camp, the effect on the estimated Ω^{B}_{1jk} of being at present in a Northern rather than a Southern camp is less for those from the North than from the South. Similarly, the effect on the estimated Ω^{B}_{1jk} of being

Coleman's article did not show how to test whether his model fit the actual data, nor was he able to measure how well it fit. Furthermore, he did not show how to test the statistical significance of the contribution made by the various parameters in the model, nor could he measure their contribution's magnitude. In addition, the variance of Coleman's estimates of the main effects in his model was larger than it would have been had he used more efficient estimation methods (e.g., maximum-likelihood estimation methods); and his estimates are biased to the extent that his model excluded relevant interaction effects.³²

Coleman's model states that the effects on the expected proportions P_{ijk} are linear.³³ Applying Coleman's estimation methods to his model, it is possible to obtain clearly incorrect estimates of the P_{ijk} under the model; e.g., estimates of the expected *proportions* P_{ijk} that are negative or larger than one.³⁴ Furthermore, his model and methods do not take into account the fact that the variance of the observed proportion p_{ijk} will depend on the magnitude of P_{ijk} .³⁵

Some of the limitations of Coleman's approach apply to the usual multiple regression model (and analysis of variance model) if used in the present context. For data of the kind considered in the present article, the assumption of homoscedasticity made in the usual multiple regression model (and in

a Northerner rather than a Southerner is less for those presently in a Northern rather than a Southern camp.

³² The remarks above apply to Coleman's (1964) and Boudon's (1968) articles, except that Boudon's model did allow for interaction effects.

³³ Recall that $P_{ijk} = F_{ijk}/n_{ijk}$, using our notation. In contrast to Coleman's, our model states that the expected odds Ω_{ijk} can be expressed in terms of multiplicative effects. In many substantive contexts, it will be more useful to consider multiplicative rather than additive effects. Furthermore, from the point of view of statistical theory, there are a number of reasons for preferring multiplicative models of our kind for analyzing data of the kind presented in Table 1. We shall not pursue these matters further here.

³⁴ Since P_{ijk} denotes an expected *proportion*, it should not be negative nor larger than one. Therefore, it is undesirable to use models and methods that can lead to *estimates* of the P_{ijk} that are negative or that are larger than one.

³⁵ In other words, Coleman implicitly assumes homoscedasticity when, on the contrary, his data violate this assumption.

the usual analysis of variance model) would be contradicted in a way that could not be ignored. In addition, as with Coleman's analysis, if one applied the usual multiple regression methods to the model in which the effects on the expected proportions P_{ijk} are linear, one could obtain clearly incorrect estimates of the P_{ijk} under the model, in the sense described above.³⁶

We noted earlier that our data were also analyzed by Zeisel (1968) and Theil (1970). Zeisel described various interesting features of these data. These features can be explained, in a more comprehensive and compact way, in terms of the estimated parameters in model (10) of the present article. For example, from the estimates for model (10) presented in Table 3 herein, we find that the estimated product of the parameters γ and γ^{BO}_{11} is approximately one (more precisely, this product is 1.13), and this single fact can be used to explain the following features of the data: (a) the preference for a Northern camp location among Negro Northerners in Northern camps is approximately equal to the preference for a Southern camp location among white Southerners in Southern camps; and (b) the preference for a Northern camp location among white Northerners in Northern camps is approximately equal to the preference for a Southern camp location among Negro Southerners in Southern camps. (In order to see that this single fact explains features (a) and (b), insert the estimated values of the parameters in model (10).) The other interesting features noted by Zeisel can also be explained in similar terms, with one exception. This exception pertains to Zeisel's mention of a supposed effect on camp preference due to the interaction between race and region of origin, among those in Northern camps. Applying the methods of the present article, we find that this ostensible interaction effect is not statistically significant, and there is no need to include it in our model (10).

We comment next on the article by Theil (1970). He used the logit model corresponding to (20), but his estimation method and his analysis differed from ours. Theil (1970) used a weighted

least-squares procedure, as did Grizzle, Starmer, and Koch (1969) in the same context; whereas, all the estimates presented in the present article are maximum-likelihood estimates. In commenting on the weighted least-squares procedure, the Grizzle-Starmer-Koch article notes that estimates obtained by their procedure have a somewhat larger variance than maximum-likelihood estimates (see also Rao 1965); similarly Theil's estimates have a somewhat larger variance than our maximum-likelihood estimates. We also find that it is harder to use the methods proposed by Theil, and by Grizzle, Starmer, and Koch than the methods proposed in the present article, when studying the kinds of hypotheses we have discussed for the four-way contingency table (or in Goodman 1970, for the five-way table).³⁷

Before closing, we remind the reader that our methods were for the case where a given variable (e.g., variable D) can be viewed as the dependent variable which is affected by the other variables under consideration. Where this is not the case, we refer the reader to the more general techniques presented in, for example, Goodman (1970, 1972).

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³⁶ The comments in footnotes 33 and 34 are relevant here.

³⁷ For further details, see Goodman 1971a.

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INDUSTRIAL CONFLICT AND UNIONIZATION *

DAVID BRITT
Assistant Professor
Florida Atlantic University

AND

OMER R. GALLE
Associate Professor
Vanderbilt University

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People who analyze industrial conflict from strike activity usually base their study on one overall measure of conflict, such as Kerr and Siegel's measure of strike propensity. We suggest it might be easier to identify several components of strike activity rather than one. We identify a composite variable, volume of conflict, which resembles Kerr and Siegel's Strike Propensity, and demonstrate the mathematical relationship to its components: proneness to conflict, extensity of conflict, and intensity of conflict. We analyze the relative contributions of each component to the composite variable, and conclude that extensity of conflict and proneness to conflict exert strong influences on volume of conflict, while intensity of conflict is weaker in impact. We then introduce two union variables—degree of unionization and average union size—and speculate on their effect on the various dimensions of industrial conflict. We propose a tentative model which explains the unionization variables' difference in impact in terms of external support, threat potential, and factionalism.

INDUSTRIAL conflict calls to mind a wide variety of phenomena embracing vertical, horizontal, individual and collective forms. Dahrendorf (1959:236-40) treats in industrial conflict as vertical class conflict between labor, the subject class, and management, the dominant class. Other authors note the relevance of more truncated authority distributions to variations in the degree of industrial conflict. Lopreato (1968), in test-

ing Dahrendorf's thesis, notes great conflict within the dominant class between greater and lesser authorities. Similarly, Michels (1962:33-56) and others describe conflicts between authority levels in unions. Where authority is split among parallel hierarchies, industrial conflict is described as more nearly horizontal: Dalton (1959), focuses on line staff conflicts, Harvey and Mills (1970:181-213) and Lawrence and Lorsch (1967) refer to more general sub-unit conflict over the resource and power allocation. Finally, Udy (1967:678-709) delineates variables which channel anger modes into individual or collective forms.

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This paper focuses on one aspect of vertical conflict: overt union-management conflict in work stoppages. Part one presents components of industrial conflict. Part two describes component logical and empirical interrelationships. Part three introduces additional variables to account for both component relationships and the variance in each component.

Components of Industrial Conflict

Most of the literature on strike activity variations in industry has focused on a single index of one sort or another: gross strike frequency, or number of man-days lost adjusted by work force size. Consequently, the development and testing of hypotheses on interindustry variations in strike activity have been relatively crude. Perhaps the most well-known study is by Kerr and Siegel (1954). They suggest several industrial-environmental variables that might affect strike activity variations,¹ but can relate only two very general hypotheses to variations in their index. These hypotheses treat the location of the worker in society, and the character of the job and the worker:

Industries will be relatively strike-prone when the workers (i) form a relatively homogeneous group which (ii) is unusually isolated from the general community and which (iii) is capable of cohesion . . . ;

and

If the job is physically difficult or unpleasant, unskilled or semi-skilled, and casual or seasonal, and fosters an independent spirit (as in the logger in the woods), it will draw tough, inconstant, combative and virile workers, and they will be inclined to strike (Kerr and Siegel, 1954:195).

These complex hypotheses may provide a "satisfying approximation to causal diagnosis" (Williams, 1969) since the clusters can roughly discriminate between high and

low strike-prone industries and since the authors could not test a number of more interesting relationships. Within these clusters, however, are several discrete variables which may vary independently across situations and be differently related to aspects of strike activity. For example, seriousness of industrial hazards, quality of living conditions, nontransferability of skills, degree of upward mobility within an industry, and number of voluntary associations within the community, may all be relevant to analyzing industrial conflict; yet their effects are not so similar that one could collapse them into a cluster without covering up interesting information. Where such independent variation is possible, a much better strategy would be to evaluate alternative indices of the phenomena of interest.

Kerr and Siegel's (1954) single index of strike activity, which they call "strike propensity," is based on the number of man-days lost from strikes and walkouts. They classified industries into one of five categories according to their scores on two different rankings: (1) number of man-days lost from strikes and walkouts; and (2) volume of employment. Using these rankings, they could adjust the number of idle man-days by the number of workers in an industry. This standardization enabled them to compare more meaningfully industrial categories with different numbers of workers. They placed an industry in a high category if the man-days lost rank was substantially above the employment volume rank, in a medium high category if the man-days lost rank was significantly above the employment rank. The other three categories: medium, medium low, and low, were similarly defined in general terms. Such crude rank-comparison definitions were forced on the authors by the different kinds and quality of data available from the countries they examined.

The present study uses only data for the United States from 1958 to 1962. With these restrictions on time and place, we can construct indices of strike activity. We shall define volume of conflict as the number of man-days lost from strikes and walkouts per 1,000 workers in the specified industrial category. This measure approximates the sense of Kerr-Siegel's measure of strike propensity, but with one advantage. Since both

¹ They make explicit reference to the following:

- a) industry sensitivity to the business cycle,
- b) the product market structure,
- c) the product elasticity of demand,
- d) labor as per cent of the total cost,
- e) the industry profitability,
- f) the average plant size,
- g) the state of technological change,
- h) the rate of industry expansion or contraction (Kerr and Siegel, 1954:196).

the numerator and the denominator are available from published sources, the Kerr-Siegel measure which took only five values (an ordinal scale) is now transformed into a continuous (ratio scale) variable. This change permits more sophisticated statistical analysis.

If we could isolate meaningful components of strike activity, we might make more discriminate predictions. We could use for this purpose the several measures of industrial conflict from strikes and walkouts published by the Bureau of Labor Statistics (see, for example, U. S. Department of Labor, 1962). They publish the number of man-days lost from strikes and lockouts, and the estimated number of workers by industry (both of which are used in the above volume of conflict measure), as well as the number of work stops per year for an industry, and the number of workers involved. From these four items, we can generate at least four measures of industrial conflict. For a given industry, and a specific time interval, let:

WKRS = the number of workers employed in the industry;

WS = the number of work stops from strikes, walkouts, and lock-outs;

WI = the number of workers involved in work stops;

MDI = the number of man-days idle from work stops.

Then, after Galle and Mariolis (1969), we can define the following indices of industrial conflict:

volume of conflict = $(MDI/WKRS)$
(1000);

prone to conflict = (WS) ;

extensity of conflict = $(WI/WS/WKRS)$
(1000);

intensity of conflict = (MDI/WI) .

Note that volume of conflict is a composite of the other three conflict measures, being a multiplicative function of them. That is,

$$\left(\begin{array}{c} \text{volume of} \\ \text{conflict} \end{array} \right) = \left(\begin{array}{c} \text{prone to} \\ \text{conflict} \end{array} \right) \left(\begin{array}{c} \text{extensity of} \\ \text{conflict} \end{array} \right) \left(\begin{array}{c} \text{intensity of} \\ \text{conflict} \end{array} \right);$$

or

$$\left(\frac{MDI}{WKRS} \right) (1000) = (WS) (1000) \left(\frac{WI/WS}{WKRS} \right) \left(\frac{MDI}{WI} \right).$$

Aside from the four indices' mathematical relationship, each may be interpreted differently.² Volume of conflict contains information on the total number of man-days lost from strikes, walkouts, and lockouts per thousand workers for each industrial category. We have noted this index's similarity to Kerr's and Siegel's (1954), but more significant is the summary impression of industrial conflict that the measure appears to give. It suggests the scale of strike activity, but loses strike activity detail and thus forfeits many interesting and important questions. Was the man-days lost total due to frequent strikes? How long did the strikes last? Were the strikes less frequent but larger in numbers of people? These questions may be more fruitfully approached through other indices.

Proneness to conflict, defined as the annual average number of work stops, isolates a different aspect of industrial conflict from volume of conflict, and yet they are related. The more strike prone an industry, the greater the number of man-days idle lost from strikes, if all other related variables are random in impact. Highly strike-prone industries might be those in which overt conflict has been fostered (cf., Gurr, 1968, 1970) by union factionalism and jurisdictional disputes, union suppression of minority groups, or excessive grievances or alienation (Blauner, 1964). In addition, if some industries' wage agreements are relatively short-term or if strikes have been a relatively successful strategy in forcing higher wage settlements, we should find some across industry variation in proneness to conflict. And following Kerr and Siegel (1954), if some industry jobs are characteristically unskilled and seasonal in their requirements, while others are skilled and steady, then industries should vary considerably in proneness to conflict, with

² The original component definition and isolation comes from Galle and Mariolis (1969).

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those requiring unskilled labor being more strike prone. Any or all of these factors might both increase the number of idle man-days and more specifically encourage frequent strikes.

The extensity of conflict index is defined as the number of workers involved per work stop per 1,000 workers in an industry. Clearly, the greater the numbers of workers in work-stops per industry, the greater the number of idle man-days. Extensity denotes numbers participating rather than frequency of participation; hence, it is potentially independent of proneness and may be explained by different sets of variables. The larger the number of unionized workers by industry, the greater the union's ability to mobilize workers for strikes. With unionization levels by industry constant, different rates of technological change or other economic traumas which threaten to displace whole segments of the labor force and create general job insecurity could cause the numbers of workers involved in strikes to vary. Or following Kerr and Siegel (1954), in industries where workers are isolated, homogeneous, and cohesive, or where workers are concentrated in large plants (Blauner, 1964), there may well be extensive conflicts. And of course, if unions or communities can provide adequate external support (cf. Gurr, 1970) for striking workers in money, food, or clothing (and these supports are more common in some industries than others), then we should expect cross-industry variation in extensity of conflict and hence numbers of man-days idle.

Our last measure of strike activity is intensity of conflict, defined as the number of man-days idle per worker in work stops. Once work stops occur, how serious are they; how long do they last? Obviously, the longer they last, the more the idle man-days in that industrial category; but again we have a conceptually independent strike activity index that might be explained by different combinations of variables than those used in other indices. Industrial categories characterized by industries with low profit margins whose unions make costly demands may have long strikes, especially where the union is relatively large and can afford a long strike or where the workers are committed to the issues. On the other hand, if a union has most of its membership in one industrial

category, it may depend on that particular industry for its existence: such a situation could result in shorter strikes. In general, disputes over new contract terms in the past (1947-1959, for example) have been longer than disputes in which contract terms were not an issue (U. S. Department of Labor, 1960b).

Analyzing Component Interrelationships

The preceding discussion suggests that a given volume of conflict may be achieved by a wide variety of component score combinations. Table 1 exhibits for eighteen broad industrial categories five-year-average measures of these four components of strike activity.³ To see how the components may vary, compare the category of contract construction with the category of transportation equipment. Although the two have quite high volumes of conflict, you can see that proneness to conflict was over six times as high for construction as for transportation equipment. This factor is counter-balanced, however, by the extremely high extensity of conflict index in the transportation equipment category. Hence, while these two industrial categories might be rated as relatively equal in volume of conflict, their scores on other industrial conflict dimensions reflect considerably different patterns.

We could point out other examples, but the component definitions enable us to systematically investigate strike activity index relationships. The strike activity indices might vary independently and all might contribute to the number of man-days idle per 1,000 workers (volume of conflict). Our argument assumes that changes in proneness, extensity, and intensity of conflict levels affect changes in the volume of conflict level, but not vice versa. We do not argue that proneness to conflict causes changes in volume of conflict, but that volume is a composite variable made from the other three.

Following Duncan (1966), we analyze the

³ We retain the eighteen broad industrial categories here for uniformity later when we relate industrial conflict to unionization measures. That is, strike propensity and conflict proneness, extensity and intensity can be collected for over one hundred more detailed industry categories, but the information on unionization is available for only the eighteen.

Table 1. The Dimensions of Industrial Conflict for Eighteen Broad Industrial Categories: United States, 1958-1962

Industrial Category	Proneness* to Conflict	Extensivity* of Conflict	Intensity* of Conflict	Volume* of Conflict
<u>Manufacturing</u>				
Food, beverages & tobacco	184	.200	12	454
Clothing, textiles, & leather products	189	.133	8	208
Furniture, lumber, wood products, & paper	199	.129	20	520
Printing & publishing	49	.477	16	365
Petroleum, chemicals, & rubber	168	.274	20	936
Stone, clay, & glass	125	.425	22	1181
Metals, machinery & rel. eqpt., expt. transpt. eqpt.	692	.123	29	2466
Transportation eqpt.	128	1.202	11	1688
Manufacturing (not classified)	486	.822	7	486
<u>Nonmanufacturing</u>				
Mining & quarrying	164	.513	27	2259
Contract construction	825	.114	16	1466
Transportation	227	.270	12	722
Telephone & telegraph	14	.702	5	49
Electric & gas utilities	21	.886	15	276
Trade	326	.014	17	75
Finance & insurance	8	.85	4	3
Service industries	118	.015	15	27
Govt.: fed., state, local	26	.064	2	4

volume of conflict decomposition by taking the logarithms of the four indices, thus adding instead of multiplying. If we then express each logged variable in standard form, letting

$$\begin{aligned} X_0 &= \log_e VC \text{ (Volume of conflict)} \\ X_1 &= \log_e PC \text{ (proneness to conflict)} \\ X_2 &= \log_e EC \text{ (extensivity of conflict)} \\ X_3 &= \log_e IC \text{ (intensity of conflict)} \end{aligned}$$

we obtain:

$$Z_0 = P_{01}Z_1 + P_{02}Z_2 + P_{03}Z_3,$$

where $Z_0, \dots, Z_3$ are the variables $X_0, \dots, X_3$ in standard form, and P_{01}, P_{02}, P_{03} are the standardized regression coefficients (path coefficients) which determine Z_0 by Z_1, Z_2 , and Z_3 .

To determine component index gross effects on the composite index, we may examine their intercorrelations. Intensity of conflict ($r_{03} = .79$) appears the most important

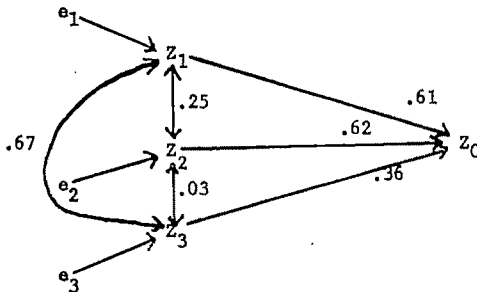
of the three, with proneness ($r_{01} = .70$) and extensivity ($r_{02} = .48$) following in that order. But the ordering is not clear, especially with only eighteen cases. For a better idea of each component index contribution (Figure 1) we may split the correlations, which represent the total effect, into their direct and indirect effects using path analytic techniques (Duncan, 1966).⁴

Proneness has a strong direct effect on volume ($P_{01} = .61$). Since industries with frequent strikes tend also to have relatively long-lasting strikes ($r_{13} = .67$), proneness has an indirect effect of .21 through intensity. At the same time, highly strike-prone industries

⁴ An independent variable's *direct effect* is its unmediated effect, apart from the effects of the other independent variables in the model. Its indirect effect is the mediated part of the total effect, that which operates through other variables in the model. Since the model is definitional rather than causal, the direct effect is a component's unmediated contribution, the indirect its mediated.

FIGURE 1

DECOMPOSITION MODEL *

* Z_1 = log Prone-ness to Conflict Z_2 = log Extens-ity of Conflict Z_3 = log Intens-ity of Conflict Z_0 = log Volume of Conflict

tend to have smaller numbers of people in each strike ($r_{12} = -.25$), a fact which yields a negative indirect effect of $-.14$ through extensity. The total indirect effect of prone-ness, then, is a meager $.08$ (Table 2).

The direct effect of intensity on volume is much smaller than that of prone-ness ($.36$ versus $.61$); yet because intensity is positively associated with both extensity and prone-ness, its total effect on volume is quite large ($r_{03} = .79$). The indirect are relatively more important than the direct effects of intensity ($.43$ versus $.36$).

The reverse is true of the relationship between extensity and volume. Strikes involving larger numbers of people had a relatively strong direct effect on the number of idle man-days' variance ($p_{02} = .62$), for the 1958-62 period and the population studied. Yet, this strong direct effect is masked by a total negative indirect effect generated by exten-sity's strong negative association with prone-ness and slight positive association with inten-sity.

Calculating direct and indirect effects greatly increases the information available for assessing the relative contribution of each component to the composite index. A comparison based solely on correlation co-efficients suggests that intensity is the most

important, followed by prone-ness and exten-sity. A look at path coefficients, however, leads us to somewhat different conclusions: extensity ($p_{02} = .62$) and prone-ness ($p_{01} = .61$) are much stronger than intensity ($p_{03} = .36$) in their direct effects. Thus, more interindustry variation seems to be associ-ated with variables that generate either fre-quent or broad strikes than variables that generate long strikes. Only by virtue of its indirect effects through extensity and prone-ness does intensity correlate the strongest with volume. But we should not infer that the indirect effects are unimportant or non-problematic. We assume from our definitions that the components' relationship is spuri-ous. Hence, we must add variables to the analysis to explain not only each component variance but their interrelationship.

Unionization and Conflict Components

In the previous section we have decom-posed a summary index of industrial con-flict into three components. Specifying the relative component contribution to the over-all index is interesting in itself, but the breakdown offers far greater analytical possi-bilities. Initially, we criticized the use of a single industrial conflict index on grounds that it might mask interesting variation; i.e., several independent variables might be dif-ferently related to the components of the overall index. Now we will introduce some additional variables, that are exogenous to the simple decomposition model of Figure 1. By doing so, we hope to explain the varia-tion in each component and component intercorrelations.

First, let's examine the effects of varia-tions in unionization patterns on industrial

Table 2. Representation of the Total, Direct and Indirect Effects of the Conflict Components on Volume of Conflict

Component of Conflict	Total Effect (Zero-order Correlation)	Direct Effect	In-Direct Effect
Proneness	.70	.61	.09
Extensity	.48	.62	-.14
Intensity	.79	.36	-.43

conflict components. We can easily construct at least two indices related to unionization from information available in the *Labor Statistics Bulletins*:

$$\begin{aligned}\text{average union size} &= \frac{\# \text{ of union members}}{\# \text{ of unions}} \\ \text{degree of unionization} &= \frac{\# \text{ of union members}}{\# \text{ of workers}}\end{aligned}$$

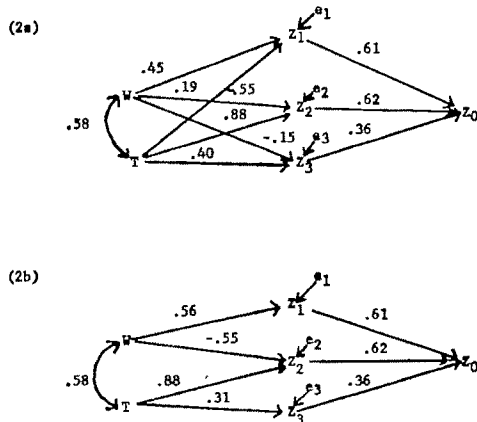
Predicting relationships between these variables and the various component indices is more difficult. The average union size and degree of unionization and the components could be either positively or negatively related, depending on the causal-structural links one chooses. Suppose extensity of conflict was determined by the ability to mobilize large numbers of workers. Then we might predict positive relationships with extensity for both average union size and degree of unionization, since both dimensions should be related to worker mobilization potential. The case would differ were we to choose degree of worker control at the local level. Then extensity might be positively related to degree of unionization and negatively related to average union size, and we could argue for a span-of-control interpretation. Similarly, our predictions might vary if we chose the probability of jurisdictional squabbles, commonality of grievances, or threat potential as our main link.⁵ Clearly, there is no established, integrated theory of union-management conflict from which these relationships could be deduced. The complexity of the causal structure which may operate here is hinted at by the partly offsetting direct and indirect effects found among the industrial conflict components in the decomposition model. We have, however, a partial theory in that we can hierarchically order two subsets of variables: average union size and degree of unionization are exogenous; conflict proneness, extensity, intensity, and of course volume are, by definition, endogenous. Thus, we can eliminate somewhat some paths by ordering, and others by defining the endogenous variables' interrelationships. Any relationship among prone-

ness, extensity, and intensity must, by definition, be spurious. Further, exogenous variables are assumed to effect volume only through its components. These constraints remove many potential models from consideration, and make it profitable to evaluate the impact of unionization on industrial conflict with a strategy of theory trimming (Heise, 1969).

We begin by examining the least parsimonious model, Figure 2a. This model is not fully identified; paths have been eliminated by the constraints imposed by our definitions. It does, however, specify all paths possible, given our definitions, between the exogenous and endogenous variables. We arrive at Figure 2b by dropping the paths between W (average union size) and Z^3 (intensity of conflict), and between T (degree of unionization) and Z_1 (proneness to conflict).

We see at once that we are dealing with a complex causal structure. Average union size and degree of unionization have positive direct effects on proneness, but contrasting effects on extensity (the direct effect of average union size = -.55; the direct effect of degree of unionization = .88). In the latter

FIGURE 2
IDENTIFIED AND SIMPLIFIED MODELS
RELATING THE UNIONIZATION AND CONFLICT VARIABLES *



* W = Average Union Size

T = Degree of Unionization

Z_1 = log Proneness to Conflict

Z_2 = log Extensity of Conflict

Z_3 = log Intensity of Conflict

Z_0 = log Volume of Conflict

⁵ Some information on the prevalence of jurisdictional disputes is available in the Bureau of Labor Statistics *Bulletins*. Unfortunately, this is not broken down either by the unionization variables or industrial conflict components.

Table 3. Original and Regenerated Correlation Matrices*

Variable	W	T	Z ₁	Z ₂	Z ₃	Z ₀
W (average union size)	1.00					
T (degree of unionization)	.58 (.58)	1.00				
Z ₁ (proneess to conflict, logged)	.56 (.56)	.45 (.33)	1.00			
Z ₂ (extensity of conflict, logged)	-.04 (-.04)	.56 (.56)	-.24 (-.02)	1.00		
Z ₃ (intensity of conflict, logged)	.08 (.18)	.31 (.31)	.67 (.10)	.11 (.17)	1.00	
Z ₀ (volume of conflict, logged)	.35 (.38)	.72 (.66)	.69 (.63)	.50 (.67)	.81 (.53)	1.00

*The regenerated correlations are parenthesized. The regenerated correlations assume the relationships shown in Figure 2b.

*The figures in the table derive from the following operations: where,
 WKRS = number of workers employed in the industry (in thousands);
 WS = number of work stoppages from strikes, walkouts, and lockouts;
 WI = number of workers involved in work stoppages;
 MDI = number of man-days idle from work stoppages.
 then,
 volume of conflict = $(MDI/1,000(WKRS))$;
 proneess of conflict = (WS) ;
 extensity of conflict = $((WI)/(WS))/1,000(WKRS)$;
 intensity of conflict = $(MDI)/(WI)$.

case, a simple correlational analysis would have misled us. The correlation between degree of unionization and extensity of conflict is .56; yet its direct effect is much greater (.88), though partly offset by its association (.58) with average union size which creates an indirect effect of -.32.

One way to assess the adequacy of our exogenous variables is to examine the amount of variance in each component that we have introduced. We have explained 51% of the extensity variance, 10% of the intensity variance, and 31% of the proneess variance. While we have not accounted for the total variance in any component, we have explained enough variance to suggest that our exogenous variables can suitably predict variation in aspects of strike activity.

As important as explaining the amount of variance (cf. Duncan, 1966, 1970) is assessing the degree to which our exogenous variables can account for component intercorrelations. In a decomposition analysis, the problem is clear, for by definition we are not specifying asymmetric, causal relationships among the components: any component intercorrelation is assumed spurious.

By comparing the zero-order correlations' matrix with the correlation matrix regenerated from paths specified in Figure 2b, we can see how well the unionization variables account for the conflict components' interrelationships. The unionization variables, following model 2b, are able to generate correlation coefficients similar to the weak relationships between intensity and extensity, and extensity and proneess. We find, however, one glaring inconsistency: the difference between the observed (.67) and predicted (.10) values for the intensity-proneess association is quite large. One can only conclude that the model does not include the variables which account for the intensity-proneess relationship.

Note also how dropping the paths from Figures 2a and 2b has affected the relationship between the conflict components and the overall index. Without some of its indirect effects, intensity has dropped from .81 to .50. While extensity, some of whose indirect effects were negative, has increased from .50 to .66. Average union size explains approximately 12% of the volume variance, most of which is due to its indirect effects through degree of unionization. Degree of

unionization explains approximately 41% of the volume variance, primarily through its effects on extensity and intensity.

Discussion

Both average union size and degree of unionization represent dimensions of power (cf. Levinson, 1966) in industrial categories. Our simplified model for eighteen categories suggests, however, that these two variables are differently related to the components of industrial conflict. A cursory glance at the model suggests that we might explain the conflict components by different sets of antecedent conditions, a view which supports our initial assumption that more information comes from looking at multiple industrial conflict indices in combination than from analyzing any one index.

Of the two unionization variables, only average union size seems related to proneness, directly accounting for 31% of the variance. One might expect that larger unions mean fewer strikes, since to maintain credibility at the bargaining table unions must be able to control workers. If this argument were valid, average union size and proneness should have an inverse relationship. That we have isolated a moderate, positive effect suggests that while such pressures for selective strike use may exist, factors associated with average union size work in opposition. Kerr (1964) suggests that larger unions have greater member heterogeneity of interest and greater factionalism than smaller unions, features which create a lack of fit between industry settlements and plant-level issues. Such conditions might lead to wildcat strikes, thus generating a positive relationship between average union size and proneness.

However, there might be reasonable alternatives to this argument. Since larger unions do affect the power position of workers in an industry—if only in that they have greater economic resources, workers may feel more job security as union size increases. Where grievances are present, proneness to conflict would then be positively related to average union size. It may be also that assuming unions want industrial peace to preserve their credibility relates to only one of a strike's possible functions. A little conflict now and again may be an excellent union

strategy for increasing its size or buttressing morale,⁶ as well as reminding management of its power to mount larger strikes. Average union size could also be associated with another exogenous variable which itself is setting the conditions for frequent strikes. If, for example, larger unions are associated with larger plants, and larger plants foster lower morale or higher grievance rates, then we would also expect the observed positive association.

A different pattern emerges for intensity of conflict. Average union size appears to effect intensity negligibly (we dropped this path from the initial model); whereas, degree of unionization exerts a moderately strong direct effect ($p_3 = .31$). Where no external support in food, money, shelter, clothes, or organization are provided, strikes of long duration could not be tolerated by any industrial workers. Since unions can supply such support and organization, the potential for longer strikes is greater. This argument suggests a positive relation between intensity and both average union size and degree of unionization. More external support exists with unions than without. Similarly, more external support exists in larger unions than smaller, since the membership that can be assessed is larger.

What is actually happening seems more complex since only degree of unionization is related positively to intensity of conflict. It may be, however, that as a union's size increases, its potential for mobilizing large numbers of workers increases. Consequently, the union's threat potential increases with its increased ability to force a total production shutdown. This argument is similar to one formulated to explain the relative success of unionization in higher-skilled occupations. Since skilled workers are harder to replace and therefore more essential to production than unskilled workers, their power to shut down a plant by striking is much greater than that of unskilled workers (Reynolds, 1949). With greater threat potential negotiation settlements and strikes should be resolved faster. If threat potential and external support increase at roughly the same rate as average union size increases, average

⁶ Coser (1966) is one of many who have elaborated on this well-established function of conflict between groups.

union size and intensity of conflict would be negligibly related.

Still another relationship exists between the unionization variables and extensity of conflict. The direct effects of both average union size and degree of unionization are strong, but opposing ($p_{2w} = -.55$; $p_{2t} = .88$). Here, we might apply our arguments about the way our unionization variables and intensity of conflict are related. In only rare cases, e.g., the grape strike in California, can large numbers of unorganized workers afford to strike: if they are not unionized the companies can replace them with other non-unionized laborers, especially unskilled ones. A union's capacity to provide external support fosters more extensive conflicts. The same argument applies to increasing average union size, but the larger unions' greater threat potential offsets the external support and creates an overall negative relationship. In addition, with increased control from centralization, a union can stage strikes at specific companies within industries or with specific workers. It may also be able to increase pressure for settling negotiations and cancelling strikes. In the automobile industry, the United Auto Workers typically strike one of the big three at a time. Hence the company being struck is forced to shut down at the very time the other companies are producing new lines.

The unionization variables differ in their ability to explain component intercorrelations. The negligible relationship between intensity and extensity (.11) is not greatly disturbed by introducing degree of unionization as a control (.17). The moderately weak relationship between extensity and proneness (-.24) is adequately explained by introducing average union size as a control (the regenerated coefficient is -.22). With their greater threat potential, larger unions can avoid costly strikes involving large numbers of workers. At the same time, larger unions, either because of factionalism or some other condition, generate frequent strikes. The great discrepancy between the original and reconstituted coefficients of the strong relationship between intensity and proneness (.67 versus .10) indicates that additional variables not included in the model are at work. Several possibilities exist, but one is intriguing. Our model as-

sumes that the amount of discontent is constant across industries (cf. Spilerman, 1971 for a similar assumption about national regions) and that the pattern of unionization fosters certain combinations of conflict forms (frequent and small strikes by large unions, for example). If the amount of discontent varies by industry (cf. Blauener, 1964, for example), and if discontent generates both frequent and long strikes, then introducing structural characteristics conducive to variations in discontent should partly explain the observed, unexplained relationship between intensity and proneness.

In summary, average union size could offset proneness through factionalism, threat potential, or external support, with the precise argument in each case differing. The relationship between average union size and intensity and extensity could be mediated by its relationship to threat potential and external support. Since these latter variables oppositely effect intensity and extensity, they offset one another, one effect being negligible, the other negative. Or degree of unionization may be positively related to both intensity and extensity through its relationship to external support. But again we are speculating. We haven't the information to examine our interpretive constructs further and can only hope our efforts prompt further research.

This paper demonstrates the viability of decomposing the overall measure of industrial conflict (volume of conflict) into theoretically distinct dimensions. Interpretations of unionization and industrial conflict relationships must be viewed as tentative, given the scarcity of industrial categories available for analysis. Our analysis, however, suggests possible paths of future research. With empirical measures of threat potential, factionalism, and external support, and an increased case-base, one could refine and expand our model.

Looking beyond union and industry conflict relationships, one may use our model in several ways, two of which come quickly to mind. First, with several industrial conflict opponents, one could isolate different types of industries for detailed case studies. It is likely that industries which achieve a given level of industrial conflict through high levels of extensity of conflict, will differ

in many theoretically interesting ways from those which achieve similar levels of proneness to conflict (i.e., industries which have many highly localized strikes). The concepts of proneness, extensity, and intensity could also be fruitfully used to study other types of conflict, such as urban racial disturbances.

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THE STRUCTURE OF SCIENTIFIC FIELDS AND THE FUNCTIONING OF UNIVERSITY GRADUATE DEPARTMENTS *

JANICE BEYER LODAHL

AND

GERALD GORDON

Cornell University

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Kuhn's concept of paradigm suggests that academic disciplines could be viewed as technologies involving degrees of task predictability. A series of hypotheses were developed in which relatively high paradigm development in a discipline was predicted to facilitate research and teaching through improved processes of communication and access to stored information. Using questionnaire data collected from 80 university graduate departments, physicists and chemists were found to exhibit more agreement over field content, and to be more willing and satisfied to spend time with graduate students than sociologists and political scientists. In addition, chemists were found to collaborate with larger numbers of graduate students in research than scientists in the other fields. Two other aspects of scientific structure are discussed: the level of innovation being pursued within a field at a given time, and the degree of institutionalized differentiation of the field into subdisciplines.

Two major sociological approaches can be discerned in recent empirical studies of science. The first of these, conceptually related to the work of Parsons (1949, 1951) and Merton (1957, 1968), studies the normative aspects of the reward system of science relative to the system goals of scientific progress and innovation (Zuckerman, 1967; Crane, 1965, 1969; Hagstrom, 1965; Cole and Cole, 1967, 1968). The second approach is that of organizational research, where scientists are studied in terms of organizational structures and processes that hinder or facilitate scientific progress (Pelz and Andrews, 1966; Marcson, 1960; Kornhauser, 1962; Gordon and

Marquis, 1966; Kaplan, 1959). Both approaches have tended to view science in its most general sense, and only rarely have sociologists looked systematically at differences between the various fields of science.¹

Perhaps one reason so little has been done to treat these differences has been the lack of conceptual frameworks around which hypotheses could be developed. One step in the development of a framework to compare scientific fields may be found in Kuhn's (1964) concept of paradigm.

The scientific paradigm, as described by Kuhn, includes not only the accepted theory and findings of the field, but also the preferred methodologies, the tacit understandings over those areas considered important to study, etc. The paradigm provides structure by suggesting which problems next re-

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¹ Storer and Parsons (1968) discussed the differentiation of science into disciplines; Zuckerman and Merton (1971) found differences among disciplines in rejection rates of journals. Roe (1956) studied the psychological differences between scientists in various fields.

quire investigation, what methods are appropriate to their study, and even which findings are indeed "proven." Following from this, the essence of the paradigm concept is the degree of consensus or sharing of beliefs within a scientific field about theory, methodology, techniques, and problems. Kuhn's discussion of the term in the second edition of his book makes this extremely clear: "... it [paradigm] stands for the entire constellation of beliefs, values, techniques, and so on shared by the members of a given [scientific] community" (1970:175). "A paradigm is what the members of a scientific community share, and conversely, a scientific community consists of men who share a paradigm" (1970:176).

These definitions suggest that paradigms² largely determine the technology of scientific fields, not only in the methods that are employed but also in the more general sense of predictability or certainty of technology.³ When scientists in a field have large areas of agreement over both scientific goals and means, there is a high probability of certain courses of action. In a sense those activities are predictable. The efforts to establish priority for research findings in some scientific fields are evidence of predictability in those fields (Merton, 1957; Hagstrom, 1965:69-104).

Kuhn goes on to suggest a continuum of degrees of scientific development in terms of paradigm development. "The members of all scientific communities, including the schools of the 'pre-paradigm' period, share the sorts of elements which I have collectively labeled 'a paradigm'. What changes with the transition to maturity is not the presence of a paradigm but rather its nature" (Kuhn, 1970:179). He states that the social sciences may be in a pre-paradigmatic

stage, while the physical sciences have relatively well developed paradigms (1964:10-15).

This research explores the usefulness of these concepts by studying teaching and research activities within the university. Since the structure of knowledge determines what is taught as well as what is investigated, the degree of paradigm development and accompanying predictability in a given field should affect both teaching and research. Specifically, this study is concerned with demonstrating and discussing some of the differences between scientific fields in the functioning of graduate departments, where the goals of teaching and research and the technology determined by the structure of knowledge are in close interaction.

Let us be quite clear. We, on balance, find Kuhn's argument persuasive. But we do not feel that a conceptual framework should be accepted or rejected simply in terms of the niceties or logic of argument. Rather, we feel that the value of any framework resides in its heuristic value as a research tool. We are interested, first, in determining if we can operationalize the paradigm concept in a manner that will be viable in terms of research, and then, in testing key hypotheses based on the paradigm concept, which, if supported, could provide the framework for increased investigation of differences between scientific fields.

A. Research Design

In exploring the utility of the paradigm concept, we first tested Kuhn's contention that paradigms are more highly developed in the physical than in the social sciences. Scientists in each of four major fields—physics, chemistry, sociology, and political science—were asked to rank the fields in terms of consensus over theory, methodology, and training. In addition, two tests of consensus over scientific content were devised.

In the next phase, hypotheses that the greater consensus of fields with high paradigm development would facilitate teaching and research activities as compared to fields with low paradigm development were tested.

B. Methods

A stratified random sample of 80 univer-

² Kuhn's concept of paradigm has been controversial. Elements of the controversy were summarized recently in Shapere's (1971) review. The sociological nature of Kuhn's analysis is one element that has been criticized. He acknowledges this "fault" as necessary to explain scientific progress (Lakatos and Musgrave, 1970:21, 237).

³ Numerous studies have suggested that the certainty or predictability of technologies has implications for organization structure and behavior (Lawrence and Lorsch, 1967; Burns and Stalker, 1961; Fieldler, 1964; Thompson, 1967; Becker and Gordon, 1966). Our treatment of ideas as technology follows the example of Perrow (1965).

sity departments⁴—twenty each in physics, chemistry, sociology, and political science—was surveyed by means of a questionnaire in the spring of 1968. Although only two levels of paradigm development (high and low) were represented, four fields were chosen so that differences between, and similarities within, paradigm levels could be assessed.

Within departments, all faculty members holding a title of assistant, associate, or full professor, in residence, and with voting membership in the departmental faculty were included in the survey sample.⁵ A total of 1161 responses were received, for an overall response rate of 51%. Physics, the field with the largest number of members, had the lowest response rate of 43% (340 responses). The other fields, chemistry, sociology, and political science had response rates of 57% (296 responses), 58% (260 responses), and 51% (265 responses), respectively. No response bias could be detected by age or rank of respondents, or by comparing numbers of early and late respondents by field or quality level.⁶

The sample does not represent all graduate schools but rather schools of high caliber, as indicated by their appearance in the Cartter Report. In consequence, the faculty surveyed is representative of scientific elite within four selected fields; the sample is further skewed in this direction in that distinguished departments are over-represented in the sample.⁷

⁴ Randomization took place at the departmental, rather than the individual level, because the inter-organizational comparisons made possible by such a design were judged to be of paramount importance to the overall study. Using the published results of the Cartter survey (1966) as a sampling frame, a random sample of 5 departments was drawn within each of the 4 quality levels of the 4 fields, for a total of 20 departments per field. For further details, see Lodahl (1970).

⁵ This was done so that some measurement of departmental structure would be possible with less than an ideal response rate. This sample excludes department chairmen, who were sent a special questionnaire as part of the larger study. It also excludes Cornell University.

⁶ The distributions of age and rank of respondents versus nonrespondents were compared within each field. Data on nonrespondents were obtained from published sources. None of the chi-square values calculated on the resultant distributions were significant (.05 level). For further details, see Lodahl (1970).

⁷ While 5 departments were chosen from each

Because all individuals in a given department were included in the sample, individual observations were not statistically independent. To satisfy the assumption of independence, as required by many statistical techniques, measures were aggregated at the departmental level. They were also weighted by the relative size of the stratum in the field. In addition each department was randomly grouped with three others into a "replication group," with the other departments representing the other three quality strata within the field. For example, one of the replication groups in physics consists of the fifth department drawn in stratum 1, the first of stratum 2, the second of stratum 3, and the first of stratum 4. This method⁸ allowed creation of a new level of randomization, and each of these groups could then be used to obtain a statistically independent measure for the field as a whole.

C. Check on Assumptions of Paradigm Level

The four fields were selected to represent two different levels of paradigm development. We chose physics and chemistry as representing high levels of development, and sociology and political science as representing low levels of paradigm development. It was possible to check on our estimate of levels by asking respondents to rank seven fields (including the four of our study) on their relative degree of paradigm development.⁹ The results are presented in Table 1 and show remarkable agreement on which sciences are more developed and which are less developed. Note that where there

quality stratum in each of the four fields, the quality stratum varied in size from 5 to 22 departments. The distinguished stratum was smallest in every field, with only 5 departments in sociology and a maximum of 9 in physics.

⁸ The authors wish to thank Philip J. McCarthy for suggesting this procedure.

⁹ The question read, "Scientific fields are often said to vary in their degree of development or maturity. Whether or not you agree with all of the implications of this statement, it is probably true that in some scientific fields scientists are more uniform in their scientific practice than in others. This is because their field has a larger body of generally accepted theory and agreed-upon methodologies—paradigms upon which to base their present investigations."

Please rank the following fields on the degree to which you feel there is consensus over paradigms (law, theory, and methodology) within the field (1=most consensus)."

Table 1. Rankings of Selected Scientific Fields on Paradigm Development.

Fields Ranked	Respondent Fields			
	Phys.	Chem.	Soc.	Pol. Sci.
Biology				
Column rank	3	3	3	3
Mean	3.0 ^a	3.0	2.9	2.8
Chemistry				
Column rank	2	2	2	2
Mean	1.7	1.8	1.8	1.7
Economics				
Column rank	4	4	4	4
Mean	4.2	4.5	3.9	3.9
Physics				
Column rank	1	1	1	1
Mean	1.3	1.3	1.4	1.5
Political Science				
Column rank	6	6	7	7
Mean	5.9	5.8	6.5	6.3
Psychology				
Column rank	5	5	5	5
Mean	5.1	4.9	4.8	4.9
Sociology				
Column rank	7	7	6	6
Mean	6.0	6.0	5.8	5.9

^aThe weighted means of rankings by individuals appear below the rank of that mean within the respondent field.

is a difference in overall ranking, both sets of scientists agree with each other, even though it is precisely the two social science fields of the study that are in question. Political scientists agree with sociologists that their field is less developed. In any case, the four fields chosen for investigation clearly represent the extremes of the fields that were ranked.

D. Paradigm and Consensus: Validating the Kuhn Concept

Since paradigm level is defined by the amount of agreement over theory, methodology, etc., we should find indicators of high agreement within fields designated as having high paradigm development, and relatively less agreement in fields designated as having low paradigm development. Such agreement should be evident in the most general aspects of the teaching curriculum, those instances where the objective was to

cover all of the important content of the field. A highly developed paradigm with the associated consensus should thus be reflected in high agreement over the content of survey courses, and also in the requirements and content of graduate programs. In both of these instances, faculty expectancies will be violated (in later courses or in degree exams) if there is a lack of consensus over content. Faculty are thus in a good position to judge how much consensus exists. In these cases, agreement on consensus appears to be a valid indicator of actual consensus.

Test I. High-paradigm fields will report more agreement over content of survey courses than low-paradigm fields. Respondents were asked how much basic or survey undergraduate courses in their field varied in content from their department to others of a comparable quality. The mobility of U.S. faculty, the frequency of visiting residence at other universities, the diffusion of textbooks by publishers, the transfer of students, etc. give faculty some bases for this judgment. Responses could vary from 1 (very little) to 10 (a lot) on a Likert-type scale.

The results are given in Table 2. Two values appear in the table for each replication group. The lower value is the weighted replication group mean. The value above the mean is the rank of the replication group within the entire data matrix. The Kruskal-Wallis (1952) one-way analysis of variance was performed on these ranks, yielding the H statistics given. The differences between the fields are significant at the .025 level. As can be seen from inspection of the sums of the ranks for each field,¹⁰ physics

¹⁰ The formula (devised by P. J. McCarthy) used for computing the .05 level of significance for the difference between two sets of ranks in this analysis was:

$$V(\bar{r}_1 - \bar{r}_2) = V(\bar{r}_1) + V(\bar{r}_2) - 2 \text{Cov}(\bar{r}_1, \bar{r}_2),$$

$$\text{where } \text{Cov}(\bar{r}_1, \bar{r}_2) = \frac{-\sigma^2}{N-1}$$

$$\text{and } V(\bar{r}) = (1 - \frac{n}{N}) \frac{1}{n} \frac{N(N^2-1)}{12}$$

The standard deviation is the square root of the solution. Two times the standard deviation gives the .05 significance level, which for Table 1 is 37.42. Thus, the 95% test value used throughout this paper is 37. Details of the derivations of the formulas and computations are given in Appendix E of Lodahl (1970).

Table 2. Differences Between Fields in Perceived Variation of Undergraduate Survey Course Content (Ten-point scale; 1 = very little, 10 = a lot).

Replication Group		Physics	Chemistry	Sociology	Poli. Sci.
1	Rank Mean	3 ^a 3.5343 ^b	13 5.4087	7 4.2115	15 5.8075
2	Rank Mean	2 3.3986	6 4.0215	12 4.6901	20 6.3774
3	Rank Mean	10 4.2695	18 5.9700	16 5.8591	17 5.9221
4	Rank Mean	1 3.2151	9 4.2485	14 5.4806	19 6.3225
5	Rank Mean	4 3.6550	5 3.7012	8 4.2342	11 4.6523
Sum of Ranks		20	51	57	82
Overall Means		3.6145	4.6700	4.8951	5.8224

(H = 11.13, $p < .025$, 3 df)

^aThe ranking of each replication group mean within the table.

^bThe weighted means. In the cases of scaled items, these are the means of the sums of individual standard scores. In the case of a single question, the means are based on the raw responses.

has much more agreement (low variance) than the other fields in content of survey courses, and political science has much less agreement (high variance). The hypothesis seems at least partially supported.

Test II. On the level of graduate education, high-paradigm fields will report more agreement on course content and requirements for degrees than low-paradigm fields. Respondents were asked three questions: how much agreement there was in their department over requirements for graduate degrees, whether it was hard or easy to agree on these requirements, and whether it was hard or easy to agree on course content. Normalized, summed scale scores based on these three items are given in Table 3, A. In these results the relative positions of the fields are as hypothesized, but political science, unlike sociology, is not significantly different from the two physical science fields.

Given the fact that differences were in the predicted direction, and generally statistically significant, the total patterning of these results tends to support Kuhn's concept of paradigm. Physical scientists reported more agreement in their fields than social scientists.

E. Differences in Behavior and Attitudes

The high consensus found in high-paradigm fields enhances predictability in at least two ways: (1) it provides an accepted and shared vocabulary for discussing the content of the field; and (2) it provides an accumulation of detailed information (scientific findings) on what has been successful in the past. The advantages in superior communication process and superior information inventory should be evident in all scientific tasks that involve communication and decision-making. But the evidence of these advantages should be easiest to detect in those areas where communication and decision-making are most essential to success. Also, these advantages cannot be realized unless the paradigm has been learned—unless the vocabulary and information is shared.

1. Teaching

Communication is the basis of teaching. We would expect to find that high-paradigm development facilitates teaching at the most advanced levels: the teaching of graduate students, and especially in the advising of

dissertation students. Although scientists are frequently accused of being uninterested in their teaching, we would predict that high-paradigm scientists would find advanced teaching more rewarding than low-paradigm scientists precisely because communication between them and their graduate students is facilitated by their extensive shared vocabulary.

Hypothesis I. High-paradigm scientists will exhibit less conflict over time spent with graduate students than scientists in low-paradigm fields. Three items in the questionnaire were designed to measure conflict over time spent in teaching. The first two of these were concerned with whether the respondents considered the time they spent teaching graduate students as (a) well spent, and (b) intellectually stimulating. The

third item asked if the respondents would prefer to spend more or less time with graduate students. The results on scale scores of these items, given in row B, Table 3, strongly support the hypothesis. Both physical science fields show significantly less conflict than political science or sociology.

Teaching not only involves the communication process, but also the substance of what is being communicated, i.e. the stored information, which is the body of "accepted knowledge" in the field. Since the physical science faculty have been shown to have greater agreement over the content of survey courses and graduate curriculum, it is likely that graduate students and faculty in the physical sciences would also agree over course content. This suggests that the use of graduate students for teaching is more

Table 3. Differences Between Fields on Selected Factors.

	Physics	Chemistry	Sociology	Pol. Sci.	H	df	Sig. Level
A Sum of Ranks Mean	24 ^a -0.5100	45 -0.0407	86 0.8197	55 0.0522	11.41	3	<.01
B Sum of Ranks Mean	65 0.2053	90 0.9166	28 -0.7860	27 -0.6202	16.07	3	<.005
C Sum of Ranks Mean	58 0.6962	85 1.3689	42 0.5084	25 0.3675	10.61	3	<.025
D Sum of Ranks Mean	66 2.6621	58 2.5847	67 2.6448	19 2.3865	8.82	3	<.05
E Sum of Ranks Mean	68 1.9131	87 2.7174	31 0.6592	24 0.5000	15.45	3	<.005
F Sum of Ranks Mean	85 2.4837	49 2.1830	51 2.2211	25 2.0376	10.44	3	<.025
G Sum of Ranks Mean	43 -0.4252	90 1.2429	36 -0.5626	41 -0.3725	10.86	3	<.025
H Sum of Ranks Mean	38 1.5427	17 1.2626	77 2.1592	78 2.1830	15.54	3	<.005

- A Differences in perceived agreement on degree requirements and course content (low score = high agreement; 3-item scale).
 B Differences in conflict over time spent in teaching (low score = high conflict; 3-item scale).
 C Differences in numbers of teaching assistants.
 D Differences in desire for teaching assistants (five-point scale; 1 = need more, 5 = too many).
 E Differences in numbers of research assistants.
 F Differences in desire for research assistants (five-point scale; 1 = need more, 5 = too many).
 G Differences in willingness to work with graduate students (high score = willingness; 3-item scale).
 H Differences in desire for more graduate students in department (1 = more, 2 = same, 3 = fewer students wanted).

^a Only the sums of ranks and the means for fields are reported here; Table 2 illustrates the manner in which these measure are obtained.

efficient in the high-paradigm fields, since they may require relatively less supervision. In line with this reasoning, we advanced and tested Hypothesis II.

Hypothesis II. High-paradigm scientists will use more teaching assistants than low-paradigm scientists. Respondents were asked to report on the numbers of teaching assistants currently working for them. Because an occasional faculty member had an extremely large number of assistants, the median, rather than the mean, was computed for each department. These departmental medians were then treated like the departmental means of other measures, i.e. weighted and averaged for each replication group.

Table 3, row C gives the results, with the pattern of numbers of teaching assistants as predicted. Physical scientists do use more teaching assistants, although the difference between physics and the social sciences is not statistically significant, by the test given on p. — (see footnote 8).

It could be argued that the physical scientists have more assistants because they have more financial support, and that social scientists would have more assistants if they could afford them. Therefore, respondents were asked whether they felt they had about the right number of assistants, too many, or too few. The results on desire for teaching assistants are given in Table 3, row D.

While all fields expressed some desire for more teaching assistants, political scientists felt the strongest need for more assistants. Their rank sum is significantly different from those of the other fields. But the results on this item must be considered in relation to the findings in Table 3, row C. Sociologists reported significantly fewer assistants than physical scientists, yet they show little desire for more. Political scientists, on the other hand, had the fewest assistants of both kinds, and they may indeed actually need more help before they will report a sufficiency. Physicists and chemists have more teaching assistants, and seem satisfied with this state of affairs, suggesting that their financial support is sufficient and has been allocated in terms of their preferences.¹¹

¹¹ Note that these data were collected in 1968 before shortages of funds were as severe as they

The hypothesis seems to hold for physics, chemistry, and sociology, but is more uncertain for political science.

2. Research

Both communication and decision making are important in collaborative research activities, especially if the collaboration involves a superior-subordinate relationship. The supervisor's role is facilitated if he can readily communicate his wishes to the subordinate, and if agreement is easy to reach on decisions because existing information narrows the number of alternatives likely to be successful. The subordinate's role is facilitated because he understands what is wanted, is likely to agree with most of his superior's judgments, and he can communicate his own ideas more readily back to the supervisor. In such a relationship, decision-making is fairly routine for many aspects of the research, and the collaborators can concentrate their efforts on the less routine aspects. Where collaborators are of equal status, joint decision-making may be minimized by dividing the problem into areas of respective special expertise. We would therefore look initially for the advantages of the high paradigm to be evident in research collaboration between graduate students and faculty.

By contrast, collaboration in social science is relatively uncertain. Respondents were asked to report on the amount of disagreement they experienced with research collaborators. Physical scientists reported significantly less disagreement than social scientists. Means by field were (1 = almost none, 10 = a great deal): physics, 3.0; chemistry, 2.4; sociology, 3.2; political science, 3.5 ($\chi^2_2 = 11.1$, 3 df, $p < .02$).¹² Even if the collaborators choose an area where they initially agree on theory and methods, the lack of prior information limiting possible alternatives makes each decision far from routine. Social scientists are therefore likely to expend more effort in reaching agreement over many decisions, and if this process be-

are at present. Other data now under analysis show political science departments to have the lowest level of financial support and the lowest salaries for graduate assistants of the four fields.

¹² Friedman two-way analysis of variance by ranks. See Siegel, 1956:166-173.

comes too difficult, collaboration may no longer offer advantages to scientists. This would be especially true of research done with graduate students, where supervision, and therefore communication and decision making, are expected and probably needed. We will investigate two types of such relationships in high and low paradigm fields: the graduate research assistant, and the dissertation student. In both cases, we predict that the physical scientist will form more collaborative research relationships with graduate students than will the social scientist.

Hypothesis III. High-paradigm scientists will have more research assistants than low-paradigm scientists. Again the respondents were asked to report on the actual number of assistants currently working for them. Since some respondents had extremely large numbers of assistants, medians were again used as departmental measures. Table 3, row E gives the results, which exhibit the predicted pattern, with both physical sciences having significantly more research assistants than social sciences. The magnitude of the differences reflected in the means below the ranks also strongly supports the hypothesis. Again the desire for research assistants was analyzed so that the previous findings could be modified (if necessary) by the effects of differential levels of funding. The results (Table 3, row F) are similar to those for the desire for teaching assistants. Political science, the field with the fewest assistants, evidences the strongest desire for more assistants. Sociologists, who have somewhat more research assistants, express somewhat less desire for additional assistants. Chemists, who are richest in research assistants, are about as desirous as the sociologists to have still more. Physicists, relatively well-endowed with assistants, feel closest to the right number of assistants. The results on chemistry and sociology together support the view that high-paradigm scientists are more receptive to research assistance from graduate students than are low-paradigm scientists. The results on the other two fields are less clear. Since physicists do have relatively high numbers of assistants, and still express some need for more assistants, these data also support the hypothesis. It is not clear whether or not political scientists

would prefer a relatively high or low number of research assistants since they now have the fewest, and probably the number has not reached what they would consider an optimum level of such support.

As mentioned earlier, collaboration and interaction with graduate students can be seen as actually being more valuable to the high-paradigm scientist than to the low-paradigm scientist. This could lead to different practices in relation to taking on dissertation students and time spent with other graduate students.

It is part of the scientific lore that physical scientists "mature" and "decay" early. This is probably because their education is largely a process of learning current paradigms together with the currently accepted findings which articulate or question these paradigms. Once this structure is learned in detail, the graduate student is in a position to design and carry on research. The standards by which results are judged are also relatively clear. Berelson (1960:157-159) reports data from a variety of sources showing that physical science students obtain the Ph.D. in less time than social science students, although this difference is greater for elapsed time than it is for full-time spent working toward the degree.

By contrast, the social science graduate student has learned about an array of competing and often conflicting theories and methodologies. He and his faculty collaborator must together agree to a series of choices on problem, methodology, theory, etc. as they work together. Even where evident agreement on such issues has brought student and professor together to begin with, they can be expected to expend more effort rationalizing their choices. The observation has already been made here that such students probably require more supervision. In line with this argument, we set up hypothesis IV.

Hypothesis IV. High-paradigm scientists will exhibit more willingness to advise and help graduate students than low-paradigm scientists. The above hypothesis was meant to indicate a general openness to working with graduate students, especially students for whom the respondent was chairman of the dissertation committee. Three questions were asked in reference to this hypothesis:

for how many students was the respondent serving as thesis chairman; how many did he think would be an ideal number for whom to serve as chairman; and how many hours per week did he spend helping graduate students with their studies or research. Because the behavior of advising dissertation students might be affected by personal levels of financial support, personal popularity, or the graduate student-faculty ratio of the department, the latter two items were included to take into account both aspirations and another behavior more under the control of the respondent. The results of the analysis by fields are given in Table 3, row G. Chemistry emerges as the field in which scientists are most willing to work with graduate students: its rank sum is significantly different from that of the other three fields. The hypothesis is thus partially supported for the sampled population of scientists.

In an effort to further investigate the unexpected position of physics, separate analyses were run for the behavioral items combined and the aspiration item alone. The same patterns of significant differences as those in Table 3, row G emerged for the number of dissertation students and the hours spent with all graduate students ($H = 12.39$, $p < 0.01$, 3 df).

However, in the analysis of the ideal number of dissertation students, physicists expressed desire for the smallest number of students: their mean was 3.27 versus 5.70, 4.00, and 4.11 for the fields of chemistry, sociology, and political science, in that order. Again the results on the physicists fail to support the hypothesis, while the results on the other three fields lend some support.

A fourth question was asked that seemed relevant to the general question of whether or not high-paradigm scientists were more willing to work with graduate students than scientists from low-paradigm fields. Respondents were asked whether they felt their department should have more or fewer graduate students. The results are presented in Table 3, row H. These data exactly support the hypothesis; both physics and chemistry want more students (19% and 38% more by mean percentages for the fields), while political scientists and sociologists want fewer students (5% and 11% less).

3. A Special Case—Elite Scientists

Because other factors could affect the numbers of dissertation students of faculty members, we tested Hypothesis IV on a group of elite scientists whose behavior and attitudes would be least constrained by the extraneous factors already mentioned.

Elite scientists are presumed to gain independence of action as a direct result of their prestige; if they are dissatisfied, they can readily move to another university (Caplow and McGee, 1958).¹⁸ They are thus in a position to take on, by and large, only as many students as they please. If, as we reason, graduate students are less valuable to elite low-paradigm scientists because of the greater amount of effort required to work with them, low-paradigm scientists will prefer fewer graduate students than high-paradigm scientists, other things being equal.

Hypothesis V. Elite high-paradigm scientists will be more willing to work with graduate students than elite low-paradigm scientists. Analyses were performed on all questions relating to interaction with graduate students: the number of dissertation students that the respondent was currently supervising; the number of thesis students the respondent would consider ideal; how many hours were spent helping all graduate students weekly; was the time spent with graduate students well-spent and intellectually stimulating; if he had the choice, would the respondent spend more or less time with graduate students?

An elite group of respondents was chosen from each field on the basis of a question about whom respondents would hire if made chairman of a department. Those respondents receiving three or more votes from other respondents were included in the elite group. In physics, there were 9 elite respondents; in chemistry, 15; in sociology, 25; in political science, 17.¹⁴

¹⁸ Again, the data were collected in 1968 before the job market became "soft." Even in a soft market, however, elite scientists will be in some demand.

¹⁴ While there were differences in the number of elite nominated in each field, there were also differences in the response rate of those who had received 3 or more votes: 9 responded out of 30 elite for a 30% response rate in physics; 15/36, 42%,

The data on each of the questions were analyzed using one-way analysis of variance; the results are summarized and included with others in Chart 1. They are not presented in more detail because they so closely follow results for the population as a whole. The "willingness" items which include estimates of actual behaviors get mixed results for physics while the attitudinal items support the hypothesis. The results for elite scientists from the other three fields clearly support the hypothesis.

These results were strikingly paralleled by the results of a more direct operationalization of potential mobility. In those analyses, a person was considered mobile in the job market if he reported that he had been approached for a job by the department he chose as the one to which he would most like to go. In the four fields, the number of persons responding yes to this question were: physics, 71; chemistry, 58; political science, 98; and sociology, 122. Data on five of the six items mirrored the findings for the group of elite scientists.

F. Summary of Results

Chart 1 gives a summary of the results referenced by hypothesis number. The direction of the findings is indicated by assigning a value of high, medium, or low to the relative values obtained for the variable for that field, and the findings are grouped by similarity of pattern, rather than by hypothesis number. A summary of the overall directions of the results in relation to the hypotheses is given at the bottom of Chart 1.

G. Discussion

Taken as a whole, the results must be seen as supporting our hypotheses. Scientific fields with high paradigm development differ in many of the predicted ways from fields with low paradigm development. It is reasonable to ascribe these differences to the differing structures of knowledge as expressed through the technology of the fields.

But alternative hypotheses might be advanced to explain the findings. One of these

would be that physical scientists are among the most highly selected and brightest of today's scientists, and that this would facilitate all their work, including reaching agreement. Roe's (1956) study indicates that theoretical (but not experimental) physicists are generally brighter than other scientists. An analysis of GRE scores (Marco, 1968) also suggests that physics graduate students are brighter but that social science graduate students exceed chemistry students in verbal aptitude while in mathematical aptitude the results are reversed. The scores of the top 1% are similar (perhaps because of test ceilings) across the four fields.

Another alternative explanation is that high-paradigm scientists are more highly motivated than others, leading them to accept more graduate students, spend more time with them, etc. We have data available from our questionnaire on this—scores on a scale of "job involvement," developed and validated by Lodahl and Kejner (1965). The scale measures the importance of work performance in the person's self-image; using another measure of involvement, Pelz and Andrews (1966:80–89) found highly involved scientists to be highly productive. Here, we found no significant differences ($H = 5.15$, 3 df, $p > .10$) across our four fields, suggesting that motivational variables will probably not explain our results.

A third explanation is basically economic: that high-paradigm scientists have more resources, an advantage which facilitates all activities and lessens conflict and disagreement. Our data on resources (based on chairmen's responses on budgets and individuals' reports of grant support) show that physics has by far the greatest resources of the four fields (Lodahl and Gordon, 1971). Again the explanation does not fit the data reported here, since physics gives evidence of more difficulties and conflict than chemistry. Also, sociology has more resources than political science, yet gives more consistent evidence of conflict and perceived difficulties than political science.

We thus return to our original explanation—that the structure of knowledge lends predictability to teaching and research, and that scientific fields with more developed paradigms have more structure and thus more predictability than fields with less developed

chemistry; 25/44, 57%, sociology; and 17/33, 51%, political science.

CHART 1: SUMMARY OF FINDINGS BY FIELD

<i>Validating Kuhn Concept</i>		Physics	Chemistry	Sociology	Political Science
Test 1	Variable	High	(Med)	Low	Low
2	Agreement on course content, degree requirements	High	(Med)	Low	(Med)
2	Ease of agreement	High	(Med)	Low	Low
<i>Behavior and Attitudes</i>					
Hyp. I	Variable				
I	Teaching conflict	Low	Low	High	High
V	Teaching conflict of elite members	Low	Low	High	High
II	No. of teaching assistants	(Med) ^a	High	Low	Low
II	Desire for more teaching assts.	Low	Low	Low	(High)
III	No. of research assistants	High	High	Low	Low
III	Desire for more research assts.	Low	Med	Med	(High)
IV	Wants more graduate students in department	High	High	Low	Low
IV	Willingness to work with students	((Low)) ^b	High	Low	Low
V	No. of students of elite members	((Low))	High	Med	((High))
IV	Ideal no. of students				
V	Ideal no. students of elite members	((Low))	High	Low	Low
V	Hours elite spend with graduate students	((Low)) High	High High	Med Low	Med Low
V	Elite wants more time with graduate students	(Med)	High	Low	Low
Total hypotheses and tests supported		10	13	16	12
Total results unclear		2	3	0	3
Total hypotheses not supported		4	0	0	1

^a Results which are not in clear support of the hypotheses (differences between summed ranks were not significant by the test explained in footnote no. 8) are in parentheses. These results, however, fit the pattern predicted by the hypothesis.

^b Results which disagree with the hypothesis in question (significant in opposite direction) are in double parentheses.

paradigms. More detailed explanations of the findings supporting the hypotheses have already been advanced in the discussion of the hypotheses. We will therefore turn to a consideration of the discrepant findings and what they might mean.

There were no discrepancies between any of the results and the hypotheses for the field of sociology. Chemistry had three sets of results that were unclear (not significant) in regard to hypotheses but no discrepancies. Political science produced only one finding not supporting the hypothesis and three unclear sets of results. Physics had the largest number of discrepancies between results and hypotheses. All four discrepancies concern the desirability of working with graduate students, with physicists in general desiring few graduate students.

1. *Styles and Stages of Research*

Several possible explanations of the discrepant findings for physics were considered.

The first of these involves the oversupply of physicists in the job market (American Institute of Physics, 1969). The physicists who responded have an unexpectedly low number of graduate students and obviously prefer so. If physicists were becoming aware of the over-supply of Ph.D.'s within their field at the time of our study (1968), they could have reacted by reducing their personal desires for more students. This explanation seems unlikely, however, in view of their strong desire for more graduate students in the department as a whole (Table 3, row H). Also, lacking comparative data from another time period, we cannot be sure if this interpretation is correct.

An alternative explanation is that physicists are finding it more difficult to work with graduate students on research. The results indicate that elite physicists have relatively few dissertation students, yet spend a great deal of time weekly with graduate students (Chart 1). Perhaps there is more uncertainty

and less predictability in physics than we had anticipated. This could not be for lack of a paradigm, because the respondents agreed with what seems evident: physics is the highest field in terms of paradigm development. Another possible source of uncertainty could be a weakening of paradigm acceptance, with the present paradigm falling into question. According to Kuhn (1964) such a phenomenon would mark the onset of a stage of crisis ending with a new revolution within physics.

One physical scientist colleague sees the prevailing differences between the practice of research in physics and chemistry as differences in style, emphasis, and breadth of concern. Many physicists prefer to search for underlying principles and investigate relatively abstract concepts while chemists tend to work on more specific problems.

These stylistic preferences are similar to distinctions in types of research drawn earlier. Gordon (1966:18) has described research as "innovative" if it attempts to add to knowledge through new lines of inquiry or through the development of new theoretical statements or findings not explicit in previous theory. Research which represents an addition to knowledge along established lines of research or an extension of previous theory he calls "productive research."

Both sets of distinctions parallel Kuhn's discussion of normal science vs. extraordinary science. In defining normal science, Kuhn states, "... a paradigm ... is an object for further articulation and specification under new or more stringent conditions" (Kuhn, 1964:23). Elsewhere Kuhn describes normal science as "achieving the anticipated in a new way" (1964:36). Extraordinary research develops in times of crisis, when the existing paradigm can no longer explain all findings satisfactorily. In the crisis stage, Kuhn describes scientists as "trying experiments just to see what will happen," and as "turning to philosophical analysis ... to weaken the grip of tradition upon the mind" (1964:87-88). He also states, "Like much other research in physics during the last decade, these experiments [on the neutrino and parity nonconservation] were in part attempts to localize and define the source of a still diffuse set of anomalies" (1964:87). Clearly Kuhn considered the field of physics, at the time he wrote, to be

in a crisis stage involving the practice of extraordinary science.

It seems fair to say that our data are consistent with Kuhn's contention that physics is in a stage of crisis. An aspiration toward basic discoveries would call for consciously innovative approaches. This would lower the predictability of research procedures through the deliberate expansion of possible alternatives, leading to some of the same difficulties in decision-making experienced by low-paradigm fields. Vocabulary, however, would not be so affected by the innovative posture, and thus communication would probably still be relatively facilitated by the paradigm.

Our findings tend to support this interpretation. It is in the collaborative research relationships with students that physicists expressed desire for contact with relatively fewer students than we had expected. They, however, are quite satisfied to spend time with students and do, in fact, spend a relatively large amount of time with graduate students. This suggests that the students are useful to them, but that each student requires a relatively high amount of supervision. Measures related more closely to just teaching, where communication is the primary concern, yielded the predicted results.

The unclear findings for Hypotheses II and III in the field of political science have already been discussed. The relatively high desire for additional assistants is balanced by the relative deprivation of political scientists in the number of assistants they have.

2. *Differentiation of Scientific Fields*

The remaining discrepancies include the equivocal results on Tests 1 and 2 for the field of chemistry, and on Test 2 for political science. Other data from this study (Lodahl, 1970:106-129) suggested that the field of chemistry was more highly differentiated into subdisciplines than the other fields in this study, while the field of political science was relatively undifferentiated.¹⁵ Table 4 presents data on the pro-

¹⁵ Data asking respondents to nominate the leading journals in their fields first suggested this interpretation. Chemistry is notable for many specialized journals, while political science is notable for a lack of journals, leading respondents to go outside the field proper for nominations. (See Table 4.2, Lodahl, 1970).

STRUCTURE OF SCIENTIFIC FIELDS

Table 4. Indicators of Differentiation^a within Four Scientific Associations.

	Phys.	Chem.	Soc.	Pol. Sci.
Members (In thousands)	25	115	12	15
Years since Founding	71	94	65	67
No. of Assoc. Journals	4	23	5	1
No. of Divisions or Sections	9	25	7	0

^aThese data were obtained from the National Trade and Professional Associations of the U.S. (1969), from the American Physical Society Bulletin (1968), the American Chemical Society Abstracts (1969), the American Political Science Association Biographical Directory (1968), and from the American Sociologist (1969).

fessional associations of these four fields. These data suggest that the four fields differ in their degree of internal differentiation independently of their level of paradigm development, age, or size. Chemistry is clearly the most highly differentiated field, and political science, with no institutional differentiation,¹⁶ is the least. Physics¹⁷ and soci-

¹⁶ There are professional associations peripheral to the field, such as the one in public administration, or that in comparative law. But this is not internal differentiation or comparable to it. Other fields also have applied or interdisciplinary associations, but are also internally differentiated.

¹⁷ An early reader of this paper suggested that physics was more differentiated than shown here because physics has several applied and interdisciplinary associations banded together with the American Physical Society in the American Institute of Physics. To examine this possibility, we drew a random sample of 25 respondents from each of the four fields and tabulated all individual memberships in professional associations. The results did not reveal any physics subfield association with more membership than auxiliary associations in the three other fields. The second in physics was the Optical Society with four memberships (of 25). As many chemists belonged to the American Physical Society. Five political scientists out of 25 belonged to the American Society of Public Administration. Three of 25 sociologists belonged to the American Anthropological Association. Memberships in the "primary" professional associations in Table 4 ranged from 23 to 25 (92-100%). In addition, it can be noted that no journals published by the other associations in the AIP (see Encyclopedia of

ology both have a moderate degree of differentiation.

With no strongly institutionalized differentiation, political scientists may have perceived more agreement than sociologists over course content and degree requirements simply because "everything" is still presumed to be included or required. When the emphasis is on how easy or hard it is to reach agreement, the political scientists report significantly high conflict, suggesting a lack of consensus, as hypothesized.

The chemists, on the other hand, did not express a significantly high or low amount of agreement on any of the items concerned with field content. This result is not surprising, given the much higher differentiation within this field. With so many subspecialties and the divisions between them so institutionalized, agreement based on paradigm may be attenuated by difficulties of integration. Indeed, under the circumstances, it seems remarkable that the level of agreement is not lower.

The differentiation within scientific fields we are discussing here seems to be a functional kind of differentiation analogous to the general concepts of specialization or division of labor. In fact, the academic subdisciplines are often called specialties. It seems obvious that changes in social structure and changes in technology accompany increased specialization. But what is the impetus for this differentiation? The data (Table 4) on professional associations, admittedly limited, do not support the view that size alone leads to functional differentiation (Lawrence and Lorsch, 1967:7). A more persuasive argument (Lawrence and Lorsch, 1967:8) could perhaps be made for advances in technology leading to differentiation, except that physics, which is a highly developed field, has not differentiated much more than has sociology, a much less developed field. It also seems possible that aspects of the social structure could provide impetus to differentiate (Storer and Parsons, 1968). The beliefs, norms, traditions, and elites of a scientific field may simply favor a broader or narrower approach, respond to conflict by division or co-optation, etc. It is conceivable that these aspects of structure develop in ways largely internal

Associations) were among the top ten journals nominated by our physics respondents.

to the field. In Lawrence and Lorsch's (1967:9) terms, there are "orientations to particular goals." But the most likely explanation is that an interaction of all of these factors with the outside environment gives rise to circumstances that sometimes provide impetus to differentiation and sometimes inhibit it (Lawrence and Lorsch, 1967:134). Some fields may differentiate in response to market demands of clients (Storer and Parsons, 1968:112). Others may differentiate because of internal conflicts of interest. Still others may fail to differentiate because elites prefer to maintain traditions.

Differentiation always presents increased need for coordination, but when tasks are nonroutine, it presents the more difficult problem of integration. Storer and Parsons (1968) suggest three "critical factors": the shared criteria to assess the validity of contributions; the "shape" of the body of knowledge—which determines the "ease of ascertaining a contribution's implications" for a field; and the empirical problem of how widely these criteria are shared. This analysis underlines the importance of our findings that scientific fields vary in degree of consensus. Presumably, lack of consensus also increases problems of integration. Lawrence and Lorsch (1967) have suggested that integration is enhanced across functional divisions of some organizations by the formation of integrative problem-oriented groups. The university already uses this device—the faculty committee. One of the implications of these findings is that the relative importance of faculty committees to the successful operation of a university or a department is related to the institutionalized differentiation and to the degree of consensus within fields.

H. *Conclusions*

The structure of scientific fields has important differential correlates in the attitudes and activities carried on within them. We have studied one aspect of that structure, namely the level of paradigm development, as providing predictability. We found evidence that well-developed paradigms facilitate teaching and research activities. We expect that the relationship is interactive: paradigm development facilitates performance which, in turn, further enhances paradigm development. Zuckerman and Merton

(1971) found that the average rate of rejection of manuscripts submitted to journals varied with the structure of the scientific field. The humanities (pre-paradigm) have the highest rejection rates; the social and behavioral sciences are next high (low paradigm); while the physical, chemical, and biological sciences (high paradigm) have low rejection rates. Even within a single field, the more developed subfields (e.g., experimental and physiological psychology) have a lower rejection rate than the less developed subfields (social, abnormal, clinical, and educational psychology). Various other explanations can be advanced for these results—i.e. high paradigm scientists pay the publication costs of their articles; there is more space available for publication in the high paradigm fields; and high paradigm articles are shorter because of the economical use of mathematical formulae—but the explanation provided by differing structures of knowledge (i.e. paradigms) can readily subsume all of these others. The most highly developed fields have the most efficient communication processes to speed development still further. "For the development of science, only work that is effectively perceived and utilized by other scientists . . . matters" (Merton, 1968:60).

Other variables and constructs must be identified and studied to discover the full extent of the variance in activities and attitudes associated with the differing structures of scientific fields. Our findings have suggested two that warrant further investigation: the differentiation within scientific fields, and the level of innovation occurring in a field at a given time.

The fact that the differing structures of scientific fields have correlates in attitudes, activity levels, etc. of scientists has implications for the way scientists organize themselves (or are organized) to accomplish their work. University professors in a given scientific field must operate at the level of predictability permitted by the structure of knowledge within that field. Social scientists operate in a much less predictable and therefore more anxious environment than physical scientists. We have seen that they have more difficulty in agreeing on course and degree requirements. This continuous struggle to reach consensus in a relatively unpredictable and uncertain environment is likely

to produce high levels of conflict, both within and between individuals.

While we have been primarily concerned in this paper with exploring the applicability and the usefulness of the Kuhn paradigm and related concepts as conceptual tools, our findings have obvious bearing on university structure. Our results strongly argue against some current tendencies to view university structure and university problems in terms that are too simple to match the demands of technology and the associated realities of activities and attitudes found within scientific disciplines. Our results seem to indicate that there are differences between disciplines that go to the heart of teaching, research, and student-faculty relationships. Any attempt to change the university must take into account the intimate relations between the structure of knowledge in different fields and the vastly different styles with which university departments operate. A full discussion of this and related points is beyond the scope of this paper. But our discussion of the university, even in this abbreviated context, is, we feel, an indication of the value of paradigm and related concepts in increasing our understanding of how discipline differences affect the institutional structure of science.

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GOAL IMPLEMENTATION AND OUTCOME IN MEDICAL SCHOOLS

HARRY PERLSTADT

Michigan State University

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The study of goals in organizations has theorized about, but not examined, how organizational structure may affect goal attainment. In this study we examine the relationship between product goals and outcome in terms of internship placements for 78 American medical schools. When variables such as hospital affiliation or student ability intervene in the goal-outcome relationship, we claim that the schools can manipulate them for goal attainment. These findings illustrate Thompson's discussion of constraints on outcome-preference premises.

IN recent years, an interlocking series of theoretical writings and empirical studies has characterized the study of goals in organizations. The theory of goal displacement (Merton, 1957) and its logical extension to goal succession led to studies by Blau (1955), Gusfield (1955), and Sills (1957). The critique of these studies (Etzioni, 1960; Perrow, 1961) focused on the problem of identifying the major goal of the organization in the midst of multiple goals, and the evaluation of organizational success in the light of these goals.

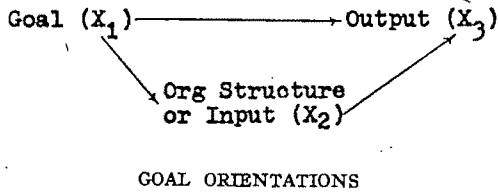
The theoretical response followed two lines. The first focused on goal formation and suggested that the goal of the organization reflects a set of constraints imposed by organizational roles and structure on decision-makers. Goals are essentially the future domain intended by the decision-makers and attained through a series of policy commitments in which a dominant decision-making coalition is formed within the organization. The formation and readjustment of the coalition explains multiple goals, goal conflict and goal succession (Simon, 1964; Cyert and March, 1963; Thompson, 1967).

The second response focused on relative efficiency and argued that organizations should not be compared to ideals of what they ought to be, but rather to the performance of similar organizations. In addition, the conception of organizational effectiveness should take the organization itself as the focal point rather than some external entity or some particular set of people, and should explicitly treat the relations between the organization and its environment in terms of input-output transactions (Etzioni, 1960; Yuchtman and Seashore, 1967; Thompson, 1967; Perrow, 1970).

These theoretical statements suggest a very general model for the effects of goals in organizations (Fig. 1). A direct relationship is said to exist between goals (X_1) and some indicator of output (X_3). Other variables (X_2), such as input or organizational structure, may exercise constraints on the goal-output relationship. By controlling for these other variables (X_2), we may determine if they intervene between goals and output. If the partial correlation between goals and output controlling for specific inputs or structures is near zero ($r_{13.2} \approx 0$),¹ then we assume that the goal leads to the manipulation of these inputs and structures in order to affect output. A partial correlation analysis was used, although a more formal and detailed model—one which specified the order in which these intervening variables might affect the goal-output relationship—would have required a path analysis.

We propose to explore this model and examine the relationships between goals, associated organizational characteristics, and output for a set of similar organizations in 78 four-year medical schools in the United States. These 78 medical schools were all the four-year schools in the U.S. in 1965-66 which were founded before 1955. Goal orientations were obtained by examining the *Announcements* and *Catalogs* of each school. Information on output, in terms of internship appointments, was obtained from the National Intern Matching Program for 1966. Organizational data on the medical schools were obtained from the 1965-66 annual questionnaire submitted to all American medical schools by the American Medical Association and the Association of American Medical Colleges.

Figure 1. Goal Model.



The use of public or official goal statements as indicators of organizational goals has been criticized by Etzioni (1960) and Perrow (1961) because the statements may not contain the latent or operational goals of the organization. But Perrow (1970) seems to have overcome this problem by proposing a new typology. He identifies five types or levels of goals: societal, output, systems, product, and derived goals. An organization may hold any or all five types simultaneously, and may even experience conflict among its various goals.

Perrow's approach permits us to examine one type of goal and its effect on an organization while acknowledging the existence of other goal levels and possible goal conflict. In essence, we can now talk about a specific type of goal that can be differentiated from other types of goals. We shall focus our attention on product goals which classify organizations according to characteristics of goods and services produced. Perrow hypothesized that organizations which are known for the quality of their goods or services have certain characteristics in common, including the utilization of specialized and talented personnel. Additional hypotheses can be made concerning technological skills and such organizational characteristics as size, budget and ownership-control.

We can now examine the product goals which are described in the catalogs of medical schools. These statements promulgate an image of the school to prospective applicants and the public, and provide a self-image for the dean, faculty and students. They are real constraints on the medical school. If a school's statement is quite out of line with what the dean or faculty think the school should be producing or actually was producing, then, in the long run, pressures would arise to change the public statement to conform with output or to curtail discrepant programs.

The thrust and scope of these statements are evident in excerpts from the publications of three medical schools: Prairie State, a large Midwestern school; St. Vincent, a Catholic school; and Louis Pasteur, a private school in a large metropolitan area.

"The course of study in the College of Medicine at Prairie State is designed to produce an unspecialized physician, soundly grounded in modern science, who, after a variable period of postgraduate education and training, is competent to enter his chosen field of medicine.

"In broad scope, medical education has three end products. The first, in terms of numbers, is the practicing physician. In addition, the medical investigator and the medical teacher are products of the same educational process. All three are essential to the growth and application of knowledge in the field of medicine. Two of these, the practicing physician and the medical teacher, must be capable of practicing an art and of applying the natural sciences to medicine. Furthermore, relatively few medical investigators will do research without some responsibility in the teaching or practice of medicine.

"The growing complexity of our society makes it increasingly necessary that the physician be well founded in the humanities which put him in touch with his society, not only to recognize health needs and to be informed of available facilities, but also to gain an understanding and compassion which are basic to the art of healing. The rapid development of medicine as an applied science, requiring quantitative precision and analytical reasoning in diagnostic and therapeutic procedures is also placing an increasing premium upon excellence of preparation in these areas."

The opening of the above statement clearly establishes the primary goal of Prairie State as the production of unspecialized practitioners, that is, general practitioners. Three possible outcomes, however, are acknowledged—practice, teaching, and research. The statement aligns the teaching faculty with the practicing physicians and stresses the art of medicine as an application of scientific knowledge, while medical research isolated from actual practice is not encouraged. The humanitarian aspects of medicine are extolled although the devil of quantitative precision is given his due. Prairie State, then, recognizes three possible ends of medical education and indicates its preference for the unspecialized physician.

While Prairie State acknowledges several possible outcomes, many schools do not. St.

Vincent Medical School, for example, sees only one outcome, the private practitioner. The humanistic and religious underpinnings of general practice and the charismatic role of healer are presented while lip service is paid to science.

As a Catholic University St. Vincent is dedicated to the understanding of divine and human truth so that man may understand himself as a creature in the image of God and with an immortal soul. St. Vincent Medical School joins other institutions and individuals to recognize the dignity of the human person as a creature of God. It is the purpose of the Medical School to train moral and ethical physicians, to bring compassion and scientific leadership to present and future health needs . . .

In direct contrast is the opening statement in the *Announcements* of the Louis Pasteur School of Medicine:

The Louis Pasteur School of Medicine provides for its students a rigorous and excellent program of studies designed to prepare its graduates for distinguished careers in medicine. Emphasis is placed on the scientific basis of medicine and on the skillful application of scientific principles to human problems. In order to maintain a high ratio of teachers to students, thus insuring for the latter a maximum of individual attention, each entering class is limited to 80 students.

Those who manifest interest and aptitude in research are encouraged in this direction and are afforded opportunities and facilities to engage in investigation under proper supervision and sponsorship. These research opportunities can be arranged along with the regular medical studies by utilizing elective time or summer vacations, or they may be scheduled in continuous periods of a year or more intervening between regular terms of medical study.

The primary goal of medical education at Louis Pasteur is preparation for a distinguished career in medicine. A distinguished career will not be found in private or general practice, for Louis Pasteur prides itself on the number of faculty and former students who have won Nobel Prizes in Medicine, become deans of other medical schools or appointed to public health positions.

Notice how the statement links scientific principles with human problems without mentioning humanistic, religious, or emotional themes. The Louis Pasteur School of Medicine believes that research opportunities

and a high ratio of teachers to students is essential for goal attainment.

Schools with statements similar to that of the Louis Pasteur School of Medicine which stressed science and research were coded by an independent coder¹ as research oriented. Schools with statements similar to those of Prairie State or St. Vincent which stressed the art of medicine and the compassionate nature of the physician were coded as practitioner oriented.

MEDICAL SCHOOL OUTPUTS

While medical schools produce physicians, their immediate outputs are graduates who enter internships. At the time under study, the American Medical Association recognized three basic types of internships: (1) *rotating internships* which provide training on the four major clinical services of medicine, surgery, pediatrics and obstetrics-gynecology; (2) *mixed internships* which provide training in two or three of the four major clinical services but emphasize other services such as pathology or psychiatry; and (3) *straight internships* which provide training in a single service such as medicine and offers training in such subspecialties as allergy, cardiology, and gastroenterology. A rotating internship is the most general and may be associated with practice while the other two types of internships are clearly associated with early interest in specialization.

The National Intern Matching Program places a senior in his most preferred internship if that hospital lists him as an acceptable applicant. In March, 1966, 81% of the seniors received their first or second choice hospital while 71% of the hospitals received their first or second choice applicant. Some 50.4% of all seniors participating in the matching program were placed in rotating internships. The percent of seniors placed in rotating internships for individual schools ranged from 5.8% to 93.4%. We wish to explain these variations, and, therefore, take as our measure of school output the percent of each medical school's 1966 graduating class who were placed in rotating internships.

¹ The work of Roberta Klatzky, coder for the Comparative Organization Research Program, and Peter M. Blau, director, is gratefully acknowledged.

We can now make an easy comparison between goal statements and actual output in terms of internship placements. The zero order correlation (Pearsonian r) between practitioner goal orientation and percent of seniors placed in rotating internships is $r = .372$. With a case base of 78, any Pearsonian r greater than .29 is significant on the .01 level. Although the correlation between goals and output is significantly greater than chance, goals account for only 13.8% of the variance in the output. While saying so does not create a specific outcome, goals are in some small way related to outcomes.

MEDICAL SCHOOL INPUTS

We wish to control for the organizational inputs into the medical education system. Philip H. Coombs (1968) identifies the major inputs into an educational system as students, teachers, money and facilities—all of which can easily be operationalized for medical schools.

The student input includes size of student body and scholastic ability. Size may be viewed as the number of medical students in the preclinical or first two years, in the clinical or last two years, or the number in the entering class. Since all three are highly correlated with each other ($+ .90$), we shall use the number of first-year entrants into

medical schools in 1966. The mean size of the entering class was 101 and ranged from 50 to 212. Scholastic ability is defined as the mean score on the science subtest of the Medical College Admissions Test (MCAT) for the entering 1966 class of each medical school. The mean MCAT science test score for all schools was 548 with individual school means ranging between 466 and 656.

Faculty input can be divided into teaching-research areas and salary arrangements. Faculty are either in the basic medical sciences such as anatomy and physiology or in the clinical sciences such as internal medicine and surgery. This distinction is relevant because the basic sciences are traditionally taught in the first two years of medical school and the clinical sciences in the last two.

Faculty may be associated with the medical school on a full-time salaried basis with no private practice or other teaching-research commitments; on a part-time salaried basis with limited outside activities; or on a voluntary basis with an extensive private practice. We shall control for full-time salaried faculty. In addition, we can consider the effects of the various faculty-student ratios on goals and outcomes.

Monetary input into medical schools will be measured by the total amount of budgeted expenditures for each medical school and the total sponsored research expenditures. Sponsored research funds are restricted funds made available to the medical school in the form of contracts, grants, gifts or endowments for the conduct of research activities. In 1965-66, the mean total budget was \$10.6 million, ranging from \$2.3 million to \$36.9 million, and the mean sponsored research expenditure was \$4.4 million, ranging from \$455,000 to \$17 million.

Of the several facilities a medical school requires, the teaching hospital is of utmost importance. Every medical school is associated with at least one general hospital in which it holds didactic clinics for third and fourth year medical students. Although it is relatively uncommon for medical schools to sever ties with hospitals, occasionally a school's department of medicine or surgery will become disenchanted with the teaching program and take up affiliation with another hospital. The situation is by no means stable

Table 1. Seniors Placed in Rotating Internships (X_1) by Practitioner Goal (X_2) and Intervening Variables.*

Variables	$r_{13.2}$	r_{12}	r_{23}
Nonsect. & State Hospitals	.099		
Nonsect. Supported Hospitals	.112	-.459	-.642
State Supported Hospitals	.149	.457	.575
MCAT Science Score	.178	-.423	-.563
Rotating Internship Offered	.222	.437	.442
Total Sponsored Research	.255	-.380	-.414
Total Budget	.261	-.400	-.375

* $r_{13} = .372$

Table 2A. Effects on Percent of Seniors Placed in Rotating Internships ($R^2=.444$)

Variable	b*	Std. Err.	Partial
Practitioner Goal	.00	.11	.00
State Supported Hospital	.39	.12	.33
MCAT Science Score	-.23	.14	-.19
Total Budget	-.09	.33	-.03
Rotating Internship Offered	.09	.11	.10
Total Sponsored Research	-.00	.32	-.00

Table 2B. Effects on Percent of Seniors Placed in Rotating Internships ($R^2=.462$)

Variable	b*	Std. Err.	Partial
Practitioner Goal	.03	.10	.03
Nonsectarian Supported Hospital	-.47	.14	-.37
MCAT Science Score	-.18	.14	-.15
Total Budget	-.08	.32	-.03
Rotating Internship Offered	.02	.12	.02
Total Sponsored Research	.00	.31	.00

as departments or groups of departments may switch from one hospital to another over a period of time. In order to simplify the analysis, we identified one general hospital for each medical school using medical school ownership or major affiliation as our criteria. We are interested in the following characteristics of the teaching hospital: whether it is financially supported by public, private nonsectarian or church funds, and whether it offers rotating internships.

GOALS AND OUTCOMES

Table 1 presents the partial correlations for those control variables which reduce the zero-order correlation between research goals and percent of seniors placed in rotating internships by at least one-fourth.

The intervening variable that reduces the correlation between goals and outcome the most is the financial base of the medical school's teaching hospital. As can be seen, the partial between goals and percent placed in rotating internships is greatly reduced from a zero-order correlation of .372 to a partial of .149 controlling for affiliation with a publicly supported hospital; .112 for affiliation with a nonsectarian supported hospital; and .099 for affiliation with either a nonsectarian or publicly supported hospital.

A regression analysis further confirms the primary importance of the affiliated hospital's financial base. In Tables 2A and 2B, the standardized regression coefficient or beta weight (b*) for affiliated hospital base is greater than for the other four variables present, each of which by itself reduced the correlation between goals and outcome. In Tables 3A and 3B, we find that affiliated hospital financial base has a higher beta weight than MCAT when only those two variables are considered. Finally in Table 4, the nonsectarian supported hospitals outweigh MCAT, while the publicly supported hospitals become inconsequential.

In each of these tables, the intervening

Table 3A. Effects on Percent of Seniors Placed in Rotating Internships ($R^2=.428$)

Variable	b*	Std. Err.	Partial
Practitioner Goal	.05	.10	.06
State Supported Hospital	.36	.11	.36
MCAT Science Score	-.34	.11	-.35

Table 3B. Effects on Percent of Seniors Placed in Rotating Internships ($R^2=.457$)

Variable	b*	Std. Err.	Partial
Practitioner Goal	.05	.09	.06
Nonsectarian Supported Hospital	-.45	.11	-.42
MCAT Science Score	-.25	.11	-.25

Table 4. Effects on Percent of Seniors Placed in Rotating Internship ($R^2=.462$)

Variables	b*	Std. Err.	Partial
Practitioner Goal	.04	.09	.04
Nonsectarian Supported Hospital	-.36	.16	-.24
MCAT Science Score	-.25	.11	-.25
State Supported Hospital	.12	.15	.09

variables reduce the correlation between goals and output to near zero, ranging from .00 to .06. We find, then, that under different controls, teaching hospital financial base consistently appears as the strongest variable, and MCAT as the next strongest in reducing the correlation between goals and outcome.

An interpretation based on just the regression analysis might be judged insufficient because a teaching hospital's financial base is a trichotomous variable—public, church, and nonsectarian—rather than a dichotomous variable of the type usually employed in regression problems. A series of percentage tables, however, may lend support and further insight into our analysis.

Table 5A indicates that schools affiliated with public hospitals are more likely to have a higher proportion of seniors placed in rotating internships than schools affiliated with nonsectarian hospitals. Table 5B shows that schools having practice-oriented goals are more likely to have a higher proportion of seniors in rotating internships than schools with research goals. Table 5C indicates that schools with practice oriented goals are more likely to be affiliated with publicly supported hospitals than schools with research goals, which are more likely to be affiliated with nonsectarian hospitals.

The combined effect in Table 5D indicates that regardless of school goals, medical schools affiliated with publicly supported hospitals are more likely to have a high proportion of seniors placed in rotating internships than schools affiliated with either church supported or private nonsectarian hospitals. In addition, medical schools affiliated with church supported hospitals are more likely

to have a high proportion of seniors placed in rotating internships than schools affiliated with nonsectarian hospitals. The effect of controlling for hospital financial base is evident, although the association may be forced by the small number of research oriented schools affiliated with public hospitals.

GOAL IMPLEMENTATION

Since hospital financial base and MCAT science scored separately, together and combined with other variables substantially re-

Table 5A. High Rotating Internships by Hospital Type.

Hospital Type	% High Rot. Internship	Base N
State	81	42
Church	57	7
Nonsectarian	14	29

Table 5B. High Rotating Internships by School Goal.

School Goal	% High Rot. Internship	Base N
Research	25	20
Practitioner	69	58

Table 5C. Medical School Goal and Hospital Type.

School Goal	St.	Ch.	Ns.	Base N
Research	15%	10%	75%	20
Practitioner	67%	9%	24%	58

Table 5D. Percent High Proportion of Seniors Placed in Rotating Internships by School Goal and Hospital Type.

School Goal	State	Church	Nonsect.
Practice	79% (39)	60% (5)	21% (14)
Research	100% (3)	50% (2)	7% (15)

duce the correlation between goals and output, we conclude that medical schools implement their goals by maintaining affiliation with a particular hospital and admitting students on the basis of MCAT scores.

Perrow (1970) states that organizations which emphasize product goals are often highly specialized and concerned with the quality of their product. These organizations should have certain characteristics in common, including the utilization of specialized and talented personnel and catering to a select segment of the market. We hypothesize that such organizations will also be concerned about the quality of inputs and that service organizations in particular will attempt to preselect clientele.

This hypothesis can help explain the importance of MCAT and hospital affiliation in goal implementation. By admitting students who have done well on the MCAT science achievement test, a medical school can create a rather homogeneous freshman class which is interested in and has achieved success in the basic medical sciences. Schools with research oriented goals are more likely to have a high mean MCAT science score ($r = .423$) than practitioner oriented schools, and schools with a high mean MCAT science score are less likely to have a high proportion of seniors in rotating internships ($r = -.563$) than low mean MCAT schools.

An organization concerned with the quality of output will attempt to control the quality of input. A uniform high quality input will facilitate production of a high quality output. The use of MCAT science scores in the admission process is a quality control over input into medical schools and appears to have the desired effect on output in terms of internship placements. Without a rather homogeneous student body predisposed to scientific research, a medical school desiring to prepare students for specialized careers would have to spend more time orientating students away from the humanitarian side of medicine and towards the research-dependent specialties (see Kendall and Selvin, 1957; Rogoff, 1957; and Becker *et al.*, 1961).

The selection and maintenance of affiliation with a teaching hospital is also important because the hospital can preselect patients whom the third and fourth year

medical students will observe in their clinical studies. Medical schools or their departments select teaching hospitals on the basis of the size and variety of the patient population; but which patients get admitted to a hospital may depend upon several factors beyond the ability to pay.

According to the American Hospital Association (1966:48), many city and county hospitals can accept only those who live within a given geographical area and are residents of the community. A private hospital need not accept all who apply for admission, although emergency service must be offered. Any hospital may limit in-patient admissions by number of clinical beds available in a selected clinical service, and may reject a person who has some common ailment while accepting another with a more unique case. Similar considerations undoubtedly apply to the out-patient clinics where medical students are also trained (Goss, 1957; Olenski, 1957).

A medical school affiliated with a publicly supported hospital may not have the same variety of patients to present for clinical observation as a medical school affiliated with a nonsectarian supported hospital which may be able to admit nonresidents with exotic diseases. Although others have observed public versus private hospital differences in variety of diagnosis (see Becker *et al.*, 1961; Elling and Halebsky, 1961), it is difficult to document because of the confidentiality of medical records. Nevertheless, the financial base of the affiliated hospital appears to influence patient admission and case variety, which are inputs into the medical school educational program. A medical school's choice and maintenance of an affiliation is an indirect selection and control over inputs.

Other aspects of the teaching hospital do not appear to be as important as hospital financial base. The presence of rotating internships in the affiliated hospitals does not have a strong role model effect judging from the beta weights. Becker *et al.* (1961) state that although the house staff and students work in close proximity, their relations are such that they have little direct effect on one another, and the house staff does not figure greatly in the student subculture.

Where the affiliated hospital gets its finan-

cial support is more important for goal attainment than how much money the medical school spends. The total budget and the total amount of special research funds have very low beta weights and partials when combined with hospital financial base and MCAT.

Bucher (1970) claims that at the medical school she studied, the dean did not have much say over the size of the budget or the granting of outside research funds. In contrast, the medical school can control admissions in terms of MCAT and decide upon hospital affiliation. The stronger variables, then, appear to be those that the medical school has more control over and can manipulate to achieve its product goals.

One variable which does not fit into the goal implementation scheme is size of student body. All our measures of size of student body are highly intercorrelated ($r = +.90$), and we have arbitrarily chosen the number of first-year entrants as our measure of size. Number of first-year entrants is unrelated to practitioner goals ($r = -.016$), but is significantly correlated at the .05 level with the percent of seniors placed in rotating internships ($r = .250$). This implies that the greater the size of the entering class, the larger the proportion of a school's seniors placed in rotating internships, but that the schools cannot manipulate size for goal attainment.

The inability of medical schools to control size may rest on pressures towards growth and expansion. External pressures from government and civic groups for more physicians may prevent medical schools from controlling size. Internal pressures to admit many highly qualified candidates may also increase class size. The result is that schools with research goals cannot cut back on enrollment and use size to help attain their goal.

CONCLUSION

These findings illustrate Thompson's (1967:137) discussion of constraints on outcome-preference premises. He writes: "When the core technology of an organization must be employed on human objects, the outcome is in part determined by those human objects; and if they hold opposing outcome preferences, some compromise is likely. . . . This matter of compromise also shows up

clearly in public education and makes one major difference between schools whose students are motivated and those whose students are not."

Our analysis shows that medical schools will attempt to control the outcome preferences of the students. By admitting only students with high MCAT science scores, schools with research oriented goals can influence the dominant outcome preferences in the student culture toward specialization.

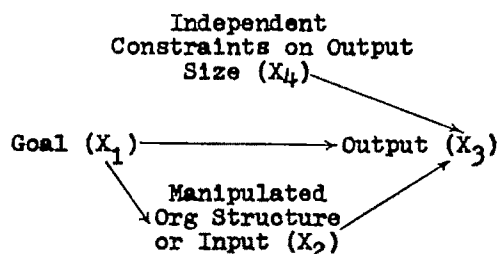
While the public high school in Thompson's example cannot control its admissions, most colleges, universities and professional schools can and do so for the explicit purpose of product goal attainment. This interpretation also supports Thompson's contention that "organizations employing intensive technologies (such as hospitals and educational institutions) and subject to rationality norms seek to expand their domains by incorporating the object worked on" (Thompson, 1967:43).

Not only do they incorporate the medical students, but the schools preselect them. This problem of incorporation also affects the affiliated hospital with its patients and medical staff. We have already argued that the main influence of hospital financial base over internship placements depends upon the patient case variety to which the students are exposed.

Thompson indicates that those on whom the organization depends for inputs may hold veto power over goal attainment. The veto holder may be another organization or specific individuals. The main veto power in our study would then rest with entering medical students and the salaried specialized clinicians in the hospital and medical schools. These are inputs to be sure, but depend upon numerous individual decisions to participate and be incorporated in the schools. Medical schools can more easily set standards for admissions in terms of MCAT and maintain hospital affiliation than they can individually recruit and enroll students or persuade clinicians to abandon lucrative private practices.

A medical school then may or may not control its input. We propose that when medical schools deal with individuals as input, such as medical students, the schools establish quality standards which influence the outcome preferences of the student cul-

Figure 2. Revised Model.



ture and affect the final outcome. If individuals are part of the organizations, such as patients and physicians in hospitals, the medical school and its clinical departments can choose a hospital on the basis of some specific output characteristic which it believes will facilitate attainment of its own product goal.

High MCAT science scores produce outcome preferences favorable to specialization, and nonsectarian hospitals have patients who require specialized treatment and clinical specialists who treat them. With these inputs a medical school will have a relatively large percentage of its seniors placed in straight internships compared to schools with other inputs. These are controllable. But size (number of entering students) which does have an effect on outcome is not as controllable.

The implications for the study of goals in organizations rests on a clearer specification of what organizations can or cannot do to implement goals. Specific outcomes are affected by many different variables, but only a few are under sufficient control of the organization and its decision-makers to be manipulated meaningfully for goal attainment. The model (Fig. 2) still maintains a direct relationship between goals (X_1) and output (X_3). Now, however, two types of inputs are identified: those which can be manipulated for goal attainment (X_2), and those which are independent of goals but place constraints on actual outcome (X_4).

An organization can control specific inputs or organizational characteristics in an attempt to attain a product goal and does best when it can establish a standard above which it will accept inputs, and below which it will reject inputs. An organization can deal effectively with another organization's output as input through affiliation or merger.

But if the organization has to deal with many unorganized individuals and has to entice them to join, or limit the numbers who can join, or maintain ratios of specific sorts, then it does not appear to be able to manipulate them for goal attainment even though these do affect outcome.

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ON THE TRANSMISSION OF MARITAL INSTABILITY IN BLACK FAMILIES *

JEROLD HEISS

University of Connecticut

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An empirical test is made of the idea that marital instability among present day blacks is importantly related to a family history of instability. Data from a sample of urban, northern blacks indicate that for the total sample the association between parental family instability and respondent's marital disruption is quite small. The correlation is higher in the middle class, and for middle class females the effect is particularly strong when the family break occurred early in life. However, parental family instability generally explains very little of the variance. The nature of the post-break living arrangements and the cause of the break are not importantly related to later marital instability, but there is evidence that the loss of the parent of the same sex through death has a stronger effect than the loss of the parent of the opposite sex. It is suggested that pre-marital traits are not highly correlated with marital stability and that parental family instability is particularly weak because it is tenuously tied to the intervening variables between it and respondent's marital stability.

WHEN discussing the causes of divorce and separation among blacks, authors frequently use, alone or in conjunction with other hypotheses, some version of what might be called the "tradition of instability" idea. That is, they suggest that marital instability in this generation is the

legacy of instability in previous generations. Frequently the line is drawn back to slavery, but our concern is with a test of the variant which looks back just one generation. In this case it is postulated that the instability of contemporary black marriages is related in some way to the fact that many blacks grew up in broken homes.¹

* This study is based on data gathered by Robert L. Crain and the National Opinion Research Center under a contract with the U. S. Civil Rights Commission. The author wishes to express his appreciation to Dr. Crain for his kindness in making the data available and to C. Edward Noll and Michael Gordon for their advice and assistance. The analysis is solely the author's and of course does not necessarily reflect the views of the Commission. The data analysis utilized the computer of The University of Connecticut Computer Center which is partially supported by National Science Foundation Grant GJ-9.

¹ Most writers focus on the family headed by a female, but others speak in general of broken families. Some of the latter seem to assume that all broken families are headed by a female. Our evidence does not support this assumption. If a respondent did not live with his real parents until sixteen and did not leave home, he is operationally defined as coming from a broken home. Only fifty-seven per cent of such respondents were brought up in a household in which all the adults were women. In this report we will consider different kinds of broken homes.

For example, Pettigrew concludes, from a study which compares black men who grew up in fatherless households with men whose fathers were present, that "the most critical distinction involves marriage; the first group was more likely to be divorced or separated" (Pettigrew, 1964:20). Bernard takes note of this study and asserts with some subsequent qualification, that "Men socialized without fathers reveal a marked inability to maintain a marital relationship" (Bernard, 1966:125). A similar position is reflected in Rohrer and Edmonson's explanation (1960:190) of why the family life of a particular group of blacks is stable. They state: "Both by precept and by example our 'family members' were taught by their parents to prize the values of stable family life above other goals, and they in turn are well-embarked on transmitting these same values to their children."

Views such as these are common, but usually the causal chain assumed to be involved is not clearly delineated. However, on the basis of a variety of hints we would suggest the following.

Many believe that broken homes are important sources of social and personal maladjustment in both the white and black communities. A complete list of the conditions laid at the door of the broken home is beyond the scope of this paper, but the following are typical: lessened academic achievement, delinquency, lessened resistance to the negative effects of racism (Moynihan, 1965:35ff), failure to learn the values and expectations of society (Lincoln, 1971:346), failure to learn the discipline and habits necessary for personality development (Frazier, 1950:276), inability to delay gratification, mental illness, drug addiction, and crimes against individuals (Pettigrew, 1964:17-26).

These will perhaps suffice. If broken homes lead to only a few of these conditions, one would expect the marital disruption rates of those from broken homes to be high, for such traits do not foster stable marriage.

Broken homes may have yet another effect more specifically related to marriage. Pettigrew (1964:17-21) points to problems of sex identification and sex-role learning among children from broken homes. It is

only a short step from this to the suggestion that such children fail to learn adequate versions of the husband-wife role. This effect might take place in two ways: the parents' interaction before the break may teach "inappropriate" spousal roles, and a role model may be absent after the break. In either case, the child from a broken home may not have learned how to be "a good spouse."

Despite the plausibility and popularity of the view that parental family instability generates instability in the next generation, few data from blacks bear on the point. Studies by Duncan and Duncan (1969) and Pettigrew (1964) do, however, provide directly relevant systematic data. Duncan and Duncan found parental family instability and current marital status are not related. The study by Pettigrew (1964) did show differences in the marital stability and marriage rates of matched samples of working class black males whose parental home stability differed, but the groups number only about twenty each. Given the paucity of studies, their contradictory results, and their limitations, further investigation of this topic seems indicated. We propose in this

Table 1. Respondent's Marital History by Stability of Parental Home by Sex

Stability of Parental Home	Percentage Ever Divorced or Currently Separated		
	Males	Females	Total
Unbroken Home	25.1	36.4	31.2
Broken Home	34.1	42.4	39.2
Weighted N	1492	1995	3487
r12	.08	.08	.08
r12.3	.08	.08	.08
r12.4	.08	.09	.09
r12.34	.08	.09	.09
r12.5	.06	.06	.06
r12.345	.07	.08	.07

1, number of respondent's marriages broken through divorce or separation (0 through 4); 2, stability of parental home (0=unbroken, 1=broken); 3, parental SES (0=lowest, 9=highest); 4, number of years since first marriage; 5, age at marriage.

study to test in detail the transmission of instability idea. Do broken homes run in certain black family lines? If so, is there a causal connection? If so, do the usual explanations square with the facts?

METHODS AND RESULTS

The data to test the idea are drawn from a 1966 study whose primary goal was to determine the long range effects of school integration (Crain, 1970, 1971; Crain and Weisman, Forthcoming). The sample used is a block-quota sample of black males and females aged twenty-one to forty-five, living in the metropolitan areas of the north. The sample was weighted to over-represent persons in higher income neighborhoods and the smaller metropolitan areas. Therefore the data used here are weighted to reflect the actual population. The true sample size is 1651 and the weighted sample contains 4153. Thus the true number of cases is about forty percent of the N's reported here. All the interviewing was conducted by black interviewers.

As is typical in samples of this kind, lower status persons are somewhat under-represented. However, the sample compares with the census quite well on education and occupation. Crain (1970:595) reports the largest difference he found was six percent, and he believes that this discrepancy is, in part, a reflection of true changes in the universe between the time of the census and the time of the study.

The data presented in Table 1 show only a weak relationship between instability of the parental home and instability of the respondent's marriage.² For both sexes, those from unstable parental backgrounds are somewhat more likely to experience divorce or separation in their own marriages ($r = .08$),³ but given the emphasis often put on parental family instability the association is hardly striking.⁴ Though there is probably

Table 2. Respondent's Marital History by Stability of Parental Home by Sex and Parental SES

Stability of Parental Home	Percentage Ever Divorced or Currently Separated					
	Lower SES			Middle SES		
	Male	Female	Total	Male	Female	Total
Unbroken Home	28.4	38.0	33.5	12.0	29.7	21.9
Broken Home	36.0	40.4	38.8	29.4	44.8	38.0
Weighted N	1146	1528	2674	320	408	728
r_{12}	.06	.05	.06	.22	.16	.18
$r_{12.4}$	.06	.06	.06	.22	.17	.19
$r_{12.5}$	.04	.04	.04	.21	.12	.14
$r_{12.45}$	.05	.05	.05	.21	.14	.16

1, number of respondent's marriages broken through divorce or separation (0 through 4); 2, stability of parental home (0=unbroken, 1=broken); 4, number of years since first marriage; 5, age at marriage.

a relationship between these variables, it is clearly a trivial one.⁵

However, this analysis is a weak test of the hypotheses which concern us in this paper. The assumption is that marital instability in the two generations is causally connected and these data barely touch on that assumption. On the one hand even the slight association found may be spurious; on the other, we may be underrating the importance of parental instability because a third variable is acting as a suppressor or because the relationship is limited to a particular subgroup. Even if none of these is true, the effect might be indirect, through some intervening variable not implied in the theory.

Finally, negative responses to the ques-

² A respondent is classified as having an unstable marriage if he is currently separated or has ever been divorced.

³ For the calculation of the Pearson correlations the absence of instability is coded zero and its presence 1.

⁴ Crain and Weisman (Forthcoming) reach the same conclusion after an independent analysis of these data.

⁵ A test of significance might seem appropriate here, but we have generally avoided such tests in this paper. First, the use of weighted N's makes the level of significance unclear. Second, for these data the test of significance is a very weak criterion. All in all it seemed better to focus mainly on association measures. We generally consider correlations of the order of .10 to be real but of little practical importance. Correlations of about .20 or higher are considered worthy of note.

Table 3. Respondent's Marital History by Respondent's Age at Break in Parental Home by Sex

Age at Break in Parental Home	Percentage Ever Divorced or Currently Separated		
	Males	Females	Total
Under 6	33.9	44.4	39.9
6 through 9	27.5	46.0	39.8
10 through 15	43.5	31.2	35.2
Weighted N	545	881	1426
r16	.01	-.04	-.02
r16.345	.05	-.05	-.02

1, number of respondent's marriages broken through divorce or separation (0 through 4); 3, parental SES (0=lowest, 9=highest); 4, number of years since first marriage; 5, age at marriage; 6, respondent's age at time of break in parental home.

tion, "Did you live with both your real parents until you were sixteen?", though often used, are inadequate as a measure of the independent variable, even when we eliminate those who are in this category because they left home. The hypotheses suggest that certain consequences are likely to follow from a broken home, but it is clearly assumed that certain kinds of broken homes are more likely to have these consequences than others. The variable we have used contains within one category a variety of situations: homes broken for varying reasons, followed by a variety of post-break living arrangements, occurring at different times in the respondents' lives. If we are to test the idea and its reasoning adequately, we must try to separate out these varying conditions. We must be certain that the weak relationship reported above is not due to a dilution effect caused by particular kinds of broken homes which are not expected to have a strong effect on the respondents' marital stability. We turn now to a test of these possibilities.

To test for spuriousness we controlled for number of years married and parental SES, as these seemed logical possibilities. The results using partial correlations indicate, however, that these are not associated biases. In every case the partial correlation roughly

equals the zero-order correlation (see Table 1). However, the partial correlations hide an important finding revealed by the use of tabular control (see Table 2). If the respondent comes from a lower class background it makes little difference whether the parental family was stable. However, for middle class blacks, particularly males, the breakup of the parental home appears to have important effects. This finding is consistent with the reasoning underlying the hypotheses as we shall demonstrate in the discussion section. We would note here, however, a misplaced emphasis in previous discussions of family instability inheritance, most of which focus on the lower class. Among blacks, at least, the real effect seems to occur in the middle class.

We have seen so far that the relationship between parental instability and respondent's marital instability is not spurious as far as we can tell, though it is specific to the middle class. We will now consider the directness of the effect. The postulates underlying the idea provide, of course, for a

Table 4. Respondent's Marital History by Respondent's Age at Break in Parental Home by Sex and Parental SES

Age at Break in Parental Home	Percentage Ever Divorced or Currently Separated					
	Lower SES			Middle SES		
	Male	Fe-	To-	Male	Fe-	To-
	male	male	tal	male	male	tal
Under 6	34.9	39.4	37.5	(35.1)	61.5	50.0
6-9	26.6	45.1	39.5	---	---	(29.7)
10-15	(48.5)	33.6	38.1	---	---	(48.9)
Weighted N	394	659	1053	127	170	297
r16	.03	.03	.03	-.14	-.39	-.29
r16.45	.09	.03	.04	-.11	-.43	-.31

1, number of respondent's marriages broken through divorce or separation (0 through 4); 4, number of years since first marriage; 5, age at marriage; 6, respondent's age at time of break in parental home.

*The percentages are not given if the weighted N is less than 50 in a cell. The percentage is enclosed in parentheses if the weighted N is between 50 and 75.

number of intervening variables; but these are generally characteristics of the respondent's early situation believed to affect his personality. Should a marriage characteristic be found to be an intervening variable, the analysis would have to be significantly revised. To test this possibility we controlled for age at marriage to determine whether parental instability has an effect merely because it leads to early marriage and early marriage leads to marital instability. The data of Tables 1 and 2 indicate that in every subgroup, controlling for age at marriage has only a small effect on the zero-order correlations. We conclude that age at marriage is not a significant intervening variable.

We have seen that the effect of a broken home is greater in the middle class. We will now consider if the respondent's age at the time of the break is relevant. Are the effects maximized at a particular age? The Pearsonian correlations presented in Table 3 reveal no consistent relation between age at break of parental home and respondent's marital instability. However, Table 4 suggests we would be hasty to judge this variable irrelevant. Though the N's are too small for the cross-tabs to be useful, the correlations indicate, as before, there is little relation in the lower class and a clear one in

Table 5. Respondent's Marital History by Household Composition After Break in Parental Home (2 way) by Sex

Household Composition	Percentage Ever Divorced or Currently Separated		
	Male	Female	Total
Both sexes	35.5	39.9	38.1
Females only	34.8	39.5	37.8
Weighted N	462	754	1216
r17	-.05	-.01	-.02
r17.345	-.01	.01	.00

1, number of respondent's marriages broken through divorce or separation (0 through 4); 3, parental SES (0=lowest, 9=highest); 4, number of years since first marriage; 5, age of marriage; 7, household composition (0=both sexes, 1=females only).

Table 6. Respondent's Marital History by Household Composition After Break in Parental Home (2 way) by Sex and Parental SES

Household Composition	Percentage Ever Divorced or Currently Separated					
	Lower SES			Middle SES		
	Male	Fe-	To-	Male	Fe-	To-
		male	tal		male	tal
Both sexes	36.7	40.2	38.8	---*	34.0	33.0
Females only	35.8	37.6	37.0	34.9*	44.3	40.4
Weighted N	343	578	921	107	138	245
r17	-.05	-.03	-.04	-.01	.12	.07
r17.45	.02	.00	.00	-.03	.10	.03

1, number of respondent's marriages broken through divorce or separation (0 through 4); 4, number of years since first marriage; 5, age at marriage; 7, household composition (0=both sexes, 1=females only).

*The percentages are not given if the weighted N is less than 50 in a cell. The percentage is enclosed in parentheses if the weighted N is between 50 and 75.

the middle class, at least for women. The older a middle-class girl at the time of the parental break the less the likelihood of a broken marriage.

We see then that under certain conditions a broken parental home has some relation to the next generation's marital stability. We now ask, is it a broken home, per se, or a particular post-break living arrangement which has the effect? Judging from the literature's concern with the female-headed household, we would expect persons brought up in such homes to show greater marital instability than those brought up after the break in a household containing adults of both sexes.

To test this notion, we divided the group experiencing a parental break into two parts according to the nature of the post-break living arrangements. As Table 5 shows, the rate of marital disruption in the respondent's generation is not related to the sex composition of the post-parental break household. However, before we conclude we must look at Tables 6 and 7 to determine if there is

Table 7. Respondent's Marital History by Household Composition After Break in Parental Home (2 way) by Sex and Age at Break in Parental Home

Household Composition	Percentage Ever Divorced or Currently Separated					
	Under 6 at Break			6-15 at Break		
	Male	Female	Total	Male	Female	Total
Both Sexes	41.6	50.7	46.3	---*	29.3	30.6
Females only	34.8	38.6	37.2	34.4	37.6	36.4
Weighted N	266	375	641	165	344	509
r17	-.11	-.13	-.11	-.08	.07	.03
r17.345	-.09	-.07	-.08	-.02	.10	.07

1, number of respondent's marriages broken through divorce or separation (0 through 4); 3, parental SES (0=lowest, 9=highest); 4, number of years since first marriage; 5, age at marriage; 7, household composition (0=both sexes, 1=females only).

*The percentages are not given if the weighted N is less than 50 in a cell.

an effect in particular SES groups or when the break occurred at a particular age.

In general, Table 6 shows that both SES groups reveal the lack of effect. Middle class females seem to be somewhat more prone to divorce and separation if they are raised by women, but the correlation is small and the N's are too small to give us much confidence in the finding. Even using the weighted N we find that the correlation is significant only at the .08 level. When we control for age at the break-up of the parental home (see Table 7), the relationships are of about the same small magnitude in both categories. In three of the four subgroups the differences "favor" the female-only households.

There is no evidence then that a female household has a greater effect than a two-sex household. The fact of a broken home is more important than the living arrangements which follow. In other words, including two-sex households did not dilute the effect of female households.

Though we have disposed of the notion that female households are breeding grounds

for later marital instability among blacks, this does not necessarily mean that post-break family arrangements are irrelevant. We have shown only that the standard "both sexes v. female" distinction is unimportant. Before we leave this subject we will try to discover if this gross classification hides differences of note.

In Tables 8, 9, and 10 we have broken the both sexes category of the previous tables into two parts—reconstituted families in which the remaining parent remarried and expanded families in which the respondent, with or without one of his parents, joined another family group, typically a grandparent group, less typically an aunt and uncle. Generally, these tables reveal no differences of any magnitude. The highest correlations are of the order of .10, and the differences are not consistently in one direction.

Our final concern is to investigate the relationship between the cause of the break in the parental home and the outcome of the respondent's marriage. Many writers on the black family seem to equate the broken home with the home broken by conflict.

Table 8. Respondent's Marital History by Household Type After Break in Parental Home (3 way) by Sex

Household Type	Percentage Ever Divorced or Currently Separated		
	Male	Female	Total
Expanded	(37.8)*	39.2	38.6
Female	34.8	39.5	37.8
Reconstituted	(46.7)	44.0	44.8
Weighted N	413	729	1142
r18	.06	-.01	.02
r18.345	.06	-.01	.01

1, number of respondent's marriages broken through divorce or separation (0 through 4); 3, parental SES (0=lowest, 9=highest); 4, number of years since first marriage; 5, age at marriage; 8, household type (0=expanded, 1=female, 2=reconstituted).

*The percentages are not given if the weighted N is less than 50 in a cell. The percentage is enclosed in parentheses if the weighted N is between 50 and 75.

Table 9. Respondent's Marital History by Household Type After Break in Parental Home (3 way) by Sex and Parental SES

Household Type	Percentage Ever Divorced or Currently Separated					
	Lower SES			Middle SES		
	Male	Fe-	To-	Male	Fe-	To-
		male	tal		male	tal
Expanded	(37.3)	(40.6)	39.0	---	---	---
Female	35.8	37.6	37.0	(34.9)	44.3	40.4
Reconstituted	---	44.0	46.0	---	---	---
Weighted N	301	553	854	100	138	238
r18	.12	-.03	.02	-.08	.11	.02
r18.45	.11	-.02	.02	-.11	.11	.04

1, number of respondent's marriages broken through divorce or separation (0 through 4); 4, number of years since first marriage; 5, age of marriage; 8, household type (0=expanded, 1=female, 2=reconstituted).

*The percentages are not given if the weighted N is less than 50 in a cell. The percentage is enclosed in parentheses if the weighted N is between 50 and 75.

However, in this sample, forty-six per cent of the persons who did not live with their real parents until sixteen report this was because one or both of the parents died. We will now try to determine whether the cause of the break is related to the dependent variable.

The data of Tables 11 through 13 indicate, first, that the cause of the break has small effects at best. Second, for males the effect of a parent's death is greater than the effect of a conflict break. For females the relative effect of the two causes varies in different subgroups when other variables are held constant. We conclude that the cause of the break is of no significance.

Though a parent's death produces results not much different from the results of a break through conflict, we looked deeper to determine if a particular parent's death is important. Table 14 which compares the marital disruption rates of those who lost a father through death with those who lost a

mother yields a negative response. The category differences are not large. However the correlations differ in direction for males and females. Apparently the main differentiating factor is the loss of the parent of the same sex.

Despite this intriguing clue, we find that the relationship is a weak one. However, if we control for parental SES a familiar picture emerges. In the lower class the categories differ little but the middle class reveals a substantial relationship (see Table 15). For persons of middle class background the loss of a parent of the same sex has a much greater impact than the loss of the parent of the opposite sex. Note, however, that the N's are quite small for both sexes. When we control for age at the break, as in Table 16, we find only one relationship of any size. Males who at an older age lost their fathers through death are more likely

Table 10. Respondent's Marital History by Household Type After Break in Parental Home (3 way) by Sex and Age at Break in Parental Home

Household Type	Percentage Ever Divorced or Currently Separated					
	Under 6 at Break			6-15 at Break		
	Male	Fe-	To-	Male	Fe-	To-
		male	tal		male	tal
Expanded	---	(52.4)	52.0	---	---	(25.0)
Female	34.8	38.6	37.2	34.4	37.6	36.4
Reconstituted	(48.0)	(55.6)	52.2	---	35.5	36.9
Weighted N	230	367	597	154	327	481
r18	.02	-.03	-.02	.00	.06	.04
r18.345	.01	-.01	-.02	-.03	.07	.03

1, number of respondent's marriages broken through divorce or separation (0 through 4); 3, parental SES (0=lowest, 9=highest); 4, number of years since first marriage; 5, age at marriage; 8, household type (0=expanded, 1=female, 2=reconstituted).

*The percentages are not given if the weighted N is less than 50 in a cell. The percentage is enclosed in parentheses if the weighted N is between 50 and 75.

to have unstable marriages than those who lost their mothers at this age.

In summary, our findings indicate that a break in the parental home affects marital stability primarily in the middle class, and in this class a break at an early age has more effect, particularly for women. The composition of the household after the break is irrelevant but the cause of the break has some significance. Only small differences exist between those whose parental home was broken by conflict and those whose home was broken by death. However, within the latter group the loss of the parent of the same sex has a greater impact—if the person was of middle class background.

All in all, however, the correlations obtained indicate that the kind of variables we are considering explain little of the variance in this sample. We find the larger correlations only by comparing categories of rather limited subtypes. Most of the factors which affect marital stability among blacks remain to be discovered.

DISCUSSION

In the light of the above, why are the relationships so weak in general, and, why are a few so much stronger?

In general, the hypothesis that parental family instability causes marital disruption

Table 11. Respondent's Marital History by Cause of Break in Parental Home by Sex

Cause of Break	Percentage Ever Divorced or Currently Separated		
	Male	Female	Total
Death	39.9	45.6	43.5
Conflict	27.8	41.1	35.8
Weighted N	528	863	1391
r19	-.13	-.04	-.07
r19.345	-.09	.02	-.01

1, number of respondent's marriages broken through divorce or separation (0 through 4); 3, parental SES (0=lowest, 9=highest); 4, number of years since first marriage; 5, age at marriage; 9, cause of break (0=death, 1=conflict).

Table 12. Respondent's Marital History by Cause of Break in Parental Home by Sex and Parental SES

Cause of Break	Percentage Ever Divorced or Currently Separated					
	Lower SES			Middle SES		
	Male	Fe-	To-	Male	Fe-	To-
		male	tal		male	tal
Death	43.1	45.2	44.4	(34.0)*	(46.2)	40.7
Conflict	27.3	38.4	34.2	28.2	41.6	35.3
Weighted N	379	657	1036	131	154	285
r19	-.16	-.06	-.09	-.09	-.01	-.05
r19.45	-.09	.00	-.03	-.14	.04	-.03

1, number of respondent's marriages broken through divorce or separation (0 through 4); 4, number of years since first marriage; 5, age at marriage; 9, cause of break (0=death, 1=conflict).

*The percentages are not given if the weighted N is less than 50 in a cell. The percentage is enclosed in parentheses if the weighted N is between 50 and 75.

in the following generation suffers from the problems shared by all hypotheses predicting marital outcome from the pre-marital characteristics and experiences of individuals. Marriage is a relationship and a process, and its outcome is determined not only by each individual's previous experiences but also by the relationship between the traits of the married pair and by the details of the situation in which they interact. While the characteristics of individuals are of some relevance, and while these characteristics are perhaps predictive of some of the relevant post-marital factors, to deal with a single pre-marital trait of individuals means that one is using extremely limited information. Such a focus insures that only a small part of the variance will be explained. It should be remembered that marital prediction studies of whites which use instruments composed of large numbers of factors of this type do not explain more than 25 per cent of the variance in marital outcome.

However we believe that the particular trait considered in this paper is not the most

Table 13. Respondent's Marital History by Cause of Break in Parental Home by Sex and Age at Break in Parental Home

Cause of Break	Percentage Ever Divorced or Currently Separated					
	Under 6 at Break			6-15 at Break		
	Male	Fe-male	Total	Male	Fe-male	Total
Death	39.1	45.4	42.8	41.6	45.1	44.1
Conflict	29.8	44.2	38.1	27.0	35.8	32.8
Weighted N	309	425	734	200	418	618
r19	-.13	-.02	-.06	-.12	-.10	-.11
r19.345	-.11	.13	.04	-.05	-.08	-.07

1, number of respondent's marriages broken through divorce or separation (0 through 4); 3, parental SES (0=lowest, 9=highest); 4, number of years since first marriage; 5, age at marriage; 9, cause of break (0=death, 1=conflict).

useful of this type. To predict marital outcome among blacks from premarital traits of individuals one probably could find many more powerful indicators.

Why is parental family instability not a key premarital trait for blacks? To find out let us examine the theory that says it is. The assumption is made that instability has an indirect effect. It leads to a lessened ability to adjust and fosters spousal role conceptions whose enactment the partner will not accept. The ability to adjust and conceptions of one's role are the supposed intervening variables. We believe the first link in this chain is particularly weak. Our basic point is, of course, that the second link—the link between the intervening variables and marital outcome—is, of necessity, weak. However, we believe the connection between the intervening variables of this study and marital outcome is as strong as any. The unique problem is that the antecedent situation, parental home instability, is weakly tied to the intervening variables.

The postulate that parental instability leads to poor adjustment ability and inade-

quate role conceptions is based on several weak assumptions. For one thing, the break up of the parental home is assumed to be traumatic and psychologically upsetting. Though this might often be the case, black children face many traumas, regardless of the stability of their home. In many cases, the difference between broken and unbroken families may be minimal. In fact, a parental break up may come as welcome relief if there is constant conflict. There is, of course, limited evidence from whites that the children of broken homes are better adjusted than those living in conflict-ridden homes (Nye, 1957). Also many black women without spouses manage to provide good homes for their children. The evidence on the effects of broken homes cannot be denied, but it is more complex and not as strong as generally assumed. (See, for example, Rosenberg, 1965.)

The postulated relationship between parental family stability and spouse role learning can also be questioned. It derives from the following assumptions: (1) The presence of both parents indicates that adequate models will be presented. (2) The major, if not the sole, source of role learning is the family. Relatives, substitute parents, and members of the surrounding community have little relevance. (3) Not only is the major learning done within the family, the

Table 14. Respondent's Marital History by Sex of Dead Parent by Sex

Sex of Dead Parent	Percentage Ever Divorced or Currently Separated		
	Male	Female	Total
Mother died	36.1	53.5	46.7
Father died	44.2	40.1	41.5
Weighted N	203	349	552
r1A	.13	-.10	-.02
r1A.345	.13	-.04	.01

1, number of respondent's marriages broken through divorce or separation (0 through 4); 3, parental SES (0=lowest, 9=highest); 4, number of years since first marriage; 5, age at marriage; A, sex of parent that died (0=mother, 1=father).

only adequate teacher is the occupant of the role. Specifically, a mother cannot teach her son to be a "good" husband, and cannot teach her daughter what to expect from, and how to adjust to, a man. (4) If a role model is presented, it will be learned. (5) The relevant socialization takes a long time to accomplish.

Though there is probably some element of truth in most of these assumptions, they are at best weak. The break-up of a family does not ensure inadequate role learning and stability does not ensure adequate learning. Particularly dubious, in terms of the general good showing of the female households, is the notion that the mother and/or the general community cannot provide the necessary instruction for males.

Nonetheless, parental family instability is more likely to lead to maladjustment and inadequate role learning under some circumstances than others. This being the case, we would expect predictable variation in the

Table 15. Respondent's Marital History by Sex of Dead Parent by Sex and Parental SES

Sex of Dead Parent	Percentage Ever Divorced or Currently Separated					
	Lower SES Male Fe- To- male tal			Middle SES Male Fe- To- male tal		
Mother Died	(42.1) [*]	46.8	45.0	---	---	(50.0)
Father Died	44.6	41.4	42.4	---	---	(33.3)
Weighted N	149	280	429	52	61	113
r1A	.08	-.04	.00	(.27)	(-.51)	-.13
r1A.45	.12	.00	.04	(.23)	(-.49)	-.19

1, number of respondent's marriages broken through divorce or separation (0 through 4); 4, number of years since first marriage; 5, age at marriage; A, sex of parent that died (0=mother, 1=father).

*The percentages are not given if the weighted N is less than 50 in a cell. The percentage is enclosed in parentheses if the weighted N is between 50 and 75.

Table 16. Respondent's Marital History by Sex of Dead Parent and Age at Break in Parental Home

Sex of Dead Parent	Percentage Ever Divorced or Currently Separated					
	Under 6 at Break Male Fe- To- male tal			6-15 at Break Male Fe- To- male tal		
Mother Died	---	(59.7)	52.8	---	(46.0)	39.0
Father Died	(35.8) [*]	36.8	36.4	---	42.0	46.4
Weighted N	113	157	270	84	182	266
r1A	.01	-.12	-.06	.32	-.08	.04
r1A.345	.00	-.03	-.04	.37	.00	.10

1, number of respondent's marriages broken through divorce or separation (0 through 4); 3, parental SES (0=lowest, 9=highest); 4, number of years since first marriage; 5, age at marriage; A, sex of parent that died (0=mother, 1=father).

*The percentages are not given if the weighted N is less than 50 in a cell. The percentage is enclosed in parentheses if the weighted N is between 50 and 75.

strength of the relationships between parental family stability and the stability of the respondent's marriage.

It is no surprise, for example, that the relationship is stronger in the middle class. In most cases a broken home means the loss of a father, and the loss of a middle class father is, in some relevant respects, a greater loss than the loss of a lower class father. Much literature on the lower class black family suggests that the father when present can't fulfill many of his functions, so his leaving would have a slighter effect. On the other hand, the middle class black youth from a broken home is at a disadvantage compared to other middle class blacks. A similar line of reasoning would explain why the respondent's age at time of break is negatively related to marital stability in the middle class and not at all related in the

lower. An early break causes more problems for the middle class child than a later. His family with its guidance and help is intact for a shorter time. Given the situation of the lower status father his child does not gain as much from a longer association.

In addition, and more generally, the family is more central for the middle class individual, and thus he is more influenced, for "good or evil" by what goes on there. Therefore, the correlations within the middle class should be higher.

It also makes sense that more significance attaches to the death of the parent of the same sex than to the death of the parent of the opposite sex. Losing a parent of the same sex may be the more traumatic event and its effect on role learning would seem to be greater. Such a loss ensures the loss of a role model, and though the addition of a step-parent might moderate the effect it will not remove it. In losing a parent of the opposite sex, the child does not lose a role model.

We would conclude, then, that the transmission of instability idea is not the key to the high rates of marital instability among blacks. Nonetheless, some types of broken homes are relevant in some subgroups. When conditions maximize the likelihood of certain intervening situations, the associations are higher. However, if we are truly to understand variations in marital stability among blacks or whites, we will have to stress more the couple's characteristics and marriage situations.⁶

⁶ Crain and Weisman (Forthcoming) takes an important step in this direction using data from the sample used in this article.

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FAMILY DISRUPTION, DELINQUENT CONDUCT AND THE EFFECT OF SUBCLASSIFICATION *

ROLAND J. CHILTON

University of Massachusetts

GERALD E. MARKLE

Western Michigan University

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Employing seriousness of offense as a measure of delinquency, we re-examine the relationship between delinquency referral and family disruption. Information from the Juvenile and County Courts of Florida for the first four months of 1969 provided us with uniform delinquency data for 8,944 children. We compare the family situations of 5,376 of these children with the situations of children in the U.S. population in 1968. The analysis suggests (1) that children charged with delinquency live in disrupted families substantially more often than children in the general population, (2) that children referred for more serious delinquency are more likely to come from incomplete families than juveniles charged with minor offenses, and (3) that family income is a more important factor for understanding the relationship between delinquency referral and family situation than age, sex, or urban-rural residence, but that it may not be more important than race. We briefly examine the implications for alternative intervention strategies in light of these variables' interrelations.

SYSTEMATIC examinations of the relationship between family arrangements and juvenile delinquency have been undertaken throughout this century. In general most such studies, including Gluecks' (1950), Monahan's (1957), and Toby's (1957), suggest that children charged with juvenile delinquency reside in disrupted families far more often than children in the general population, although one earlier (Shaw and McKay, 1931) and one later investigation (Sterne, 1964) produced results inconsistent with these findings.¹

This significant yet unresolved issue, the extent to which children from disrupted families are over-represented among children charged with delinquency, led us to compare the family situations of a set of children referred to juvenile court on delinquency charges with the situations of juveniles in the general population.

We identified two additional issues in examining earlier studies. First, some research reports suggest that the seriousness of officially recognized misconduct might be related to family living arrangements (Sterne; Ferdinand); and second, some investigations try to assess the effect of selected background variables on the relationship between

delinquency and family situation (Monahan; Toby). In pursuing these issues, since income, race and family disruption are obviously interconnected, we were particularly interested in the effect of income and race on the relationship between family disruption and seriousness of offense.

Data and Method

Data for our analysis were taken from reports provided by the Juvenile and County Courts of Florida on a form developed by the U.S. Children's Bureau.² One copy of the form was to be submitted for each child referred to court during the first four months of 1969. Fifty-two of the state's sixty-seven juvenile courts, representing counties which contain 96 percent of the state's population, submitted reports during this period. Report forms were submitted for almost nine thousand children (8,944), with information on each child's living situation being provided for approximately 60 percent (5,376) of the total. However, this proportion was constant for both sexes and major age categories, and fairly constant for both races, being available for approximately 58 percent of the white and 66 percent of the black children.³

* Revision of a paper presented at the 1970 meeting of the American Sociological Association, Washington, D.C.

¹ For comprehensive reviews of such studies, see Herzog and Sudia (1970), Rodman and Grams (1967), Sterne (1964), and Monahan (1957).

² Juvenile Court Statistical Card: Children's Bureau, Social and Rehabilitation Service, Department of HEW, Form CB-203-S Revised.

³ Since we lack this information for almost 40 per cent of the children referred to court, our data

Table 1. Percentage distributions of the family situation of children 10-17 in the general population, children charged with offenses applicable to juveniles only and children charged with other offenses.

Family Situation	General ¹ Population ¹ (age 10-17)	Children charged with offenses applicable to juveniles only N=2470	
		which would be crimes for adults N=2906	
Husband-wife family	83.0	60.3	60.0
Mother only	11.7	25.0	27.5
Father only	1.6	4.1	3.1
Neither parent	3.7	10.6	9.4
Totals	100.0	100.0	100.0

¹(U.S. Bureau of the Census, 1968:22)

Comparison with the General Population

Table 1 compares the family situations of children in the general population with those of children referred to court on delinquency charges. It suggests that the living arrangements of children ten to seventeen years old in the U.S. population distribute quite differently from those of children in Florida charged with offenses which would be crimes if committed by adults and from those of children charged with offenses which apply to juveniles alone.⁴

The use of census information also permitted separate comparisons of the family situation of ten to seventeen year old black children and ten to seventeen year old white children referred to juvenile courts on delinquency charges. By assuming that as many girls as boys in the general population do not live with both parents, we could make this comparison by sex as well as age and race. Table 2 presents the results of our analysis.⁵

are seriously limited. However, this disadvantage must be balanced against the advantage of having reasonably comparable data on a uniform report form from courts distributed over one state, representing both rural and varied urban counties.

⁴The issue to which such a comparison is directed is this: Are children referred to court on delinquency charges more often located in disrupted families than children in the general population? The information is percentaged thus because our procedure provides a reasonable and informative comparison. The text makes explicit all inferences about causal relations or causal order.

⁵Census information for Florida's population might have been better for this purpose but was unavailable for 1968. A comparison of Florida and

Although the proportion of girls referred on charges applicable to juveniles only who were not living with both parents is slightly higher than the boys' proportion, the same difference does not appear for offenses which would be crimes if committed by adults; and it is not nearly as striking as differences found when we compare the situations of black and white children. While black children in the juvenile court sample not living with both parents total approximately 58 percent, the total of white children not living with both parents is 29 percent for one set of referrals and 34 for another. However, these differences must be examined in relation to the situations of black and white children in the general population, where 43 percent of the black children do not live with both parents in contrast to 13 percent of the white. While black children in the general population are three times as likely as white to be living in situations other than husband and wife families, black children in the juvenile court population are less than twice as likely as white to be living in similar situations. This suggests that, in comparison with the proportion of black children in the general population not living in husband-wife families, the proportion of black children in the court population in similar situations is not high.

U.S. data for 1960 suggests that the proportion of children living in husband-wife families was approximately four per cent lower in Florida, a situation which appears to result primarily from differences in the living arrangements of non-white families. (U.S. Bureau of the Census, 1961:142; 1964:210)

Table 2. Percentages of children 10-17 not living in husband-wife families for those in the general population, those charged with offenses applicable to juveniles only and those charged with other offenses.

Classification	General Population (age 10-17)	Children charged with offenses applicable to juveniles only which would be crimes for adults	
All classes	17.0	39.7	40.0
Male	17.0	37.2	40.2
Female	17.0	43.6	39.0
Black	42.8	58.1	58.5
White	13.1	33.7	28.5
Black male	42.8	56.8	59.3
White male	13.1	30.7	28.3
Black female	42.8	60.4	54.7
White female	13.1	38.5	29.6
Age 10-13	15.2	45.8	41.6
Age 14-17	18.9	37.9	39.6

A comparison by age group of the living arrangements of children charged with delinquency and those in the general population suggests that the proportion of younger children (ten to thirteen) charged with delinquency who are not living with both parents is much larger than the comparable proportion of children in the general population. Table 2 also shows this comparison for fourteen to seventeen year olds and suggests that this difference between the general and juvenile court population may not be as

great for this older age group. However, interpretation of this subclassification by age is complicated by the fact that most seventeen year olds in Florida are referred to adult courts.

Except for this issue, Table 2 appears to support conclusions presented in Toby's 1957 study that white children and pre-adolescents are more vulnerable to the effects of family disruption than black or adolescent children. Unfortunately, census information which would permit similar comparisons for

Table 3. Family situation by seriousness, number of children in specific offense categories with complete and incomplete families.

Family Situation	Seriousness			all classes ⁴
	most ¹	less ²	least ³	
Incomplete	87	919	304	1310
Complete ⁵	50	859	383	1292
Total	137	1778	687	2602
Gamma = .18	$\chi^2 = 20.97$		$p < .01$	

¹Aggravated assault, robbery, rape, homicide.

²Larceny, burglary, auto theft, shoplifting, simple assault, purse snatching, unauthorized use.

³Drunkenness, disorderly conduct, sex offenses, vandalism, drug law violations, weapons offenses.

⁴This table includes only children who could be charged with crime if they were adults. It excludes 2470 children charged with offenses applicable to juveniles only and 304 children where "other" was recorded as the reasons for referral.

⁵In this table and those which follow "complete" does not include family situations with one parent and a step parent.

Table 4. Percentages of children not in complete families for three categories of offenses, subclassified by age, race, sex and residence.¹

	Seriousness ²			Gamma	χ^2
	most	less	least		
All classes	64(137)	52(1778)	44(687)	.18	20.98*
Sex					
male	63(122)	52(1467)	45(556)	.17	15.65*
female	67(15)	51(311)	41(131)	.22	5.42
Race					
white	50(38)	41(1040)	36(500)	.13	5.90
black	69(99)	66(738)	67(187)	.01	.21
Sex and race					
white male	50(36)	41(856)	35(404)	.13	5.57
black male	69(86)	67(611)	70(152)	.04	.77
white female	50(2)	41(184)	38(96)	.08	.46
black female	69(13)	65(127)	51(35)	.24	2.31
Age					
under 13	50(16)	54(305)	36(98)	.30	10.39*
13-14	68(31)	49(630)	46(183)	.13	5.06
15 and over	64(90)	52(843)	46(406)	.17	11.99*
Residence					
rural-suburban	58(19)	46(342)	46(112)	.04	.95
urban	65(111)	51(1245)	52(499)	.22	23.24*

¹Figures in parentheses indicate the total number of cases in each seriousness category; figures outside the parentheses indicate the percentage of this number not living in complete families.

²See notes 1-3, Table 3.

*Indicates probabilities less than .01.

subclassifications based on rural-urban residence and income is not available.

Seriousness and Persistence

In 1964 Sterne, however, did suggest a method which permits additional subclassification and comparison, and which widens our understanding of the relationship between family disruption and delinquent conduct. From his work one could hypothesize that, if delinquency referral is related to family situation, children charged with serious misconduct would come from broken homes more often than children charged with minor violations. Table 3 presents the results of this analysis when a three point scale is used to measure seriousness. It suggests a modest, positive association between seriousness and family situation for children charged with offenses which would be crimes if committed by adults.⁶ Among children referred for the

most serious offenses, more are reported as living in incomplete families than complete; among children referred for the least serious offenses, the opposite is true. The gamma value (.18) summarizes this relationship and avoids confusion about the direction in which subsequent tables should be percentaged.⁷

When the children charged with delinquency are subclassified by sex, the same comparison produces a pattern for boys almost identical to Table 3's. Table 4 presents this result and suggests that the relationship for girls is stronger but not statistically significant. Table 4 suggests a relationship roughly similar to Table 3's for children from

incorrigibility, truancy and running away from home cannot be logically integrated into a crime classification and may themselves result from a confused home situation, making whatever results one observed difficult to interpret.

⁷Tables 4 and 5's percentage figures are intended to illustrate a particular pattern and to make reconstruction of the original tables possible. Conclusions about the degree and direction of association are based on the Gamma values in each case and not percentage differences.

⁶For this and subsequent analysis only children charged with offenses which would be crimes if committed by adults are included. Referrals for

Table 5. Percentages of children not in complete families for three categories of offenses, subclassified by income.¹

	Seriousness ²			Gamma	X ²
	most	less	least		
All classes	66(105)	51(1231)	44(449)	.19	17.00*
under \$3,000	81(26)	83(217)	87(55)	-.14	.69
\$3,000-5,000	79(43)	62(401)	60(124)	.15	5.48
\$5,000-10,000	41(34)	36(489)	33(185)	.07	.97
over \$10,000	0(2)	23(124)	19(85)	.11	1.17

¹See note 1, Table 4. Only those cases for which income and living arrangements data were available are included here (N=1785).

²See notes 1-3, Table 3.

*Indicates probability less than .01.

urban areas and for children in two of three age categories. It does not appear for black children, primarily because seriousness and family situation for boys from black families appear not to be associated.

The result of this subclassification by race is probably the most striking variation we observed. The information in Table 4 indicates that differences between white and black children in the percentage of children living with both parents is sizable regardless of the degree of seriousness of the group of offenses examined. But, for black children, the percentage of children not living with both parents is very similar for those charged with the least serious offenses (67 percent) to those charged with the most serious (69) percent. The weaker, and statistically insignificant, association between family situation and seriousness for black children (Gamma = .01) suggests that the family situation may be less important in generating serious misconduct among black children, particularly boys, than it is among white. Why this should be so is not immediately clear. But an obvious hypothesis is that the economic, social and political position of black families in Florida has more to do with the extent to which their children encounter the system of justice on serious charges than do their living arrangements.⁸

⁸ Since classification schemes for seriousness are essentially arbitrary, we also examined these relationships using a variety of classification systems. In general, the results were like those produced in the above analysis.

Family Income

Having family income information available in the Florida court data, we were able to examine the relationship between delinquency referral and family situation. Table 5 presents the results of our analysis and illustrates the importance of income for understanding this relationship. The Gamma value computed for children from families with incomes of less than three thousand dollars, for example, is not simply lower than the originally observed value but suggests a reversal of the general pattern. Moreover, the proportions of children from disrupted families sizeably declines when we examine the living situations of children from families with incomes over five thousand dollars. For those in families with incomes over ten thousand, the proportion living in disrupted families is small for all seriousness categories, never rising above 25 percent of the children in any single category. Combined with generally lower Gamma values for the relationship between seriousness and family situation for two higher income subcategories, this decline suggests that family income may be an important variable for interpreting the relationship between delinquency referral and family situation.

Important to our discussion is the obvious relationship between low income and family situation. While it is not surprising that one-parent families in this sample have lower incomes than two-parent families, the fact that seriousness seems unaffected by family situ-

ation for low income families is important. For children from families with very low income, it would appear that the family's economic situation and not its composition is the more important factor in understanding the child's apprehension for delinquency. Seen in light of the generally low income of black families in Florida, this analysis appears to reinforce the results of our subclassification by race, where we found little or no relationship between family situation and seriousness of offense for black children.⁹

Conclusions

However we interpret these subclassifications, our study provides added empirical support for the conclusions of earlier investigators who have suggested that proportionately more children who come into contact with police agencies and with juvenile courts on delinquency charges live in disrupted families than do children in the general population. In addition, the study suggests that children charged with more serious misconduct more often come from incomplete families than children charged with less serious delinquency. Although these conclusions call into question Sterne's (1964) and Ferdinand's (1964) results, both of whom used broad, dichotomous classifications of offenses by seriousness, others of our conclusions support Monahan's general findings and to a lesser extent Toby's.

Though we present it cautiously, our analysis' most important finding is the apparent impact of subclassification by reported family income on the relationship between delinquency referral and family living arrangements. Our subclassification analysis suggests that income may be more important for understanding delinquent conduct than age, sex, or residence. Given the association between low income and race, this finding may also explain the relatively low association

between family situation and seriousness for black boys.

What does this possibility imply about intervention? In emphasizing the possibility that an increase in the number of complete families may reduce delinquency referrals, one is reduced to deploring family disruption. If, however, the relationship between referrals for serious misconduct and family living arrangements largely results from the relationship between family income and such referrals, more practical approaches to delinquency reduction are possible.

It seems quite possible, for example, that providing a reasonable minimum income might reduce the relationship between family disruption and delinquency referral without measurably decreasing the proportion of incomplete families in the general population. Of course, such an income maintenance program might also reduce family disruption. But it, and similar approaches, would appear to have greater chance for reducing delinquency referral than attempts to have parents maintain traditional family relationships—especially in situations where the structural pressures producing low incomes also contribute to family disruption.

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⁹ For an earlier, more extensive analysis of the interrelations of family situation, economic factors, race and delinquency, see Willie (1967). Like Eisner (1966) and Fleisher (1966), Willie had to rely on census tract data for his measure of economic status. But his article is an excellent discussion of the importance of these relationships for evaluating the policy position implicit in the 1965 "Moynahan Report" (U.S. Department of Labor). The problem is also concisely presented in Short's (1968:6-9) discussion of the work of Shaw and McKay.

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THE CREDIBILITY OF PROTEST *

DAVID L. ALTHEIDE AND ROBERT P. GILMORE

Southern Colorado State College

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Data are presented from a study on the public perception of protest. Turner (1969) listed five categories that affect the public perception of protest. Findings are presented for several hypotheses derived from one of these general categories—credibility and communication. One hypothesis received more support than others; with one exception, all were in the predicted direction. The exception was that middle-class people were not more likely than other classes to define an event as a demonstration or protest. The most important predictor variable was ethnicity: Mexican-Americans were most likely to view favorably the focal incident for this study. Other variables that affect the definition of an event are discussed.

THIS paper reports a testing of Turner's (1969) hypotheses on conditions that affect the public perception of protest. Turner has generally stressed the social context of protest and, specifically, those factors which define an event as deviant behavior, or protest, or a demonstration. He has argued that this difference is important since protests and demonstrations may be regarded as respectable and useful; thus to define a disturbance as protest is often a positive sanction; and demonstrations have become legitimate forms of manifesting discontent (Etzioni, 1969). However, many persons do not consider protests or demonstration as legitimate and may treat the participants as troublemakers, revolutionaries and the like. Turner (1969:824) suggests five possible reactions to a protest or demonstration. Facing a possible conflict with protestors, the dominant group may: (1) surrender (but this is not a viable choice); (2) define the disturbance as deviance and thereby ignore

or depreciate the conflict; (3) define the event as rebellion and use force to suppress it; (4) accept the legitimacy of the protest and identify with the dissidents (at least symbolically); (5) offer conciliation to the dissidents.

Some will recognize the *right of dissent* but not *dissent* itself. They are less likely to be sympathetic to demonstrators even if the demonstrators' rights are abused or if the demonstrators themselves are physically injured.¹ That a protest definition connotes

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¹ This helps account for the general disbelief of the findings of the Walker Report, *Rights in Conflict*, on the confrontation during the 1968 Demo-

recognition of intent is implied by Turner's (1969:822) statement that "Crime and rebellion are in an important sense easier interpretations to make since they can be inferred from the most conspicuous and superficial aspects of behavior, without a search for the motives and grievances behind the violence and disruption."

Working from this presumed relationship between differing definitions of legitimate dissent and reactions to events subject to those definitions, Turner offered several general propositions about the various dimensions of "reality construction" by actors and their concomitant behavior (Berger and Luckman, 1967; Holzman, 1968). We combined, and in some cases reduced, several of these propositions into hypotheses to be tested. This paper reports findings bearing on hypotheses from one set of Turner's postulates—Credibility and Communication.

TURNER'S THESIS

Turner (1969) defines protest as an expression or declaration of objection, disapproval, or dissent often in opposition to something a person is powerless to prevent or avoid. Elements differentiating protest from rebellion and revolution are as follows: (1) the action expresses a grievance, a conviction of wrong or injustice; (2) the protestors are unable to correct the condition directly by their own efforts; (3) the action is intended to draw attention to their grievances; (4) the action is further meant to provide eventual, ameliorative steps by some *target group*; (5) the protestors depend upon some combination of sympathy and fear to move the target group in their behalf.

Social protest is an expression of grievance that intends to provoke ameliorative action; it is distinguished from rebellion according to the ultimate goal and whether the disruptions are meant as communication or direct action. This is the general framework of our study. Turner's focus was on the major urban disturbances rather than a campus

disagreement over the sale of beer—the subject of our investigation. However, most of the expected relationships did occur, offering tentative support for Turner's ideas.

THE STUDY

This study was undertaken to assess public reaction to a demonstration involving college students at Southern Colorado State College in Pueblo, Colorado. The facts are as follows. A brewery had been charged with unfair employment practices against Blacks and Chicanos (the state Civil Rights Commission later ruled that discrimination had occurred in *one* of these cases). Prior to the incident, a student body referendum to remove the allegedly discriminating brewery's beer from the campus had failed. Several days after the referendum vote, approximately fifty high school and college Chicano students entered the campus pub and requested that this particular brand not be sold until after the state Civil Rights Commission rendered a verdict on the charges against the brewery. When this request was denied, the students blocked the entrance to the pub, permitting certain people to enter, although anyone was permitted to leave. Consequently, criminal charges were brought against the participants, including a temporary restraining order (all of which were later dropped). These matters received widespread coverage in all of the local media.

Over a two-day period (April 27–29, 1970), we administered a twelve-page questionnaire to the student population of four classes in various departments on campus, a simple random sample of faculty members, and the total population of college administrators.² In addition, we mailed questionnaires—accompanied by a cover letter referring to the "beer incident"—to a stratified simple random sample of residents of the city, requesting the respondent (preferably the head of household) to help in describing what citizens thought about this and other events.³ At the "cutoff" date (May 8) we

cratic Convention in Chicago. This report placed the major responsibility on police. One poll showed that the majority of people did not agree with the Walker Commission's findings. The subsequent trial of the "Chicago Seven" met with a similar response—71% of a national sample felt it was a "fair trial" (Harris, 1970).

² The timing of data collection was critical. Since we did not have a questionnaire prepared, we were rushed to complete one and pretest it; the rush resulted in several of our measures being somewhat crude. The incident occurred on March 6.

³ Prior to this incident, the code of an organization of junior and senior high school Chicano

had a 54% return, including usable questionnaires from 284 students, 27 faculty members, 16 administrators, and 159 residents of the city for a total of 486.

HYPOTHESES

The collective action of the students at the campus pub seems to meet Turner's definition of a protest or demonstration. But, an event takes on meaning according to the "folk concept," the criteria against which people judge whether or not an event meets their definition of an incident (Turner, 1957). The incident in this case was social protest.

We derived three hypotheses from Turner's discussion on Credibility and Communication—components of the "folk concept" in American society (Turner, 1957; Boskin, 1968). According to the "folk concept," a very small number of participants predisposed observers to see them as deviants, agitators, or revolutionaries. We suggest the following Hypothesis 1: the more participants are seen as constituting a major part of a group whose grievances are known, the more credible they are as protestors.

Pointing out that groups differ in their likelihood of defining an incident as a protest, Turner (1969:819) argues, "Crucial here is the assumption that objectives and detached comparison between the situations of the trouble-makers and the target group is less powerful in shaping the assessment of injustice than the observing group's position vis-a-vis the troublemakers." Groups who see themselves as more disadvantaged are not likely to regard disturbances as legitimate because they are more easily comprehended as power plays or deviance. Groups seeing their situation as about the same as the protestors also find it difficult to accept the protest interpretation (Turner, 1969). From this general explanation we derived the most inclusive Hypothesis 2: more advantaged groups, i.e., the dominant group, will more readily accept the claim of injustice than will those groups less advantaged. In addition, three subhypotheses were drawn: (2a) the

middle-class is more likely to interpret events as protests than the working class groups; (2b) those who see their situation as about the same as that of the protestors are less likely to accept the credibility of protest; (2c) respondents will perceive that college students have less basis for social protest than do disadvantaged people.

Turner (1969:820) notes that the credibility of protest is reflected in the tendency to not believe that massive crime, protest or rebellion is taking place. Each person's security system is anchored in an assumption that he is integrated in society. If widespread crime and deviance become extensive and blatant, the assumption that society is integrated on the basis of major values is shaken: "people who understand society as a sort of jungle accommodation will find it easier to interpret disturbance as criminal outbursts . . . therefore, the belief in widespread protest calls into question the mechanics of society's operation, but not necessarily the value consensus." We defined the view of "society as a sort of jungle accommodation" as *individualism*, and the belief in widespread protest questioning the "mechanics of society's operation" as *social instability*. We derived the following Hypothesis 3: the more people view society as unstable and with an individualistic orientation, the less likely they will be to consider collective action as legitimate.

FINDINGS

Table 1 shows how each subpopulation defined the incident. Students were *least* likely to define the event as a group of trouble-makers (37%) while administrators were the most likely (50%).⁴ On the other hand, faculty members and students were *most* likely to view the incident as a demonstration or protest (52% and 49% respectively), while administrators (25%) and citizens (22%) were less likely to do so.

Since Turner argued that other variables play a large part in how events are defined,

⁴The distinction between administrators' attitudes and the attitudes of the rest of the college community was predicted by Lipset (1968). In keeping with this assumption, Turner argued that college and university faculties were more likely than administrators to define an event as social protest.

students was published in a local newspaper, along with the fact that they wore black berets. This sparked a great deal of controversy. Several of these youth participated in the "beer incident."

Table 1. Classification of Respondents on the Dependent Variable:
Per cent* Responding by Subpopulation to "How would you
describe the 'Beer Incident'?"

	STUDENTS	FACULTY	ADMIN.	CITIZENS	TOTAL (N)	
A communist plot	1%	7%	0%	4%	2%	12
A revolution	1	0	6	1	1	4
Rebellion against authority	6	0	12	15	9	44
A few troublemakers causing trouble	37	41	50	45	40	196
Demonstration or protest	49	52	25	22	40	193
Don't know anything about it	2	0	0	5	3	15
TOTALS	96* (N=275)	100 (N=27)	93* (N=15)	92* (N=147)	95*	464

* Total percent is less than 100 due to multiple or omitted responses.

it was essential to determine how much respondents knew about this particular event in order to place any confidence in our discussion of their interpretation. If people are familiar with some of the facts, and if they arrive at different conclusions, we have increased confidence that their interpretations are (1) at least partially empirically grounded, and (2) that something other than the facts have affected their interpretation.

Data for our only "factual" question are presented in Table 2. The correct response was 25 to 100.⁵ The majority of our sample

⁵ This "fact" variable was a predictor for another of Turner's propositions—the fewer the number of participants, the greater the tendency to view them as deviant, revolutionary, or agitators.

Table 2. "How many people would you say took part in the 'Beer Incident'?"

	Students	Faculty	Admin.	Citizens
Over 300	2%	0%	0%	3%
200-300	1	0	0	6
100-200	8	4	5	5
25-100	69	89	88	48
Don't Know	18	7	0	33
TOTAL	98%* (N=272)	100% (N=27)	93%* (N=15)	95%* (N=158)

*Total percent is less than 100 due to nonresponse.

correctly assessed the number of participants. An indication that social perception affected interpretation of the same fact, viz., the number of people in the demonstration, is shown by the percentage distribution and the low lambdas (Table 3). Even those who knew at least one of the facts did not agree on the definition of the event; i.e., knowledge of the size of the demonstration was not a deciding factor in how observers interpreted the disturbance.

There were two main ethnic groups in our sample. White (Anglo) and Brown (Mexican-American, Chicano).⁶ The following will show that ethnicity was the strongest predictor in defining the incident.

Eighty-five percent of all respondents claimed to know what some of the Mexican-Americans' problems were (data not presented).⁷ Even though their problems were

⁶ Pueblo has a wide array of ethnic communities, several quite strong in ethnic, religious, occupational and other cultural ties. However, our concern was with Mexican-Americans as opposed to other groups or, more correctly, dominant whites. The area is quite unique in that fewer than 3% of the population is black. We stratified our sample to make certain that a proportionate number of Mexican-Americans were included, although only 19% returned questionnaires.

⁷ According to the U.S. Censuses of Population and Housing, 1960, for Pueblo, Colorado, 52% of the Mexican-Americans lived in 6 of 27 census tracts—all areas with the most deteriorating and dilapidated housing—40% of Mexican-Americans living in deteriorated or dilapidated housing. In addition, their median income was over \$1,000 less than that of the dominant group. These figures are

Table 3. "How many people would you say took part in the 'Beer incident'?"
Percent Defining the Incident as a Demonstration or Protest

	Students	Faculty	Admin.	Citizens
Over 300	20% (5)	- (0)	- (0)	20% (5)
200-300	* (4)	- (0)	- (0)	13% (8)
100-200	62% (21)	- (1)	* (0)	13% (8)
25-100	49% (193)	54% (24)	31% (13)	24% (70)
Don't know	55% (49)	- (0)	- (0)	29% (51)
Lambdas	.02	.08	.14	.01

*Base N less than 5.

recognized by most of our respondents, only 40% (Table 1) defined the incident as a demonstration of protest. Our first hypothesis led us to expect that the participants were not considered representatives of the Mexican-American community. The data are presented in Tables 4 and 5. Only a small percentage responded that the incident represented the Mexican-American community's feelings and would be approved by them. The Mexican-American respondents did not agree. Thus, our findings indicate that the most important thing we could know about a respondent to predict correctly how he would evaluate the campus disturbance was whether or not he was a Mexican-American (Table 6). Mexican-American students and adults had one thing in common: they differed from the dominant group in attitudes about (1) defining the event as a demonstration or protest (Table 6); (2) the demonstrators representing the Mexican-American community (Table 4); (3) how much Mexican-Americans would approve of the disturbance (Table 5).

The second part of the first hypothesis deals with perception of the powerlessness and grievances of the Mexican-American com-

munity (Table 7). Recognition of the Mexican-Americans' subordinate position was the best predictor for dominant group members' definition of the incident. For at least three of our subpopulations, awareness of the plight of the protesting group had a noticeable effect on how the disturbance was defined—lambdas of .20 for students, .60 for faculty, and .56 for citizens.

Our first hypothesis was supported. Those who considered the participants as representatives of the Mexican-American community (disproportionately Mexican-American respondents), and those aware of their plight compared to other groups, were more likely to define the disturbance as a demonstration or protest.

Findings on second Hypothesis. Social class has been an important predictor variable (Gordon, 1963; Bendix and Lipset, 1966; Bottomore, 1966; Coleman, *et al.*, 1966; Kohn, 1969; Wilson, 1969). Subhypotheses A and B imply that middle-class people will not view their situation the same as more disadvantaged groups, i.e., the ones protesting, and that they would be more sympathetic with the Mexican-American com-

Table 4. "Do you think those who took part in the 'Beer Incident' were representing the Mexican-American (Chicano) community's feelings?"

	Yes	No	Don't Know	TOTAL
All Students	14%	61%	25%	100% (N=274)
Anglo Students	10	64	26	100 (N=219)
M. A. Students ^a	38	50	12	100 (N=40)
Faculty	0	72	28	100 (N=25)
Administrators	13	80	7	100 (N=15)
All Citizens	10	69	21	100 (N=146)
Anglo Citizens	5	75	20	100 (N=117)
M. A. Citizens	39	35	26	100 (N=23)

^aIncludes 4 black students.

for the city; the rural area is even more impoverished. See *Mexican-American Study Project: Advance Reports*, 1-10, U.C.L.A., 1967, and Grebler *et al.*, (1970).

munity. Therefore, it was expected that middle-class people would be more likely to define the incident as a demonstration or protest. Our index of social class was a combination of occupation, education, and income. Four classes were derived from the community: upper-middle, lower-middle, working, and lower (Table 8). The upper-middle class was *not* more likely than the other classes to view the "beer incident" as a demonstration or protest. Overall, social class accounted for little of the variation.

One reason why social class did not have the expected effect may be that the issue was the selling of beer rather than rioting in an urban center. However, another plausible reason may be that there are regional differences in class correlates.

Knowlton (1965a:3) has noted that the Southwest is marked by a greater cultural and ethnic diversity than almost any other region in the United States. This diversity appears to have originated in three centuries of subsistence living which sets the region

Table 5. "How much would the majority of Mexican-Americans (Chicanos) in Pueblo approve of the 'Beer Incident' at the College Pub?"

	Approve			TOTAL
	Yes ^a	Not Care	No ^b	
All Students	31%	29%	40%	100% (N=261)
Anglo Students	26	30	44	100 (N=222)
M. A. Students ^c	55	23	22	100 (N=40)
Faculty	24	32	44	100 (N=25)
Administrators	13	18	69	100 (N=16)
All Citizens	23	26	51	100 (N=137)
Anglo Citizens	15	27	58	100 (N=112)
M. A. Citizens	57	22	21	100 (N=23)

^aThe categories of "strongly approve" and "approve" were combined.

^bThe categories of "disapprove" and "strongly disapprove" were combined.

^cIncludes 4 black students.

Table 6. Percent Defining the Incident As a Demonstration or Protest

	Negro (Black)	Mexican- American (Chicano)	White (Anglo)
Students	* (4)	86% (36)	44% (221)
Faculty	- (0)	* (1)	52% (26)
Admin.	- (0)	- (0)	25% (15)
Citizens	- (0)	65% (23)	25% (132)

*Base N less than 5.

apart from the rest of the country. The mixing of Indian, Hispano, Mexican, and Anglo cultures, ranging from friendly cooperation to open hostility, resulted in a general lack of respect for the culture of Spanish and Mexican heritage (Hollon, 1967:7; Grebler *et al.*, 1970:35-100). Furthermore, the industrial boom has not benefited the Southwest as it has other regions. The gradual overcoming of economic depression resulted in an influx of people from other regions and has "inexorably altered the local landscape, people, traditions and politics" (Hollon, 1967:335). In spite of the recent radical changes in the Southwest, its people have maintained a parochialism and a sense of unreality about their problems; business and professional leaders tend to ignore serious social problems such as slums, intergroup relations, urban planning, zoning, housing codes, education—problems that most other regions were dealing with as long as fifty years ago (Knowlton, 1965b:4; Grebler *et al.*, 1970). Pueblo, Colorado, reflects many of these conditions.

Table 9 shows that Turner's hypothesis—those who see their situation as about the same as that of protestors are less likely to accept the credibility of protest—receives support. It will be recalled that this is the converse of the first hypothesis, except that the former was more *group* oriented, whereas this one focuses on individual comparisons. Those who chose the first response (except for administrators) were more likely to define the pub incident as a demonstration or protest, while those with "the same amount of problems" as Mexican-Americans were

less likely to offer this interpretation. These findings support Turner's expectations.

Data for the third subhypothesis are presented in Table 10. Administrators were the only subpopulation affected by this variable ($\lambda = .43$). The less respondents agreed that college students had a lot to protest about, the less likely they were to define the incident as a demonstration or protest. In our college population, proportionately more administrators (7 of 14) said college students had "very little" or "nothing" to protest about.

The underlying motivations of any student protest might be overlooked in favor of the general view that they are students first, and second, students with a particular problem. Administrators are in a strategic position to affect college policy on student activism, but they did not consider this incident to be legitimate. Paradoxically, it might be detrimental to minority group students if they are viewed as equal or similar to the rest of the student body rather than students with special problems. Regardless, Turner's hypothesis was supported by the percentage distributions although accounting for little variation.

Third Hypothesis. Data for the last hypothesis tested are presented in Tables 11 and 12. The key concepts are *perceived instability* of society and an *individualistic*

Table 7. "How much harder do you think Mexican-Americans (Chicanos) have it compared to other groups in Pueblo?"
Percent Defining the Incident As a Demonstration or Protest

	Stu- dents	Faculty	Admin.	Citi- zens
A lot harder	86% (51)	100% (8)	* (4)	67% (12)
A little harder	54% (111)	63% (8)	29% (7)	45% (40)
About the same	31% (98)	13% (8)	* (4)	11% (80)
A little easier	60% (5)	* (2)	- (0)	0 (7)
A lot easier	0 (6)	- (0)	- (0)	* (5)
Lambdas:	.20	.60	.14	.56

*Base N less than 5.

Table 8: Definition of the Incident by Social Class (Percent)

Definition	Social Class			
	Upper- Middle	Lower- Middle	Work- ing	Lower
Communist Plot	-	-	14%	-
Revolution	-	2%	-	-
Rebellion	13%	13%	19%	-
Trouble-makers	65%	60%	31%	44%
Demonstration or Protest	22%	23%	29%	22%
Don't Know	-	2%	7%	34%
TOTAL	100% (N=23)	100% (N=48)	100% (N=42)	100% (N=9)
Lambda .15				

*orientation.*⁸ These concepts are linked to current discussions on social legislation in general and social welfare in particular—the notion of collective responsibility and interdependence vs. individualism (Hauser, 1969: 1-19). The majority of respondents chose the second category (Table 11), indicating a belief in individual responsibility. Those indicating an individualistic orientation (choice 1 or 2 on Table 11) were less likely to give the protest interpretation. This is most clearly seen with administrators—one of ten who agreed that a person is mostly responsible for what happens to him defined the incident as a protest. The data offer cautious support for Turner's proposition.

Table 12 demonstrates that Turner was again at least partly correct in his assessment of the relationship between social instability and the credibility of protest. This was true to some extent for all four groups, but little of the variation in the perception of protest was accounted for by perceived instability of society.

⁸ Our understanding of individualism is perhaps cultural rather than sociological. It includes elements of extreme individual responsibility for anyone other than one's immediate group, perhaps only kinship ties. Circumstances are viewed as being overcome by individual initiative and, therefore, are not seen as decisive in one's future.

Table 9. "How do you consider your problems compared to those of Mexican-Americans (Chicanos)?"
Percent Defining the Incident As a Demonstration or Protest

	Stu- dents	Faculty	Admin.	Citi- zens
They have a LOT more problems	86% (55)	88% (8)	* (4)	53% (17)
They have a FEW more problems	52% (121)	45% (11)	20% (5)	29% (35)
Everybody has about the same amount of problems	24% (63)	25% (8)	40% (5)	18% (73)
They have a FEW less problems	40% (5)	- (0)	- (0)	* (3)
They have a LOT less problems	33% (6)	- (0)	- (0)	* (4)
Don't Know	33% (18)	- (0)	- (0)	0% (6)
Lambdas:	.16	.38	.00	.12

*Base N less than 5.

DISCUSSION

Our research supports Turner's contention that knowledge of an event (as indicated by a few "facts") had little effect on how people defined the disruption. Several other factors were expected to affect how the public perceives particular incidents.

Three hypotheses were derived from Turner's theoretical discussion; all received some support but only for one was knowledge of choices on the predictor variable a strong indication of their perception of protest. This was, "the more participants are seen as constituting a major part of a group whose grievances are already documented, the more credible they are as protestors." The only hypothesis that was not supported was that middle-class persons would be more likely to interpret events as protests than would working-class persons.

FOLK CONCEPT

An essential element of the public perception of protest is the "folk concept." To further clarify the role of the "folk concept" in perceiving an event, we combined our measure of individualism with the best predictor for the dominant group (Table 7). The importance of "perceived individualism" in achieving success can be viewed as a cultural or background factor. We expected that these responses would interact with many other indicators, especially perception of the Mexican-Americans' subordinate position.

Each question (Tables 7 and 11) had five choices. We dichotomized each—(1, 2 and 3, 4, 5)—and combined the dichotomies into four combinations. Table 13 shows the combinations and the percent of dominant group students and citizens who defined the incident as a demonstration or protest.

We expected the second category to have the highest percentage offering a protest interpretation because if individual initiative is not believed to be the best explanation for a person's future, then perhaps being a minority group member is. If the group's subordinate position is recognized, it is more likely that their collective action will be con-

Table 10. "Compared to other groups, how much do college students have to protest about?"

	Percent Defining the Incident As a Demonstration or Protest			
	Stu- dents	Faculty	Admin.	Citi- zens
Much more	87% (37)	* (1)	* (2)	63% (8)
A little more	56% (52)	* (1)	- (0)	67% (6)
About the same	46% (89)	62% (13)	* (3)	40% (20)
Very little	38% (64)	44% (9)	17% (6)	27% (52)
Nothing	21% (14)	* (2)	* (1)	4% (46)
Don't Know	62% (13)	* (1)	* (2)	20% (10)
Lambdas:	.07	.08	.43	.07

*Base N less than 5.

Table 11. "Which of the following best describes what happens to a person?"
Percent Defining the Incident As a Demonstration or Protest

	Student	Faculty	Admin.	Citizens
A person's BACKGROUND has NOTHING to do with it	13% (8)	- (0)	- (0)	33% (6)
A PERSON is MOSTLY responsible for what happens to him	43% (156)	50% (20)	10% (10)	23% (114)
A PERSON is ONLY PARTLY responsible for what happens to him	65% (60)	67% (6)	* (2)	39% (13)
CIRCUMSTANCES are MORE IMPORTANT than a person's desire	67% (24)	- (0)	- (0)	20% (5)
ONLY CIRCUMSTANCES are responsible for a person's failure or success	71% (7)	- (0)	* (1)	* (4)
Don't know	54% (13)	* (1)	* (1)	- (0)
Lambdas:	.11	.08	.29	.00

*Base N less than 5.

sidered legitimate. This was truer for students than citizens.

With similar reasoning, we expected the third category to have the lowest percentage giving a protest interpretation. If an indi-

Table 12. "How would you describe society today?"
Percent Defining the Incident As a Demonstration or Protest

	Stu- dents	Faculty	Admin.	Citi- zens
More un- stable than any time in history	59% (68)	* (1)	* (1)	30% (47)
Less stable than it should be	41% (108)	47% (17)	14% (7)	20% (69)
About as stable as it ever has been	47% (53)	67% (6)	40% (5)	14% (22)
More stable than it has been	64% (11)	* (1)	- (0)	* (1)
Too stable not enough change	77% (26)	* (1)	* (1)	* (3)
Lambdas:	.04	.08	.29	.06

*Base N less than 5.

vidual is mostly responsible for what happens to him, then belonging to an ethnic group would not matter, for the individual would be the main factor in his future. According to the criteria of this "folk concept"—high individualism and equal opportunity—collective action by Mexican-Americans would usually be considered illegitimate. Generally, this was true for students and citizens if the small number of citizens in category 4 is considered, although individualism appears to have been more salient for the latter.

"Perceived individualism" as a background variable thus accounted for additional variation in definition of the incident studied and interacted with variables pertaining to the "folk concept" in general and the public perception of protest in particular. We have presented one example of this interaction.

Finally, our research suggests the usefulness of being "close to the action," at least in understanding the context of this particular disturbance and the meaning it had for those involved. This has aided our interpretation considerably, while hopefully not affecting our objectivity.

It is hoped that Turner's hypotheses will be tested in various parts of the country and with differing incidents in order to further clarify the "credibility of protest." Our research has shown that this is a viable concept and that indeed a great number of social variables intervene between a "fact" and its interpretation. Only if this is further checked

Table 13. Percent Defining the Incident as a Demonstration or Protest

Individualism and subordinate position*	Dominant Group	
	Students	Citizens
High Indiv. and M.A.s have it harder	48% (64)	30% (30)
Low Indiv. and M.A.s have it harder	61% (61)	29% (7)
High Indiv. and M.A.s don't have it harder	25% (87)	10% (84)
Low Indiv. and M.A.s don't have it harder	50% (18)	0% (8)

*High Indiv.=First or second response, Table 11.
 Low Indiv.= Third, fourth, or fifth response, Table 11.
 M.A.s have it harder=First or second response, Table 7.
 M.A.s don't have it harder=Third, fourth, or fifth response, Table 7.

can we hope for grounded theory on such a crucial aspect of the contemporary scene—collective behavior in general and social movements in particular

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HALL'S PROFESSIONALISM SCALE: AN EMPIRICAL REASSESSMENT *

WILLIAM E. SNIZEK

Western Kentucky University

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A growing number of sociologists seek to determine appropriate criteria and measurement of individuals' professional standing. One notable attempt in this regard has been the professionalism scale recently developed and employed by Richard Hall. Using Likert scaling procedures, Hall measured five attitudinal components of professionalism: use of the professional organization as a major referent, belief in public service, belief in self regulation, sense of calling to the field, and a feeling of autonomy. By using rotated factor matrices, I assessed the empirical "fit" of Hall's items as they relate to each of the five above theoretical dimensions of professionalism. I suggest how such a "fit" might be improved. I consider the near term desirability of using a shorter version of Hall's original scale for both stratified and total scale reliability. The data analyzed are those of vastly diverse study populations collected by both Hall and me.

INTRODUCTION

THE term professionalization, as defined by Vollmer and Mills (1966: vii-viii) refers to "... a dynamic process whereby many occupations can be observed to change certain crucial characteristics in the direction of a 'profession.'" Such characteristics may either be structural or attitudinal. The latter, that is, the attitudes and ideology held by its practitioners, denotes the degree of professionalism characteristic of an occupation.

Recently, Richard Hall (1967; 1968:92-104; 1969: Chapter 4) developed an attitude scale to measure the degree of professionalism among practitioners of various occupations. Using Likert scaling procedures, Hall used ten items to measure each of five attitudes of professionalism: use of the professional organization as a major referent, belief in public service, belief in self regulation, sense of calling to the field, and a feeling of autonomy. In this paper I suggest modifications of Hall's scale based on a factor analysis of Hall's and my original data.

*I sincerely wish to thank Richard Hall for his cooperation in allowing me to re-examine his own data and compare it with my own. I am also indebted to Rex Warland for his helpful suggestions in the area of attitude scaling. An earlier version of this paper was read at the 1971 Southern Sociological Society meetings in Miami Beach, Florida.

THEORETICAL DIMENSIONS OF PROFESSIONALISM

Using the Professional Organization as a Major Referent

Professional organizations can be both formal and informal. Such associations reinforce the values, beliefs, and identity of the profession. By attending professional meetings, and by reading professional journals, practitioners develop what Gross (1958:79) terms "colleague consciousness." Once the practitioner acquires such consciousness, he is thought to be strongly influenced by the standards of his profession.

Belief in Public Service

A practitioner believes that his profession is both indispensable and beneficial to society. Taylor (1968:124) notes:

Professionals are concerned with their economic status, and in part motivated by it. But their commitment is also to service for their fellow man as well as to maintaining and expanding the body of knowledge which is their expertise.

Since the general public has difficulty believing in the indispensability of services performed by some occupations, practitioners are often slow to develop this belief. Therefore, the degree to which they do so becomes an important attitude component for measuring professionalism.

Belief in Self-Regulation

A belief in self-regulation represents a practitioner's endorsement of the notion of colleague control. Individuals who hold such a belief argue that given the state of specialized knowledge required in their occupation, only colleagues, not "outsiders," are qualified to judge their work. To relinquish such a belief the professional would be relinquishing his control or authority over his work.

Thus, Becker (1951:136) notes of professional dance musicians that they

... consider the client unable to judge the proper worth of the service and resent bitterly any attempt on his part to exercise control over the work . . . Musicians classify themselves according to the degree to which they give in to outsiders; the continuum ranges from the extreme "jazz" musician to the "commercial" musician.

Sense of Calling to the Field

A professional is totally devoted to his work. Work is defined as an end in itself and not merely a means to an end. So total is personal commitment, (e.g. Kornhauser, 1962 and Taylor, 1968) that the professional, according to Greenwood (1957:53), "... performs his services primarily for the psychic satisfactions and secondarily for the monetary compensation." Kornhauser (1962:1) asserts that this professional value engenders, "the belief that the development and exercise of expertise is worthy of the devotion of a lifetime and carries its own reward."

Perhaps the single best description of this attitude appears in Weber's essay on science as a vocation when he discusses the importance of an "inward calling" for the scientist. (Weber, 1946:135):

... whoever lacks the capacity to put on blinders so to speak, and to come up to the idea that the fate of his soul depends upon whether or not he makes the correct conjecture at this passage of his manuscript may as well stay away from science. He will never have what one may call the "personal experience" of science. Without this strange intoxication, ridiculed by every outsider; without this passion, this "thousands of years must pass before you enter into life and thousands more wait in silence"—according to whether or not you succeed in making this conjecture; without this, you have *no* calling for science and you should do something else. For nothing

is worthy of man as man unless he can pursue it with passionate devotion.

Autonomy

Autonomy, as a professional attitude, consists of the practitioner's desire to be free to make decisions about his work. These decisions, he feels, should be made without the threat of external pressures. Such pressures at the State,¹ or organizational level,² represent the antithesis of professional autonomy. So great is his desire for autonomy that the conflicts bred by it form a major theme in both Marcson's and Kornhauser's studies (Kornhauser, 1962:45):

Thus the basic dilemma in the social control of applied research centers on the need for executive coordination of research with other parts of the enterprise versus the need for professional autonomy in research to give wide scope to the scientist's "controlled imagination" and motivation to make original contributions.

RESEARCH DESIGN

To operationalize these dimensions, Hall used a total of fifty attitude items, ten for each of the five above components of professionalism. Survey responses were elicited to comply with standard Likert scaling procedures. Appendix I of this paper contains a complete copy of Hall's fifty item scale.

The following may be said of the exact number and types of occupational practitioners surveyed by Hall and me. Hall's data were based on 328 subjects from eleven occupational groups: physicians, nurses, accountants, teachers, lawyers, social workers, stock brokers, librarians, engineers, personnel managers, and advertising executives.³ My data were gathered from 566 aeronautical, nuclear and chemical engineers, physicists and chemists. In both samples, individuals had completed varying academic degrees: B.A. or B.S., M.A. or M.S., Ph.D., and M.D.

¹ For a discussion of the affinity between the autonomy of science and the pluralistic society, see, Edward A. Shils, (1956:176-191).

² Both Simon Marcson (1960), and Kornhauser (1962), examine at length the conflict between the professional's need for autonomy and the organization's attempts to control this autonomy.

³ Hall used data referred to in this connection in writing the article cited earlier. See Hall (1968).

Table 1. Orthogonal Varimax Rotations from Principal Axes Solutions for Five Factors: Hall v. Snizek Data

Theoretical Dimensions		Empirical Dimensions									
		Hall Data					Snizek Data				
Items	No.	I	II	III	IV	V	I	II	III	IV	V
I	* 1	.56	-.05	-.01	.18	.27	-.42	-.26	.34	-.15	-.02
	* 6	.58	-.00	.15	.23	.13	-.24	-.27	.22	.06	.04
	11	.31	.12	-.35	.38	.16	-.16	-.09	.16	.38	-.10
	* 16	.55	.03	-.03	.13	-.04	-.16	-.15	.35	.15	.04
	21	.17	.11	-.24	.35	-.00	-.29	.10	.06	.43	-.04
	* 26	.52	.15	-.12	-.03	-.03	-.44	-.25	.39	.26	.07
	31	.04	.04	-.14	-.19	.19	-.07	.03	.37	-.16	-.13
	* 36	.54	-.10	.02	-.04	.21	-.31	-.30	.37	-.17	-.08
II	41	.21	.29	-.21	-.06	.11	-.07	.12	.20	.19	-.07
	46	.20	.29	-.36	.11	.12	-.09	-.13	.52	.04	.02
	* 2	-.09	.68	.01	.07	.14	.07	-.66	.08	.01	-.05
	* 7	-.09	.55	.05	.35	.06	-.20	-.50	-.24	.21	-.10
	* 12	.13	.47	.06	.27	-.02	-.15	-.53	.15	.09	-.02
	* 17	.07	.70	.09	.02	.11	.07	-.64	-.03	.13	-.05
	22	-.04	-.13	.12	.60	.05	-.63	-.07	.01	.21	.05
	27	.04	.13	-.12	.47	.05	-.41	-.15	.04	.36	.04
III	32	-.09	.23	-.36	.36	.07	-.47	-.21	.09	.29	.01
	37	-.02	.05	.11	.59	-.04	-.60	-.18	.03	.19	.12
	42	-.15	.01	-.11	.08	.20	-.17	.21	.01	.15	.02
	* 47	.06	.63	.07	.29	.01	-.27	-.31	-.10	.22	.01
	3	.14	-.03	-.20	.13	.14	-.31	.11	.17	.08	-.04
	* 8	-.09	-.17	-.57	.28	.07	-.07	.27	.31	.36	-.02
	13	.26	.33	-.50	-.19	-.09	.07	.02	.32	.31	-.01
	* 18	.32	.01	-.45	-.06	.02	.11	-.08	.56	.02	-.02
IV	23	.40	.07	-.16	-.12	.01	-.53	.12	-.24	-.08	.02
	28	.15	.27	-.45	-.05	.04	-.02	.08	.14	.40	.01
	* 33	.03	-.13	-.68	-.04	.04	.02	.11	.64	.10	-.11
	* 38	.34	.33	-.38	-.22	.07	.27	-.08	.27	.22	-.03
	* 43	-.05	-.12	-.67	-.03	.05	-.01	.07	.59	.11	-.01
	* 48	-.05	-.22	-.60	.22	-.01	-.03	.15	.47	.23	-.02
	4	.11	.06	-.24	.32	.11	-.17	-.09	.06	.24	-.09
	* 9	.31	.19	-.18	.50	.00	-.19	-.16	-.06	.53	-.07
V	* 14	.49	.23	-.15	.45	-.05	.01	-.25	.09	.55	-.03
	19	.17	-.06	-.24	.36	-.06	-.03	.00	.01	-.48	-.00
	* 24	.33	.15	-.16	.42	.04	-.03	-.10	.03	.68	-.07
	29	.61	.03	-.12	.14	.11	.33	-.26	.25	.41	-.08
	34	.37	.03	-.44	.05	.23	.14	-.21	.38	.19	-.14
	* 39	.32	.06	-.18	.23	.07	-.03	.04	.13	.30	-.15
	44	.00	.05	.08	.25	.01	-.27	.14	-.02	-.05	-.03
	* 49	.27	.13	-.07	.29	-.05	-.02	-.02	.02	.39	-.04
	* 5	.05	-.05	.02	.06	.71	-.04	-.05	.06	.07	-.69
	10	.06	.25	.04	-.09	.32	.04	-.30	.09	-.10	-.24
	* 15	.23	.04	-.20	-.02	.64	.05	.02	.33	.03	-.61
	20	-.08	.02	-.08	.17	.49	-.12	-.02	-.19	.24	-.55
	25	.22	.22	-.03	-.20	.37	-.03	-.12	.17	-.26	-.28
	30	.15	.11	-.27	.03	.63	-.00	-.10	.45	-.03	-.31
	35	.19	-.08	-.15	-.19	.41	.23	-.02	.22	.08	-.46
	* 40	-.06	.08	.08	.01	.55	.06	-.06	-.15	.04	-.57
	* 45	.08	-.01	-.01	.19	.71	-.08	-.01	.12	.11	-.73
	* 50	.11	.15	.09	-.06	.66	.05	-.03	-.05	.08	-.70

* Items selected as best empirically corresponding to their proper theoretical dimension. The number of each item indicates its location in the questionnaire.

RESULTS AND ANALYSIS

Using both sets of data, I employed rotated factor matrices to determine the degree of empirical "fit" of the items used to measure each of the five theoretical dimensions of professionalism. Table 1 presents the results of that analysis.

This table reveals that approximately half of the fifty items formulated by Hall

have less than an acceptable factor loading on their appropriate theoretical dimension. (High factor loadings should cluster along the major diagonals of each Table.) Items, such as 31, 41, and 42, are shown to have little, if any, discernible "fit" within any of the five theoretically established components of professionalism. Others, such as items 4, 11, 19, 21, 32, 34, 38, and 46, appear to be empirically corresponding to mul-

multiple dimensions, and as such, contribute to the gross empirical overlap of the scale's dimensions. Still other items, such as 22, 23, 27, 29, and 37, have high factor loadings on theoretical dimensions other than those originally intended. In summary, apart from dimension five (autonomy), the data pose serious questions about the scalability of many of the items.

Since the above observations generally appear to hold for both sets of data (Table 1), one is led to believe that the scale's difficulties lie with its items, rather than its sampling idiosyncracies. A study of item contents suggests several possible sources of these difficulties. One such may be the rather sporadic references to "self" noted within items. And their replacement by referents such as "the profession," "its work," and "others" detaches the respondent further. By the same token, respondents very likely interpret differently ambiguous words and phrases like "professional standards," "severe penalties," "colleagues," "competence," and even "society." To some "colleagues" may mean "professional associates," to others, "co-workers"; the two need not be synonymous. Some may interpret "society" to mean "mankind," others to mean one's immediate "clients." In summary, I believe Hall's scale would benefit from extensive item content modifications.

Table 2. Inter-Dimensional Product - Moment Correlations:
Hall v. Snizek

Dimen- sions	Hall Data 10 items	5 items	Snizek Data 10 items	5 items
1 v. 2	0.264	0.177	0.271	0.156
1 v. 3	0.377	0.161	0.334	0.179
1 v. 4	0.466	0.373	0.308	0.176
1 v. 5	0.313	0.256	0.166	0.084
2 v. 3	0.071	-0.046	0.189	0.072
2 v. 4	0.321	0.325	0.269	0.219
2 v. 5	0.167	0.158	0.089	0.105
3 v. 4	0.379	0.209	0.184	0.179
3 v. 5	0.193	0.085	0.189	0.145
4 v. 5	0.203	0.161	0.211	0.175

Table 3. Stratified Reliability
Coefficients: Hall v. Snizek

Dimen- sion	Hall Data 10 items	5 items	Snizek Data 10 items	5 items
1	0.674	0.686	0.620	0.621
2	0.676	0.742	0.656	0.640
3	0.694	0.731	0.596	0.699
4	0.711	0.703	0.455	0.583
5	0.776	0.760	0.730	0.738
All di- mensions	0.860	0.843	0.799	0.783

NEAR-TERM SCALE MODIFICATIONS

Those wishing to use Hall's scale in the immediate future and hence, unable to modify its items, may wish to delete certain of its original items (the unmarked items of Tables 1 and 2). In so doing, the scale item empirical overlap, noted earlier, diminishes considerably. Consider Table 3 which presents a series of inter-dimensional correlation coefficients computed on the basis of both the original (10) and revised (5) number of scale items. These figures reveal marked improvements in the unidimensionality of each of the scale's dimensions.

Finally, how might item deletions effect the scale's reliability? Certainly any decrease in the total number of scale items will diminish the scale's total reliability. Table 3, however, indicates this decrease to be quite minimal (Hall's data reveal a drop from .86 to .84; Snizek's data from .80 to .78). More importantly, a deletion of the specified items should generally increase the stratified reliabilities of the scale's dimensions.⁴

In summary, deleting items of the type specified commends itself pragmatically and empirically. What little theoretical breadth may be sacrificed in any of the dimensions of professionalism seems more than recompensed by their clearer more precise measurement.

⁴ All reliability coefficients presented in Table 3 were computed through the use of Kuder-Richardson Formula 20. See Guilford (1954:383).

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APPENDIX I

Professional Inventory*

The following questions are an attempt to measure certain aspects of what is commonly called "professionalism." The referent in the questions is your own profession. Each item then, should be answered in light of the way you yourself both feel and behave as a member of your particular profession.

There are five possible responses to each item. If the item corresponds *VERY WELL* (VW) to your own attitudes and/or behavior, circle that response. If it corresponds *WELL* (W), *POORLY* (P), or *VERY POORLY* (VP), mark the appropriate response. The middle category (?) is designed to indicate an essentially neutral opinion about the item. Please answer *ALL* items in one fashion or another, making sure that you have *NO MORE THAN ONE RESPONSE FOR EACH ITEM*.

- | | | | | | |
|---|----|---|---|---|----|
| 1. I systematically read the professional journals. | VW | W | ? | P | VP |
| 2. Other professions are actually more vital to society than mine. | VW | W | ? | P | VP |
| 3. A person who violates professional standards should be judged by his professional peers. | VW | W | ? | P | VP |
| 4. A person enters this profession because he likes the work. | VW | W | ? | P | VP |
| 5. I make my own decisions in regard to what is to be done in my work. | VW | W | ? | P | VP |
| 6. I regularly attend professional meetings at the local level. | VW | W | ? | P | VP |
| 7. I think that my profession, more than any other, is essential for society. | VW | W | ? | P | VP |
| 8. My fellow professionals have a pretty good idea about each other's competence. | VW | W | ? | P | VP |
| 9. People in this profession have a real "calling" for their work. | VW | W | ? | P | VP |
| 10. It is easier when someone else takes responsibility for decision making. | VW | W | ? | P | VP |
| 11. I enjoy seeing my colleagues because of the ideas that are exchanged. | VW | W | ? | P | VP |
| 12. The importance of my profession is sometimes over stressed. | VW | W | ? | P | VP |
| 13. There really aren't any penalties for the person who violates professional standards. | VW | W | ? | P | VP |
| 14. The dedication of people in this field is most gratifying. | VW | W | ? | P | VP |
| 15. I don't have much opportunity to exercise my own judgment. | VW | W | ? | P | VP |
| 16. I believe that the professional organization(s) should be supported. | VW | W | ? | P | VP |

17. Some other occupations are actually more important to society than is mine.	VW	W	?	P	VP
18. A problem in this profession is that no one really knows what his colleagues are doing.	VW	W	?	P	VP
19. Professional training itself helps assure that people maintain their high ideals.	VW	W	?	P	VP
20. I know that my own judgment on a matter is the final judgment.	VW	W	?	P	VP
21. The most stimulating periods are those spent with colleagues.	VW	W	?	P	VP
22. Not enough people realize the importance of this profession for society.	VW	W	?	P	VP
23. A basic problem for the profession is the intrusion of standards other than those which are truly professional.	VW	W	?	P	VP
24. It is encouraging to see the high level of idealism which is maintained by people in this field.	VW	W	?	P	VP
25. The fact that someone checks your decisions makes this work easier.	VW	W	?	P	VP
26. The professional organization doesn't really do too much for the average member.	VW	W	?	P	VP
27. More occupations should strive to make a real contribution to society the way my own does.	VW	W	?	P	VP
28. Violators of professional standards face fairly severe penalties.	VW	W	?	P	VP
29. Although many people talk about their high ideals, very few are really motivated by them.	VW	W	?	P	VP
30. When problems arise at work, there is little opportunity to use your own intellect.	VW	W	?	P	VP
31. The real test of how good a person is in his field is the layman's opinion of him.	VW	W	?	P	VP
32. Any weakening of the profession would be harmful for society.	VW	W	?	P	VP
33. We really have no way of judging each other's competence.	VW	W	?	P	VP
34. It is hard to get people to be enthusiastic about their work in this field.	VW	W	?	P	VP
35. There is little autonomy in this work.	VW	W	?	P	VP
36. Although I would like to, I really don't read the journals too often.	VW	W	?	P	VP
37. The benefits this profession gives to individuals and society are underestimated.	VW	W	?	P	VP
38. The professional organization is really powerless in terms of enforcing rules.	VW	W	?	P	VP
39. Most people would stay in the profession even if their incomes were reduced.	VW	W	?	P	VP
40. My own decisions are subject to review.	VW	W	?	P	VP
41. Most of my own friends are not fellow professionals.	VW	W	?	P	VP
42. It is impossible to say that any occupation is more important than any other.	VW	W	?	P	VP
43. There is not much opportunity to judge how another person does his work.	VW	W	?	P	VP
44. Most of the real rewards of my work can't be seen by an outsider.	VW	W	?	P	VP
45. I am my own boss in almost every work-related situation.	VW	W	?	P	VP
46. The profession doesn't really encourage continued training.	VW	W	?	P	VP
47. If ever an occupation is indispensable, it is this one.	VW	W	?	P	VP
48. My colleagues pretty well know how well we all do in our work.	VW	W	?	P	VP
49. There are very few people who don't really believe in their work.	VW	W	?	P	VP
50. Most of my decisions are reviewed by other people.	VW	W	?	P	VP

*Permission to use this scale or any of its items may be obtained by writing directly to Richard Hall, University of Minnesota.

COMMENT ON "COMMUNITY STRUCTURE AND INNOVATION: THE CASE OF URBAN RENEWAL"

Aiken and Alford (1970a) analyze statistically the level of urban renewal activity in U. S. cities, relating this activity to theories of innovation. In a parallel article to which they refer in the *American Political Science Review* (Aiken and Alford, 1970b), they consider public housing activity in the same cities, with the same focus on theories of innovation. The two analyses are so similar that, with the substitution of the phrase, "urban renewal" for "public housing," some two-fifths of the *APSR* paper appears in the *ASR* paper in verbatim or paraphrased form; roughly two-thirds of the *ASR* paper appears in *APSR*. The (unreported) correlation between the two dependent variables must be high, for the two analyses yield essentially identical conclusions. This seems an extreme example of how to multiply one's publica-

tions while minimizing the cost in original thought.

This Comment intends both to call *ASR* readers' attention to the issue of multiple publication and to refer them to my Communication in *APSR* (Stephens, 1971), which raises substantive questions about the public housing analysis. These questions are equally relevant to urban renewal.

STEPHEN V. STEPHENS

The Johns Hopkins University

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Items (Continued)

Structure of Scientific Fields . . ." is the initial report of a larger study of the interrelationships of university structure, the structure of science, and reputational phenomena. Her current research is concerned with the relation of influence patterns to reputation, and the role of the department chairman. **Gerald Gordon** is a professor in the Departments of Organizational Behavior, School of Industrial and Labor Relations, and Sociology, Cornell University. He is Co-director of the project on University Organization, and coordinator of an interdisciplinary research program on Graduate Education. His research interests include investigations of organization and social factors effecting both scientific accomplishment and the diffusion of technology. **Harry Perlstadt** is Instructor in Sociology and a Mentor to the Class of '75 in the College of Human Medicine, at Michigan State University. "Goal Implementation and Outcome in Medical Schools" is a part of a wider investigation into the organizational aspects of medical education and the division of labor among medical schools.

■ **Jerold Heiss** ("On the Transmission of Marital Instability in Black Families") is a Professor of Sociology at the University of Connecticut. He is continuing his research on black families, and is involved in self-evaluation studies and the adaptation of college students in a "junior year abroad" program. **Roland Chilton** and **Gerald Markle**, University of Massachusetts (Amherst) are co-authors of "Family Disruption and Delinquent Conduct. . ." Chilton is engaged in studies examining the impact of age structure, reporting practices and social and economic conditions on specific crime and delinquency rates, and in studies of self-reported crime, victimization accounts, and perceived seriousness of various criminal acts. Markle is completing a study of the potential impact of sex predetermination on fertility. He is studying the relationship of sex role ideologies to preferences for male and female children. A further research

interest of his is the potential of cigarette smoking as a new form of deviance.

■ **D. L. Altheide** and **R. P. Gilmore** ("The Credibility of Protest") are associated with Southern Colorado State College in Pueblo. Altheide is on leave to continue graduate work at the University of California, San Diego; Gilmore is Assistant Professor of Sociology. Altheide is studying everyday life as a basis for collective behavior and is engaged in research on race relations. Gilmore is writing on contemporary problems of the family and experimental solutions to them. He reports that another interest is studying interaction in lower class bars. He does not indicate just how these two research interests may be related.

■ **William E. Snizek** is Assistant Professor of Sociology, Western Kentucky University. He is continuing research in the area of occupations and professions, as well as in "meta-sociology." **Stephen V. Stephens** is Assistant Professor, The Johns Hopkins University. His principal professional interest is in understanding and evaluating the ways in which political scientists and sociologists use quantitative methods, and particularly the relationship between such methods and substantive theory.

■ Finally, as we put this issue to bed, we note the largely unsung (but much appreciated by editors) contributions to the enterprise of our Board of Editors, and especially of the following just retired Associates: Simon Dinitz, Robert Perucci, James L. Price, Howard Schuman, Karl E. Taeuber, Eugene Weinstein; and the even less heralded assistance (also much appreciated) of the approximately two hundred special readers who have worked with us since we began reviewing manuscripts on July 1, 1971. Many of their names appeared in the December, 1971, *ASR*. The remainder and many more, I am sure, will appear in December, 1972, as has been the custom.

J. F. S.



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4. If there is more than one reference to the same author and year, distinguish them by use of letters (a, b) attached to year of publication, in text and in reference appendix. [". . . as was previously suggested (Levy, 1965a:331) . . ."]
5. Enclose a series of references within a single pair of parentheses and separate by semicolons. [". . . as many have noted (Johnson, 1942; Perry, 1947; Lindquist, 1948) . . ."]

Format of References in Appendix

List all items alphabetically by author and, within author, by year of publication, in an appendix, titled "REFERENCES." Use no italics and no abbreviations. For typing format, see the following examples:

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AMERICAN SOCIOLOGICAL REVIEW

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VOLUME 37, No. 2

LEADERSHIP AND ORGANIZATIONAL PERFORMANCE: A STUDY OF LARGE CORPORATIONS *

STANLEY LIEBERSON
University of Chicago

JAMES F. O'CONNOR
University of Washington

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Leadership influence in large complex organizations, though commonly assumed to be greatly significant, is normally not studied in terms of the variance accounted for in organizational performance. The leadership effect is viewed here as a product of an organization's environmental constraints and its leadership variance. Based on sales, earnings, and profit margin data for 167 large corporations over twenty years, we compare the impact of leadership changes with yearly, industry, and company influences. Industry and company account for far more of the variance in two performance variables than does leadership, but not for profit margins after lag effects are considered. It appears that the importance of external restrictions, and hence the maximum possible leadership influence, may range widely between specific performance criteria. The second phase of the study considers industry characteristics that appear to be associated with high and low leadership influences. These results suggest a perspective on organization performance that may be applied to the leadership influence in other large organizations and political bodies, like cities, states and nations.

THE options of organization leaders are restricted by both the organization's internal structure and its social milieu. Therefore, a basic proposition in sociology is that a group leader's ability to implement goals reflects not only his distinctive qualities, but also social and environmental limits. When one talks about a leader's time being "right" or "wrong," he implies an interaction between leadership qualities, policy direction, and social environment. We recognize the inherent weakness of the great-man theory or any other theory that fails to consider a leader's bounds. As Freedman, *et al.* (1956:83), put it, "the influence which any *single* individual may have upon the development of groups (and especially the larger and more important groups which men have formed) is not so great as we frequently imagine . . . While no sociologist

would deny the assertion that certain individuals have a disproportionate influence upon the course of development of the groups to which they belong, the evidence indicates that the influence of single individuals or handfuls of individuals is seldom as decisive as the great-man theory would lead one to believe." They cite a list of inventions and discoveries independently made by two or more people (Ogburn and Thomas, 1922) to demonstrate that the cultural base limits thought and they suggest that even major intellectual and technological advancements reflect the existing culture.

Despite its fundamental nature, this approach to leadership is not fully understood by either the public or sociologists. For many, it really matters who is elected the nation's president. Likewise, a team's success is attributed to its coach's brilliance, a corporation's performance is taken to reflect its chief executive's characteristics. In academe, gossip about the consequences of a choice for department chairmen, dean, or college president implies this perspective.

* We are indebted to Lee J. Haggerty and Herbert L. Costner for helpful methodological suggestions. Raul Marin did the drafting work for Figure 1.

The extensive sociological literature on leadership in complex organizations tends to focus on leadership qualities, mechanisms of successful administration, and nuances of interpersonal relations and feelings. Limitations on leadership effectiveness are typically seen as coming from an organization's internal structure, and involving communication blocks, factional conflicts, and underlings' abortive bureaucratic devices. (Blau and Scott, 1962:165-183; Bowers and Seashore, 1966:238-250.) Although exceptions exist, one looks harder to find studies of the impact of exogenous forces on leadership effects within organizations.

Two reasons make it difficult to determine environmental constraints on leader influence. One is the absence of controls, which applies to both popular conceptions and sociological research. One never knows what might have been had a different man been leader. Would the Confederacy still exist, or indeed slavery, had Lincoln died at the time of Secession? An intriguing problem, but basically unanswerable. Only in an experimental situation can one directly measure variation in leadership influence. Leadership changes do not occur in identical situations in nature; hence it is hard to infer the degree to which organizational outcomes reflect a leadership effect as opposed to forces outside a leader's control. But small group experiments, while interesting and suggestive, seem not to closely approximate the complex set of forces influencing heads of large formal organizations. At the moment they offer little promise for the kind of issue raised here.

Malinowski's analysis of religion and magic suggests a second source of the popular emphasis on leadership. Malinowski argues that belief in supernatural forces provides the Trobriand Islanders with a feeling of control over important events which they are impotent to affect through natural means. Such are the rain, sun, and insects affecting crops, and the tides, gales, and unknown reefs affecting safety in an outrigger (1948: 28-30). Similarly, belief in a political leader's ability to alter affairs may generate a feeling of indirect control. The ability to reward or punish incumbents through the vote implies this influence. Or consider the scapegoat function in sports: the readiness to fire coaches and managers. Whether they are *un-*

successful managers or *managers of unsuccessful teams* is never determined. An organization in trouble is an organization whose leaders must shoulder the blame (Gamson and Scotch, 1964).

To be sure, a balanced approach recognizes that an organization's progress over time is molded by leaders' traits, as well as by constraints in the organization's character and environment. Schlesinger's description of the decision process in politics is consistent with the leadership approach taken here: "The decision is generally the result of an accommodation between his own views of what is wise and the felt pressures on him as to what is possible" (1962:769). The president of a wealthy university enjoys certain advantages over the president of an institution in constant financial crisis which are not reflections of their administrative abilities. Nevertheless, presidents may still differ in their ability to raise money, handle internal management problems, and develop successful strategies for dealing with their problems. The coach of an excellent team will have a better record than one with weaker players, but the success stories of various coaches who turn a mediocre into an exceptional team while changing few personnel suggest important differences between coaches.

A key research issue is simply to determine the relative importance of leadership and organizational environment for organizational outcomes. How much variance in organizational performance can be attributed to persons in top leadership roles? A second issue is more analytical, namely, the organizational and environmental characteristics affecting an administrator's bounds. If one assumes organizations have more than one goal and if, further, one assumes that the magnitude of restrictions on a leader's options may vary with the particular goal, then the problem becomes complex.

LEADERSHIP IN LARGE BUSINESSES

We compare the influence of corporate and environmental factors with the leadership effects among 167 major publicly owned corporations from 1946-1965. Using three performance variables (sales, earnings, and profit margins), we compare the influence

of changes in top management (new presidents or board chairmen) with the influence of such factors as the state of the economy, the company's primary industry, and the company's position within the industry. A great advantage in using business organizations is that the dependent variables can all be quantified. One reads of President Kennedy's incredible difficulty in changing the State Department (see Schlesinger, 1965: Chapter 16), but it is hard to measure such phenomena and hence determine leadership's role in such events.

Corporate performance is not only a reflection of administrative competence. The literature in complex organizations makes clear that varied forces reduce and muffle a leader's impact (for representative examples, see Thompson, 1967:132-133; Lipset, 1968: 307-329; Guest, 1962). Beyond this, an organization's formal structure plays a role. Take General Motors, for example. A recent description of the top management committee structure reveals heavy bounds on the chairman's options (Investor's Reader, 1971:22-23). He may spend no more than \$250,000 on a project without others' approval. This amount is relatively small for a company of such size. The executive committee's permission is required for projects up to \$2.5 million and projects beyond that figure require the finance committee's consideration. The chairman's description of his role reveals these limits. "My primary job is to reconcile different viewpoints and arrive at a consensus. Occasionally, one must make individual decisions. In those cases, it's my responsibility." (p. 23)

Aside from these well-researched controls within an organization, the environment of the organization imposes heavy limits on a leader. For business administrators, these include: competitors (both foreign and domestic); an array of government regulations and policies that affect everything from the product to interest rates; unions; consumer demands; conditions in other industries that are suppliers, consumers or transporters of the company's products; market factors affected by internal population redistribution; the state of the economy; and technological considerations. Undoubtedly, a far more extensive list could be made, but this will suffice to suggest the bounds that are, at

most, only partly subject to an administrator's control.

A description of the steel industry by Demaree (1971) is a good illustration of these bounds on management in major industry. In recent years, there have been Presidential pressures against price increases, a long strike against the industry's largest single customer, a steel hauler's strike and sharply increasing costs of input materials. Contract negotiations with the United Steel Workers affect wages and, therefore, production costs. Further, because steel-making technology has changed radically, the earlier conventional economics of the process have become obsolete. Demaree (p. 76) cites a new \$1.1 billion plant started in 1961 but not completed until 1969, long after technological and market changes had totally disrupted the earlier plans. In addition, pressures from important customers like the auto and appliance industries have affected the industry's concern with product quality. Also the industry is under growing competitive pressures from both the use of substitute metals and foreign steelmakers. Finally, options are also restricted by new regulations governing pollution control.

The point of all this is not to gain sympathy for steel company leaders, but to illustrate the kinds of pressures restricting leadership. Graphically, the close correlation in sales over a twenty-year span among the nation's eighth largest steel companies is striking (Figure 1). There are plenty of wiggles in the lines which are not entirely parallel for all the companies and which may possibly reflect administrative qualities, but the overall pattern suggests that a common set of forces influences sales.

DATA AND METHODS

Because the data we use are not commonly employed by sociologists, some attention should be given to their meaning, source, and use. The three dependent variables: sales, net earnings, and profit margins, are obtained for each of 167 companies located in thirteen different industries for each year of the period 1946 to 1965. All three variables describe related, but distinct characteristics important to any profit-seeking enterprise. Sales figures tap the company's market

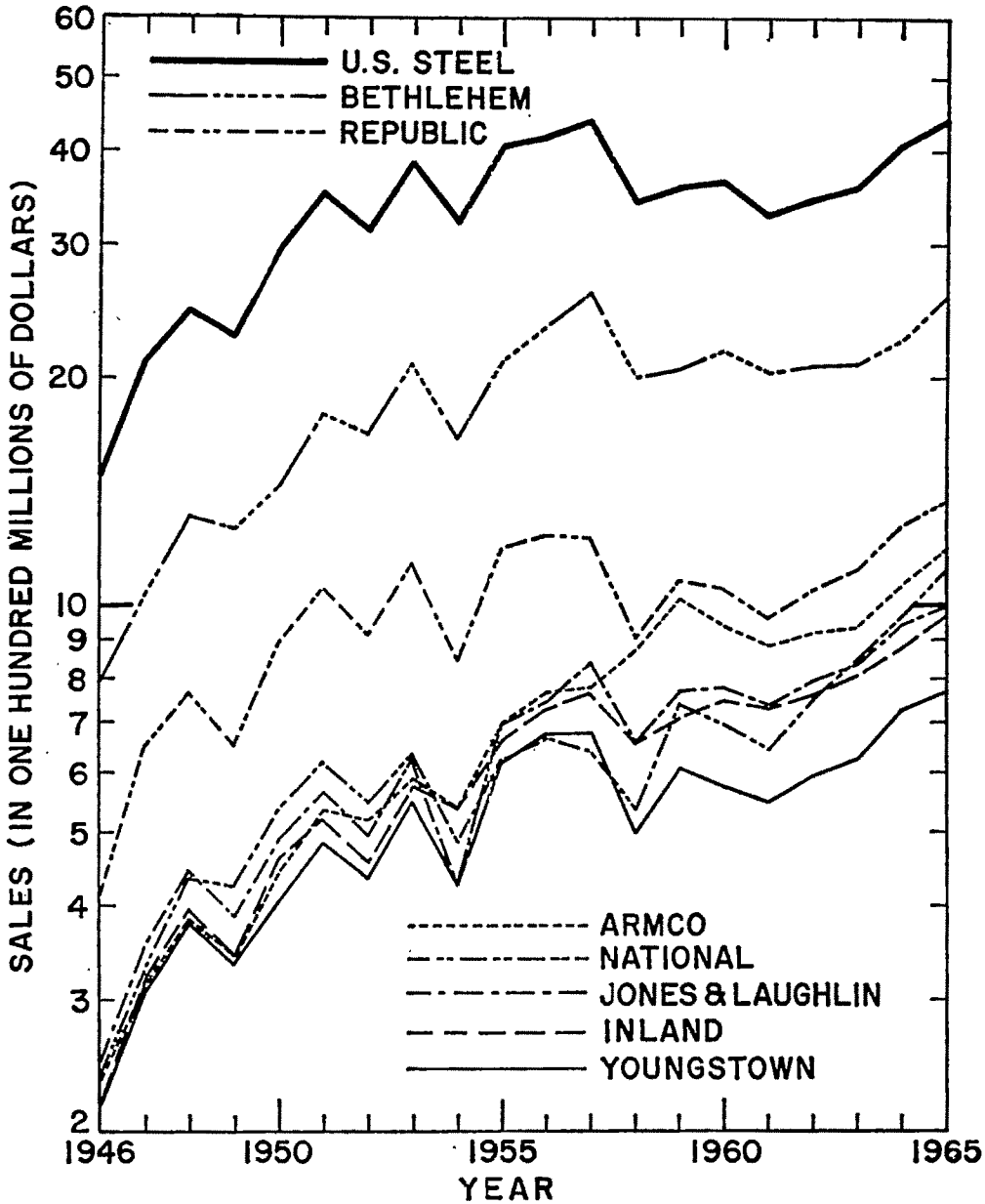


FIGURE 1. SALES OF EIGHT LARGEST STEEL COMPANIES
1946-1965

position, earnings deal with the operations' profit (and ultimately corporate survival), and profit margin describes the net return on sales. Though the three variables are basic measures of corporate success, they are by no means exhaustive; nor are they of equal importance in each industry. The data are drawn from the *Moody's Industrial Manual* and *Moody's Transportation Manual* issued

each year. These volumes contain extensive statistical information about large, publicly held corporations and appear to be primarily for the use of investors.

Technical considerations affect each measure. Excluded from sales figures are known funds listed or received by a company only in a formal sense such as excise taxes in the oil and beer industries or the sales generated

in merchandising operations by departments leased to others. Earnings are the net profit (or loss) after all expenses and taxes, but exclude any "extraordinary gains or losses" not part of regular operations as may result from the sale of properties or facilities. Profit margin is merely the ratio of net earnings to sales, as defined above. Most, but not all companies, report their results on a calendar year basis. Some companies, and certain industries, do not. Department stores, for example, traditionally use a fiscal year that ends on January 31. Such minor differences between fiscal and calendar year are readily incorporated in the study, but companies that changed their fiscal year during the span covered or with greater discrepancies from a calendar year had to be excluded.

Data were gathered on all available companies in each of thirteen industries.¹ The industries were not selected randomly, but to represent varied activities with respect to: technology employed; consumer orientation of markets; government control; size; and the like. Industry selection was also restricted by the need to have a minimum number of companies with the necessary data for the time span under study. These ranged from a low of six in the ship and boat building industry and nine in the beer and soap industries to a high of eighteen in the petroleum and shoe industries. Some industries could not be considered because their companies had too diverse a total set of activities. This study generally followed the categorizing of publicly held companies by the Securities Exchange Commission (1965) into the Standard Industrial Classification (SIC).

For a variety of reasons, the number of companies included within each industry is generally far smaller than the number listed by the Securities Exchange Commission. The *Moody's* data sources are less detailed for smaller companies; companies experiencing major accounting changes had to be dropped; those starting or disappearing (usually through mergers) within the twenty years could not be included; likewise, companies engaged in major mergers during the period were omitted. Also, companies with revised fiscal years or with fiscal years too close to

the middle of the calendar years could not be included.

Leadership changes were defined as the selection of a new president or chairman of the board. Although the duties of the two officials vary from company to company as well as within companies, it is safe to say that these are a company's top positions, and the chief executive officer is nearly always one or the other (Gordon, 1961:67). The yearly publication, *Poor's Register of Corporations, Directors and Executives*, was used to determine personnel changes in these positions. It is possible that for all its care, this work made errors in denoting new administrations. We could not determine when in a given year a new president or board chairman took office. It was hard to attribute performance exactly to its rightful administration during these changes.

Since a new administration takes office in different years, and for different time periods, determining the leadership effect on performance is complicated by each company's unique set of administrative patterns. Hence, we used a simple analytical method to find the proportion of total variance accounted for by each independent variable. For a given dependent variable, say sales, visualize a huge matrix in which each cell contains a specific company's sales figure for a specific year. The sum of the squared differences between each cell and the grand mean, divided by the number of cells, is the total sales variance for the matrix. The influence of any classified variable, say year, may be examined by first determining each category's mean, in this case all companies' mean sales in a given year. Each yearly mean is then subtracted from the matrix grand mean to determine the necessary adjustment for specific sales figures in that year. When the variance of these adjusted scores is determined, the difference between the initial total variance and the adjusted variance may be divided by the initial variance to determine the proportion of total variance eliminated after taking year into account. Similarly, this procedure may be used for other independent variables to determine the percent of total variance accounted for by each.

This form of analysis is also used to determine the proportion of variance explained by an added independent variable after allowing

¹ See Table 3 for the industries included.

for the effect of earlier variables. For example, after adjusting each year's sales figures for the subgroup means, the influence of industry was found by subtracting each industry mean from the grand mean and using this figure to adjust the cells previously adjusted for the year effect. This procedure can be repeated for the company effect (subtracting the company mean) and for administration. Subtracting the administration mean for a given company from figures that have already incorporated the year-, industry-, and company-specific effects, will yield the administrative effect that operates independent of these others. The irregular origin and term of administrations within and between companies makes this procedure of particular advantage.

The somewhat different analytical procedure and the additional variables used in the second part of the study will be discussed later.

THE LEADERSHIP EFFECT

In describing leadership influence on organizational performance, clearly one must consider the influence of other forces. First, the economy itself fluctuates yearly, affecting any corporation's performance. Beyond the major prosperity-depression cycles, the smaller yearly fluctuations in interest rates, inflation, labor shortages, and the like bear on a company's results. This will be measured here by considering the variance in organizational performance accounted for by subdividing the results into a yearly effect.

A second vital consideration is the company's primary industry. Granted that our categories are far from precise, nevertheless industry organizations are drawn together by a common level of demand and similarity in their technologies. Moore (1955:294) describes a number of common conditions needed to survive in the automobile industry. Thus, each industry has distinctive features which limit the options of its company's leaders. Aside from general economic fluctuations, varied factors unique to an industry may reflect specific market and supply factors, labor strikes, wage adjustments, foreign competition, market saturation, and the like. In the steel industry, for example, the demand is affected by a specific set of markets

and competitors only partly like those faced by the computer industry. Hence, we consider a given company's industry before accounting for its administration's performance effect.

Finally, since companies within an industry may differ radically in size, location, facilities, suppliers, markets, and reputation, the influence of the company per se must be considered. To be sure, one may argue that the variance between an industry's companies may reflect the relative skill of earlier administrators, but the fact remains that the newly appointed leader finds himself in a situation not of his own making. One of President Kennedy's difficulties with the State Department, Schlesinger observes (1965:410-411) was its deterioration during the McCarthy era and under Franklin Roosevelt. Whether this reflects partisan bias is of no concern here, the point is clear: companies within a given industry differ in ways that will influence a new leader's impact. Were General Motors and American Motors to trade all their top executives, the two companies' positions would not likely be reversed even if the quality of management were radically different (though that is, of course, an empirical question). Such factors as the two companies' production capacity, dealership organization, financial strength, consumer acceptance, and the like would undoubtedly place enormous limits on their options. Thus, one must consider company effect in examining impact on organization performance.

Table 1 below summarizes the influence of year, industry, company, and leadership on each of the three performance variables. In all cases, the yearly effect is slight, explaining no more than three percent of the total variance in organizational performance (column 2). This surprisingly low influence may reflect the period under consideration, which was generally prosperous and included no major depression. A company's industry is considerably more important and, when combined with the year effect (column 3), accounts for from 20 to 30 percent of the variance (net income and profit margins, respectively). The impact of the company position is particularly significant for sales and net income, accounting for an additional 64.8 and 67.7 percent of the total

Table 1. Variance in Performance Variables Accounted for by Selected Factors and Administration.

Performance variable	Total variance (1)	Cumulative proportion of variance accounted by:					Increment due to administration (7)	% reduction in unexplained variance when administration is considered (8)
		Y (2)	Y and I (3)	Y, I and C (4)	Y, I, C and A (5)	Unexplained (6)		
Sales	1.964(10 ¹⁷)	.031	.261	.909	.974	.026	.065	71.5
Net income	1.654(10 ¹⁵)	.017	.203	.880	.955	.045	.075	62.8
Profit margin	3.634(10 ⁻³)	.018	.303	.529	.674	.326	.145	30.8

Note: Y = Year; I = Industry; C = Company; A = Administration.
 Column 7 = Column 5 - Column 4.
 Column 8 = (Column 7/1.000 - Column 4) (100).

variance even after year and industry are taken into account (subtract column 3 from column 4). The company effect on profit margins is not as great, but still accounts for 22.6 percent of the total variance.

Column 4 of Table 1 suggests that the maximum possible administration effect varies widely between tasks. The administration has small effect on sales and net income since about 90 percent of the total variance in both is accounted for by year, industry, and company effects. By contrast, since these factors account for little more than half the profit margin variance, leadership differences could influence as much as 47.1 percent (100.0 - 52.9) of the profit margin's total variance.

However, leader differences are quite relevant for all three performance criteria. Explained sales variance moves up 6.5 percent to 97.4 when leadership is considered (column 5). Likewise, net income advances about as much to a total of 95.5 percent and profit margin variance leaps 15 percent to 67.4 (column 5). Thus, the leadership effect on company performance does matter. Though much sales and income variance is explained prior to considering leadership, leadership patterns account for a substantial part of the variance that remains unexplained (column 8). From 60 to 70 percent of the relatively small residuals in sales and income are accounted for by differences between top administration, and 30 percent of the more sizeable profit margin residual is explained by the 14.5 percent increment from administration.

The above research design only partly taps the leadership influence on performance variables. A substantial shift in the quality of top administration, either upward or downward, may set in motion long-term changes not immediately apparent. With such lags, a change in company position may not be properly credited to the appropriate administration. Lags in leadership effect are limited for sales or earnings since so much variance is explained by year, industry and company factors, but such is not the case with profit margins.

Accordingly, the influence of leadership changes is lagged on each performance variable for one-, two-, and three-year periods. For a one-year lag, for example, each company's performance variables in 1946 are attributed to the 1945 administration; for a two-year lag the 1944 leaders are credited with the 1946 performance, etc. This procedure has no bearing on measuring the year, industry and company effects because the lags are introduced after these factors are taken into account. However, leadership information is not available for fourteen companies as we move back to consider administration in 1943 for the three-year lag. Accordingly, column 1, Table 2, which gives the year-industry-company effects, and column 2, which gives the administrative effect without lags, differ slightly from the total sample results in columns 4 and 7 of Table 1. This is simply the effect of dropping fourteen companies.

The lag effect on sales and net income is relatively minor—differing little from the

administration influence determined assuming an immediate leadership effect. This is not surprising since leadership's absolute contribution to either sales or earnings is limited by the fact that prior variables account for so much of the variance. For the leadership effect on profit margins, however, the variance accounted for increases greatly assuming either a two- or three-year lag (columns 4 and 5 of Table 2). In either case, the leadership effect explains nearly a third of the total variance. Indeed, leadership influence on profit margins exceeds that for either the industry (.273) or company (.222) effects.² Altogether, the four independent variables account for 83 percent of the profit margin variance assuming a two-year lag in leadership effect.

In short, all three performance variables are affected by forces beyond a leader's immediate control, but the bounds on a leader's impact may vary between goals. For sales and earnings, 90 percent of total variance can be accounted for by the year-industry-company effects. Hence, though leadership accounts for much of the remaining variance, in absolute terms it is not as great an influence. By contrast, the restrictions imposed by year-industry-company effects on profit margins are less severe, though significant. Consequently, the leadership impact on this variable is much greater—indeed, it exceeds any other single influence.

² These figures differ slightly from the net differences shown in Table 1 (columns 3-2; 4-3) because fourteen companies are excluded in the lag analysis.

Table 2. Variance Accounted for by Administration Under Assumption of a Time-lag.

Performance variable	Variance accounted by year, industry, & company	Increment due to administration			
		No lag	1- year lag	2- year lag	3- year lag
		(1)	(2)	(3)	(4)
Sales	.910	.065	.066	.065	.063
Net Income	.880	.075	.080	.070	.069
Profit Margin	.512	.152	.127	.321	.317

If large complex organizations have more than one goal and if the leadership effect varies between these goals, then abstract debate over leadership importance is meaningless. Instead, specific research problems need study. The first problem is the degree each performance variable is influenced by non-leadership effects and, in turn, leadership influence on the remaining variance. This we have attempted with the above data. Our next step is to examine the features which determine the relative importance of both milieu and leadership effects on performance variables.

SOURCES OF LEADERSHIP CONSTRAINT

Leadership influence can be viewed as a product of two factors: the internal and external constraints on a leader and the relative ability of leaders. If constraints are high or leadership variance low, the leaders' effect will be minimal. Our results thus far raise empirical questions about both factors. First, what characteristics determine the relative influence of non-leadership factors? These will clearly influence the maximum possible leadership effect even if leader differences were to explain all remaining variance. Second, organizations differ in their leadership's role in accounting for the remaining variance unexplained. Thus, we shall examine the characteristics of organizations whose leadership is relatively important.

Complicating the issue is the recognition that both constraints and leadership effect may differ for activities or goals within an organization. In politics, the constraints in making foreign policy are probably of a different magnitude and source than those in domestic agriculture. For some goals leaders of small nations have more room to maneuver than leaders of large nations, while the inverse holds for other issues. In other words, leaders' options within an organization may vary widely depending on the goals or activities involved.

To explore these two issues, again we used the procedure described for decomposing the variance in Table 1, but this time separately for each of the thirteen industries. Thus the

Table 3. Variance within Industries Accounted for by Year, Company and Administration.

Industry	% of variance explained by year and company			% of remaining variance explained by administration		
	Sales (1)	Net earnings (2)	Profit margin (3)	Sales (4)	Net earnings (5)	Profit margin (6)
Air transportation	82.0	56.5	50.1	59.2	56.2	43.1
Beer	75.8	65.5	62.7	78.2	58.5	37.5
Clay products	90.5	86.2	23.6	69.3	47.3	14.0
Cement	86.4	86.7	73.9	77.4	67.6	45.8
Chemicals	87.9	88.9	79.2	79.6	63.9	45.7
Department stores	92.5	87.5	81.5	69.0	37.9	57.8
Drugs	83.1	80.9	70.0	61.2	58.9	52.6
Petroleum refining	88.5	89.6	85.2	81.5	71.8	50.1
Publishing	84.6	54.3	38.9	67.0	57.2	60.9
Ship building	79.7	56.1	32.6	77.7	15.7	16.7
Shoes and shoe stores	78.0	75.2	64.6	66.8	58.9	41.3
Soaps and toiletries	87.1	83.9	35.9	86.1	85.0	68.7
Steel	94.8	87.6	63.0	55.1	56.9	40.2

relative importance of year, company and leadership on each dependent variable can be compared between industries.³ Industries differ greatly in the percent of total variance that can be explained by the additive influences of year and company (Table 3, columns 1 through 3) as well as in percent of remaining variance accounted for by differences in top management (columns 4 through 6).

The variance explained by year and company ranges least for sales, running from 76 percent in the beer industry to 95 percent in the steel industry (column 1). However, the range is broader for net earnings, running from about 55 percent in publishing and air transportation to nearly 90 percent in petroleum refining (column 2), and widest for profit margins, with year and company accounting for only a fourth of the variance among companies producing clay products, compared to 85 percent in petroleum refining (column 3). Industries also differ greatly in the degree that year and company consistently account for each performance variable (look across the first three columns). Though the figures are fairly constant in petroleum refining, they differ greatly in

both publishing and ship building. In effect, there is some support for the notion that the same environmental factors differ in strength of impact on various organizational goals and activities.

The last three columns in Table 3 also suggest that similar patterns operate for inter-industry differences in the degree that administration explains the remaining variance in organizational performance.

EXTERNAL CONSTRAINTS AND ORGANIZATIONAL CHARACTERISTICS

Since the object is to study conditions which influence organizational susceptibility to year and company constraints, we should see if inter-industry differences in the variance explained is related to other industry characteristics. Table 4's eleven variables include: size, dependency on production workers, degree of machine technology, orientation of markets, importance of scientific and engineering activities, markets, industry growth, concentration, consumer orientation and average number of company vice presidents. Unfortunately, in most instances we were only able to get summary information on all of an industry's companies. Table 4's correlations represent inferences that may not hold with data based on individual

³ The industry effect is not considered in this part of the analysis because of the design used.

companies. Since the analysis deals with only thirteen industries, results are tentative and exploratory. To guard against over-interpretation, a fairly high zero-order correlation is required.⁴ No attempt is made to consider the partial correlations, as tempting as this may be; and likewise only two dependent variables, sales and profit margins, are examined for pre-leadership and leadership effects. The net income variable is excluded because its inter-industry cor-

relations are substantial with some other dependent variables.

Sales

Two key factors affect the influence of year and company on total sales variance (Table 4, column 1): the mean number of employees and the industry's market structure. The first correlation suggests that industries with large companies, at least as measured by employment, are less immune to the combined effects of the general economy and each company's position within it. Hence, relatively more total sales variance in such industries is explained by these forces. By contrast, industries having companies with smaller employment are also

⁴ The product-moment correlations are based on thirteen, eleven or ten industries. Were these a random sample of industries, a single-tailed test of significance at the .05 level would require correlations of .476, .521 and .550, respectively, before the null hypothesis could be rejected.

Table 4. Product-Moment Correlations Between Selected Industrial Characteristics and Variance in Performance Accounted for by Year, Company and Administration.

Characteristic	Inter-industry correlations between characteristics and:			
	Proportion of total variance accounted for by year and company		Proportion of remaining variance accounted for by administration	
	Sales (1)	Profit margins (2)	Sales (3)	Profit margins (4)
Employees ^{a,d}	.568	.324	-.419	.007
Vice-presidents ^{a,d}	.211	.240	-.353	.630
Production worker's payroll/total payroll ^{c,e}	.223	.016	.057	-.764
Power costs per production worker ^{c,e}	.336	.569	.194	.021
Concentration ratio ^{c,f}	.171	.206	.551	-.086
Personal consumption/total output ^{b,g}	-.537	.070	.243	.492
Advertising expenditures/total output ^{b,g}	-.226	-.116	.295	.558
Payroll/value added ^{c,h}	.044	-.473	-.269	-.752
Growth in value added ^{c,h}	.323	.155	-.035	.612
Assets per employee ^{c,h}	.236	.779	.336	.182
Scientists and engineers/total employees ^{b,i}	.127	.374	.090	.124

^a Data available for all 13 industries.

^b Data available for 11 industries.

^c Data available for 10 industries.

^d Mean for 1955 and 1956 derived from original data.

^e Derived from Bureau of the Census (1961 a,b).

^f Concentration Ratio for largest 8 companies in 1963. Bureau of the Census (1970).

^g Office of Business Economics (1969).

^h Bureau of Domestic Commerce (1970). Payroll/Value Added based on 1958; Growth in Value Added based on: (1964 & 1965)/(1958 & 1959); Assets per Employee based on 1964.

ⁱ Bureau of the Census (1963).

those offering more options and flexibility. Hence, their company sales are less influenced by combined year and company effects.

The second important variable, proportion of industry sales to consumers, is negatively associated with the influence on sales of year and company. The sales volume of industries with a high proportion of output directed to non-consumer markets is more likely to reflect the economy's influence. Industries with markets more oriented to the final consumer are less affected by general forces, presumably because each company has more room to appeal to consumer tastes, and shifts in consumer interests will have a greater impact.

Profit Margins

The influence of the year and company effects on profit margins appears to reflect industrial differences in machine technology and capital needs (Table 4, column 2). These are, of course, somewhat related factors. Industries with highly developed technologies (as measured by power consumption per employee) tend also to have great capital needs (as measured by assets per employee) and hence to be low in "labor intensity" (payroll/value added). Hence, profit margins in industries with relatively large fixed assets and greater machine technology tend to be more influenced by the year and company effects. Insofar as such industries have higher fixed overhead costs, profit margins—as contrasted with sales level—will be affected by economic demands for the industry's products and each company's position within the industry.

LEADERSHIP EFFECT AND ORGANIZATIONAL CHARACTERISTICS

We now examine differences between industries in management's impact on performance after accounting for the influence of year and company. Table 3, columns 4 through 6, shows that the industries differ greatly in the administrative effect on variance unexplained by these prior factors. In the soap industry, leadership accounts for nearly 70 percent of the unexplained profit margin variance. By contrast, management

accounts for about 15 percent in the ship building and clay products industries after these controls are applied (Table 3, column 6).⁵

Sales

The administrative effect on sales tends to vary only with the industry's degree of concentration (Table 4, column 3). Those industries whose shipment value is most concentrated in the largest eight companies are also those whose leadership effect on sales is stronger. Though the reason for this is unclear, note that the two characteristics related to the year and company effects on sales—number of employees and personal consumption—are in no way related to management impact on the remaining sales variance (compare columns 1 and 3).

This finding, along with the comparison of columns 2 and 4 below, suggests an important distinction. On the one hand are the factors affecting the remaining unexplained variance before considering management's influence, on the other, the factors affecting inter-industry differences in the leadership effect on performance after year and company are accounted for. The latter will reflect the relative uniformity of appointments as well as industry characteristics. Hence, the forces accounting for the size of an industry's year and company effect may differ from those explaining leadership effect on the remaining variance.

Profit Margins

Varied factors relate to management impact on profit margins after the year and company effects are removed. The impact tends to be greater in industries with low labor intensity and/or where production worker pay is a smaller part of total employment costs (Table 4, column 4). Presumably, here top management has more

⁵ Note that inter-industry differences in the administrative succession process are not considered. Moreover, the administrative effect on performance variables is examined here without the operation of a lag.

room to effect company profit. Thus, the same range of fluctuations in management skills will generate greater shifts in the profit margins of industries with lower fixed overhead.

An industry's general growth, as measured by the percent change in value added over time, also influences management impact on profit margins (column 4). Industries with rapid growth probably offered more options to managers and hence their differences will more greatly effect profit margins.

The profit margin tends to be influenced more by management in industries where advertising is important and consumers form a sizable market. Such industries contend with more ambiguous issues of taste, judgment, and customer manipulation. Hence, management's decisions about advertising styles and consumer interest will influence profit margins more than in less consumer oriented industries.

A fourth factor is surprising. Namely, the number of vice presidents in the industry's average company is directly correlated with the administrative effect on profit margins. At first we expected that industries with many vice presidents would tend to have relatively less administrative effect since the buffer between the company's operations and the top leaders would be larger. These results contradict the point we made about the duties of the chairman of General Motors. Namely, the impact of the president and chairman is increased when the policy execution is delegated to others. Hence, an increase in the number of vice presidents may give top management tighter control through better policy execution and greater feedback on company operations.

Perhaps our most important finding is that factors influencing variance in the administrative effect on profit margins generally differ from those influencing the year and company effects. Six different variables are substantially correlated with differences in administrative effect and four variables are associated with the year and company effects. Yet, only one operates, on the zero-order level at least, with both administrative and year-company influences.

DISCUSSION

Before we suggest this study's meaning for large organizations, certain cautions should be noted. Companies may have other performance variables more greatly affected by administration and virtually unaffected by environmental constraints. Moreover, the study design may itself lead one to underestimate leadership effect on the performance variables. We excluded companies either too diversified or radically altered by mergers. We did so to infer the influence of administrative variance under relatively controlled industrial conditions. However, the quality of management may have less effect on the existing basic processes and activities in an established company. Conversely, leadership variance may have more effect on mergers, entrance into new industrial realms, radical patterns of diversification unrelated to existing company activities, and various forms of corporate financial manipulations. Of course, this question is empirical. All the leading tobacco companies, once a rapidly growing industry, have responded similarly to growth restrictions stemming from the health issue. They merged with other companies, especially those engaged in consumer oriented activities requiring their marketing skills.

Finally, this study does not examine the process used to select new leaders. Since leaders are not selected randomly from the universe of all employees, the low leadership effect on sales and earnings could reflect a company's uniformly consistent judgments in leader selection. However, an important leadership effect on profit margins means that the leadership succession process does generate variance in administrative skill. Nevertheless, we have not dealt with the problem of succession process in organizations and leadership variance.

We considered two central leadership questions in this study: first, the influence of leadership on organizational performance; second, the factors associated with inter-organizational differences in leadership impact. Of the first, our analysis shows that much of the variance in three performance variables—sales, earnings and profit margins

—can be explained by factors other than leadership variance. Leadership has a smaller effect than either industry or company on sales and net earnings, though larger than the year effect for the period studied. On the other hand, leadership has a strong effect on profit margins compared to other restrictions.

These results suggest that in emphasizing the effect of leadership, we may be overlooking far more powerful environmental influences. Unless leadership is studied as part of a total set of forces, one cannot gauge its impact. Moreover, the leadership effect may vary greatly between goals in an organization.

The second part of the analysis deals with industry differences in variance accounted for by the year-company and leadership effects. While we must view the results with caution, the patterns we find are highly suggestive. Several factors correlate with industry differences in the impact of year, company and administration on sales and profit margins. Almost without exception, the significant variables differ for each of the four dependent variables. This suggests that the leadership role must be discussed not merely in terms of general constraints on the organization and the remaining variance accounted for by the leadership effect, but in terms of the specific organizational goal. An organization's milieu may tie a leader's hands or free them depending on which goal one is assessing.

This analysis suggests that we should treat separately the forces constraining leadership and those associated with leadership influence on the remaining variance. With profit margins, for example, only one variable associated with inter-industry leadership effect differences is also associated with the level of inter-industry differences for year and company effects.

The lag in leadership effect on profit margins suggests various research problems. What are these lags on organizational goals and what organizational structures create them? In turn, when leaders face removal from office, how does time-lag affect policy directions?

Much political rhetoric involves such is-

—sues. An incumbent ascribes the shortcomings of his term either to a time-lag in the fruition of new policies or external constraints on his efforts. All positive results in office he claims for himself, giving no credit to external forces or to a time-lag in the fruition of his predecessor's programs. By contrast, his opponent attributes all problems to the incumbent, not allowing for the effect of external forces or time-lags.

These results suggest a research strategy perhaps applicable to other organizations. For example, the impact of political leaders could be compared with the general economic, social and political constraints on cities, states and nations.⁶ The impact of mayors can be examined for various goals or budgetary expenditures in cities. In turn, the effect of inter-city leadership differences on a given goal can be correlated with other differences between cities such as the political party system, city size, legislative system, population and economic growth, state laws, and the like. If the importance voters attach to various issues were ranked, the order might well differ substantially from the ability of elected officials to affect these issues. Presently, the public is ignorant of the variance in each issue affected by external constraints. In short, leadership impact is not simply a philosophical question, but an empirical problem involving an organization's environmental constraints.

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ACHIEVEMENT ORIENTATIONS AND SOCIOECONOMIC CAREER ATTAINMENTS *

DAVID L. FEATHERMAN

University of Wisconsin, Madison

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In longitudinal data (1957-1967) for white metropolitan males in the United States, the impact of achievement orientations on career attainments and on the processes of social mobility from family of origin statuses to destination statuses is examined. Several path analytic models are constructed, in which achievement-related motivations are included as intervening variables, potentially capable of explaining how socioeconomic origins and/or early career achievements influence later socioeconomic attainments. Scant support is found for the assertion of some social psychologists that achievement orientations are "highly relevant" to status attainment processes in industrialized societies.

FROM the start, empirical studies of social stratification have acknowledged social psychological factors in status attainment processes. As early as 1927 Sorokin cited the work of Terman (1925), Cattell (1921) and Yerkes (1921) as bearing on a relationship between intelligence and social mobility. In his treatise on social mobility, Sorokin portrayed the role of intellectual and other personality differences quite graphically:

Besides the above intellectual differences it is likely that there are other psychological differences between the upper and the lower classes. The more important among them seems to be as follows: except during the period of decay, *the upper strata are composed of persons possessed of strong ambitions, bold and adventurous characters, with inventive minds, with harsh and non-sentimental natures; with a sort of cynicism and, finally, with a will for domination and power.* These terms are not quite definite but are used on account of the absence of any better ones. . . . In order to be a successful ruler or to become a boss or a captain of finance, or a great inventor and reformer, intellect

alone is not sufficient. It is necessary also, to have a corresponding character. A man without a persistent character, in spite of his talent, cannot become either a great scientist, or money maker, or ruler, or inventor, or leader generally. Except, perhaps, in the case of poetry, all these activities demand a great deal of stubbornness, persistency, and determination as prerequisites to success. Soft characters who cannot work steadily in their own line rarely achieve prominence, in spite of intellect. . . . Hence, insincerity, cynicism, manipulation of ideal and convictions are necessary prerequisites for successful climbing through many channels (Sorokin, 1964:308-309).

While contemporary social scientists may not agree literally with this tooth and claw portrayal of vertical circulation, some students of stratification persist in granting efficacy to personality factors. For example, Lipset and Bendix (1959) suggest that "by merging the sociological and psychological approaches to the study of social mobility we might advance the study of the mechanisms by which individuals and groups reach their positions in the stratification structure (Lipset and Bendix, 1959:259)." In Crockett's (1966) list of personality factors fostering social mobility, we note the argument that the *cognitions and cognitive structures* of high school students, and their subsequent educational and occupational attainments, are influenced by their peers' achievement aspirations and the encouragements of significant others (Duncan, Haller and Portes, 1968; Sewell, Haller, and Portes, 1969). Others assess the channels by which intellectual capacities influence and

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are molded by status attainments vis-à-vis social origins (Duncan, 1968; Duncan, Featherman and Duncan, 1968:80-119). Finally, achievement *motivations* are purported by social psychologists (Atkinson, 1957; Crockett, 1962; Rosen, 1956) to be "highly relevant to differential mobility in industrial societies" (Crockett, 1966:294).

Despite this sustained interest in personality factors, as Sorokin complained over four decades ago, research constructs in this area still lack clear definition. This clarity is especially critical in motivational research, where the validity and reliability of achievement motivation constructs (Krumboltz, 1957), their situational fragility (McClelland, 1958), and their probable multi-dimensionality (Kahl, 1965) should caution any who would relate achievement motivation to mobility.

In addition, any causal representation of the role of motivation in status attainment processes must specify precisely the locus of effect in the life cycle. While, for example, achievement orientations may influence adolescents' attainments in the school systems (Rosen, 1956; Lavin, 1965:237), their role in post-educational socioeconomic achievements seems rather limited (Duncan, Featherman and Duncan, 1968:131-181), even though work motivations may be associated with job productivity (Vroom, 1964).

This paper examines several ways in which certain achievement-related orientations may influence white metropolitan males' attainment of educational, occupational and economic statuses up to mid-career. It estimates the magnitude and pattern of the effects of these personality factors both before and after adjusting the data and causal models for measurement validity and reliability. This work joins others (Duncan, 1969; Featherman, 1971c; Duncan and Featherman, 1970) in attempting to specify and qualify the effects of motivation on inter- and intra-generational mobility.

DATA AND METHODS

Data from three panels of the Princeton Fertility Study refer to white native couples residing in seven of the largest SMAs in the United States over the years 1957-1967. Details of this stratified random sample are

found in Westoff *et al.* (1961; 1963) and Bumpass and Westoff (1970). Interviews were conducted in 1957 (panel I), 1960 (panel II) and in 1963-1967 (panel III) in the following SMAs: New York, Philadelphia, Pittsburgh, Detroit, Chicago, Los Angeles, and San Francisco. These data provide estimates of the stratification process parameters like those from more representative population cross-sections of a similar age (Duncan, Featherman and Duncan, 1968: 182-198; Featherman, 1969). Reference is to white, native, metropolitan males averaging thirty years of age in 1957 (panel I); the sample size for the period 1957-1967 is 715 men.

Two variables measure social background statuses: father's occupation, indexed in intervals of the 1947 NORC prestige scale (NORC, 1947), and family size or number of siblings. The respondent's achieved statuses include education (years of schooling as of 1957), occupation at each panel (1947 NORC prestige scale intervals), and income at each panel (salaries and wages).

Attitude data (collected only at panel I) permit the construction of three indexes of achievement-related motivation focused on work. Details of index construction and the questionnaire items appear elsewhere (Featherman, 1969; 1971c). Two action tendencies toward work were deduced from the literature on achievement-related motivation: intrinsic and extrinsic motivation (Lenski, 1963:89; Gurin, Veroff and Feld, 1960:149-150; Douvan, 1956; Burnstein, 1963; Burnstein, Moulton and Liberty, 1963; Kohn and Schooler, 1969). The first tendency, "Primary Work Orientation" (WO), attributes a positive non-economic value to work context. The second, "Materialistic Orientation" (MO), emphasizes the material goals of the good life achieved through instrumental activity.

While questionnaire items were clustered *a priori* to conform to these two definitions, empirical support for this clustering comes from a principal components factor analysis, with the components rotated obliquely under the hypothesis that the items in the total matrix cluster as described for WO and MO (Horst, 1965:160-167, 394-397). Results from this analysis, the "hypothesis method of transformation" (Horst, 1965:

385), are quite like those from an analytic technique which rotates the components orthogonally. By the formula for estimating index reliability from the items' intercorrelations (Nunnally, 1967:193), *coefficient alpha* for WO is .841, a figure which represents the expected correlation of this six-item index with another six-item index were they both drawn from the same item domain; *coefficient alpha* for MO is .717. Scores for both indexes are computed from a simple addition of items answered in the appropriate direction rather than from item factor loadings, since the simpler procedure produces nearly identical results (Featherman, 1969).

A third index is constructed from items which elicit a man's overall satisfaction (i.e., subjective sense of relative deprivation) with his socioeconomic achievements at panel I. The index, "Subjective Achievement Evaluation" (SA), indicates the conscious psychological effects of an individual's past and present socioeconomic achievements on his achievements during the follow-up period. *Coefficient alpha* for SA is .797; and as for indexes WO and MO, scores for SA are generated from a sum of item responses to this ten-item index.¹

Table 1 lists coefficients of correlation among the social background, achieved status, and motivational variables. Above the main diagonal, coefficients are uncorrected for attenuation; below the diagonal, coefficients are corrected. Corrections for attenua-

tion (Nunnally, 1967:204) were effected with the following reliability coefficients, as given above or taken from Duncan (1969: 83): education, .933; occupation, .861; income, .852; siblings, .963; WO, .841; MO, .717; SA, .797.

MOTIVATIONS AND ACHIEVEMENTS

Path analysis techniques (Duncan, 1966) are used to examine models of the status attainment process which incorporate personality components as intervening variables. The path model in Figure 1 makes the following inductive and deductive causal assumptions. (Its causal paths are hypothetical and statistically significant; i.e., the absolute size of each path or beta coefficient is greater than or equal to twice its standard error.) Note that Table 2 contains numerical coefficients to be inserted in Figure 1 both before and after the data have been corrected for attenuation.

Figure 1 specifies a recursive causal model. It takes the correlation between father's occupation and son's education as the estimate of the path coefficient p_{31} .² Occupation at panel I is a linear function of both education and father's occupation, and income is a linear function of education and occupation at the initial panel. Occupational statuses at panels II and III depend in part on education and earlier occupational statuses. The rationale for such causal representation among the social structural variables is well stated in Blau and Duncan (1967). Occupation at the terminal interview also is determined by a direct path, or "lagged effect," from father's occupation (Featherman, 1971a). Both occupational status and income at panels II and III are directly influenced by the motivational indexes. For the moment we will defer a detailed discussion of these effects. However, economic achievements at panels II and III

¹ Further descriptions of the three constructs (MO, WO, and SA) and an examination of their potential to explain religio-ethnic differences in status attainment appear in Featherman (1971c). Note that the terms "achievement-related motivation," "achievement orientation," "action tendencies toward work" are used synonymously in discussing constructs WO, MO, and SA. In adopting a terminology similar to that used by motivational psychologists (Atkinson and Birch, 1970) I have avoided the thorny issue of whether attitude items reflect opinions, attitudes, values and/or motivations. Suffice to say that attitude statements can be regarded as fallible indicators of (achievement) motivations in much the same way as Thematic Apperception Test scores have been interpreted as fallible indicators of motivations (e.g., Crockett, 1962); both index some common action tendency (to achieve) and reflect the tendency which yields generalized achievement behaviors (e.g., competition in school, occupational mobility). See Atkinson and Birch (1970) for fuller treatment of these conceptual issues.

² A decomposition of the correlation r_{21} using additional social background variables (viz., number of siblings and residential background) from the Princeton data appears in Featherman (1971b). The path coefficient p_{21} probably is overestimated somewhat by the correlation, but this specification in the model facilitates later calculations of the net effects of father's occupation (holding constant education) which the correlation would not permit.

Table 1. Coefficients of Correlation: Uncorrected (above diagonal) and Corrected (below diagonal) for Attenuation

Variable ^a		1	2	3	4	Variable (see stub)												
						5	6	7	8	9	10	11	12	13				
Father's Occ-NORC	1	--	-.14	.34	.33	.15	.30	.21	.34	.24	.22	-.06	.17	--				
No. Siblings	2	--	--	-.29	-.23	-.11	-.22	-.12	-.23	-.19	-.13	.05	-.16	--				
Education	3	.38	-.31	--	.64	.33	.65	.44	.60	.48	.37	-.13	.34	--				
Occ-NORC,I	4	.39	-.26	.71	--	.34	.78	.44	.69	.47	.41	-.12	.36	--				
Income,I	5	.18	-.12	.36	.40	--	.32	.62	.27	.40	.24	-.06	.33	--				
Occ-NORC,II	6	.35	-.26	.73	.90	.37	--	.44	.74	.47	.40	-.09	.33	--				
Income,II	7	.24	-.13	.52	.52	.73	.52	--	.40	.52	.28	-.05	.40	--				
Occ-NORC,III	8	.40	-.26	.70	.80	.32	.86	.47	--	.51	.36	-.11	.31	--				
Income,III	9	.28	-.21	.54	.55	.47	.55	.61	.60	--	.31	-.01	.34	--				
WO (Primary Work Orient.)	10	.26	-.15	.42	.48	.28	.47	.33	.42	.37	--	-.01	.44	--				
MO (Material. Orient.)	11	-.08	.06	-.16	-.15	-.08	-.11	-.06	-.14	-.01	-.02	--	-.24	--				
SA (Subj. Ach. Eval.)	12	.21	-.18	.39	.44	.40	.40	.49	.37	.41	.54	-.31	--	--				
$\bar{W}^b$	13	.31	-.18	.50	.57	.33	--	--	.48	--	.84	-.02	.64	--				
$\bar{M}^b$	14	-.11	.08	-.22	-.21	-.11	--	--	-.19	--	-.03	.72	-.43	-.03				

^a See Text for definition of variables.

^b Hypothetical variables calculated from Figure 2.

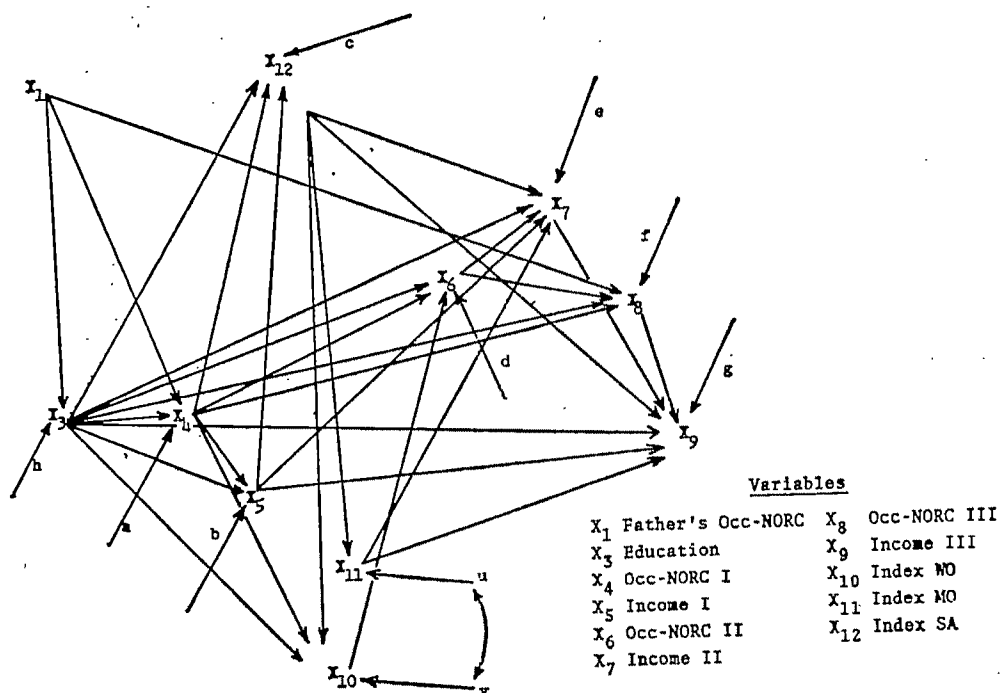


Figure 1. Achievement Orientations and Status Attainment (coefficients listed in Table 2)

are understood also to depend on educational and prior economic attainments, and not only on current occupational statuses. The arguments for the causal links between income, occupation, and education are straightforward and need not be repeated here (cf., Duncan, Featherman and Duncan, 1968).

The social structural relationships in Figure 1 and Table 2 are treated in other sources (Featherman, 1970; 1971a). This paper is concerned only with the effects of personality variables (motivations) on the status attainment process.

Figure 1 introduces the motivations measured at panel I as variables intervening between the socioeconomic achievements at panel I (and their antecedents) and those in the follow-up period. Social psychologists might argue that achievement-related motivations arise in pre-adolescence within the primary family and are modified by adolescent and adult social experiences. Table 2 lends some credence to this conception, although as reported below the supporting

evidence is weak. Causally Figure 1 asserts that one's subjective evaluation of socioeconomic achievement (SA) reflects, in part (note the substantial residual path to SA), the objective achievements of education, occupation and income at panel I. This index and the actual attainments are the source for the expression of the two adult achievement orientations, indexes WO and MO. Conceptually distinct, WO and MO are empirically orthogonal measures (zero-order correlation is $-.013$, or $-.017$ when corrected for attenuation).

While one might argue for an alternative set of these indexes' causal relationships (as appears later), for Figure 1 there are no statistically significant direct paths from social origins to the indexes; the relationships are indirect through the path from father's occupational status to son's education (p_{31}) and the direct effect of the former on son's occupation at panel I (p_{41}). For example, the effect of father's occupation on WO, net of son's education, is $.037$, which is the in-

Table 2. Coefficients for Figure 1 for Data Corrected and Uncorrected for Attenuation

Dependent Variables	Path Coefficients			Residuals		Correlated Residuals	
	Independent Variables		Uncor-rected	Cor-rected	Uncor-rected	Cor-rected	Uncor-rected
Education	Fa.Occ-NORC	P ₃₁	.339	.378	a .694	.762	$r_{uv} .127$
Occ-NORC I	Fa.Occ-NORC	P ₄₁	.129	.137	b .931	.911	
	Education	P ₄₃	.593	.657	c .898	.859	
Income I	Education	P ₅₃	.184	.169	d .596	.417	
	Occ-NORC I	P ₅₄	.220	.275	e .715	.600	
SA	Education	P _{12,3}	.138	.118	f .636	.497	
	Occ-NORC I	P _{12,4}	.203	.252	g .766	.692	
	Income I	P _{12,5}	.220	.261	h .941	.926	
WO	Education	P _{10,3}	.135	.098	u .972	.950	
	Occ-NORC I	P _{10,4}	.207	.237	v .851	.794	
	SA	P _{10,12}	.320	.396			
MO	SA	P _{11,12}	-.235	-.311			
Occ-NORC II	Education	P ₆₃	.251	.170			
	Occ-NORC I	P ₆₄	.589	.766			
	WO	P _{6,10}	.063	.028			
Income II	Education	P ₇₃	.141	.160			
	Income I	P ₇₅	.478	.559			
	Occ-NORC II	P ₇₆	.154	.137			
	SA	P _{7,12}	.157	.169			
	MO	P _{7,11}	.052	.076			
Occ-NORC III	Fa.Occ-NORC	P ₈₁	.086	.087			
	Education	P ₈₃	.133	.126			
	Occ-NORC I	P ₈₄	.226	.080			
	Occ-NORC II	P ₈₆	.450	.662			
Income III	Education	P ₉₃	.148	.081			
	Income I	P ₉₅	.101	.076			
	SA	P _{9,12}	.083	.099			
	MO	P _{9,11}	.071	.110			
	Income II	P ₉₇	.252	.309			
	Occ-NORC III	P ₉₈	.280	.354			

direct effect via son's occupation and income, computed by multiplying the relevant path coefficients (uncorrected values);³ the indirect effect on MO is $-.006$. Similarly, the indirect effects of father's occupation via son's education on WO and MO, net of son's occupation and income at panel I, are $.061$ and $-.011$, respectively. Thus, social origins (as indexed here by father's occupational status) are less important for directly determining adult achievement motivations than are the more immediate social situation factors; and of the latter, it is education (rather than early career achievements) which more effectively transmits the impact of social origins to the measured motivation levels.

The model explains less of the variation in MO than in WO. In addition, the partial correlation r_{uv} ($.127$ uncorrected; $.210$ corrected) suggests correlated errors in the responses to WO and MO and/or the existence of some unmeasured source of common variation in the two indexes, which is not represented adequately by the independent variables causally antecedent to both.

Primary Work Orientation (WO) reflects its situation origins in the positive paths to it from education ($p_{10,3}$) and panel I occupation ($p_{10,4}$), and these net effects are separate from the direct positive influence of the subjective evaluation of achievement ($p_{10,12}$). On the other hand, the extrinsic work orientation given in MO is connected to the antecedent variables only through the subjective evaluation of past achievements ($p_{11,12}$), being unaffected directly by these objective situation statuses. Materialistic Orientation toward work is an apparent product of a negative Subjective Achievement Evaluation, while the intrinsic value of work tapped by Primary Work Orientation proceeds from a positive subjective evaluation of achievement. For its part, Subjective Achievement Evaluation increases positively with prior objective achievements but is not a veridical reflection of these (residual c is $.898$, or $.859$ when corrected values are used).

In sum, the two work orientations MO

and WO are empirically separable. They seem to arise mostly from early social achievement activities and only weakly and indirectly reflect family social psychological events.

In arguing that achievement orientations are "highly relevant" to status attainment in industrialized, achieving societies, social psychologists presumably mean that work motivations operate as key intervening variables which transmit the effects of social origins to social destinations. Figure 1 permits a limited test of the "relevance" assertion.

Both orientation indexes have positive, albeit small, path coefficients to socioeconomic statuses throughout the follow-up period; index MO expresses a positive net influence on income, while WO gives rise to higher occupational attainments.

Controlling statistically for the effects of social background, prior achievements, and the other two indexes (WO and SA), MO directly affects income levels at panels II and III, or achievements some three to ten years after these orientations are assessed; none of MO's direct effects on occupational attainments are statistically significant. Similarly, index WO has a positive direct effect on occupation at panel II, but bears on no further socioeconomic achievement. The effect of correcting all correlations for attenuation (see Table 2) is negligible; the relative path coefficient sizes remain essentially the same: the coefficients for the two work orientations are among the least important, if not the smallest. Index SA has slightly stronger positive coefficients and directly affects the income variables at panels II and III, suggesting that this index, too, may tap some motivational factor. In any event, the achievement orientations have some small marginal efficacy in the status attainment process over the ten-year re-study course. Inasmuch as indexes WO and MO are essentially orthogonal, each provides an independent resource for differential mobility and socioeconomic success, as given by levels of occupational prestige and income.

Figure 1 illustrates that both MO and WO are rather weak intervening variables between father's occupation and son's socioeconomic statuses at panels II and III; this is so, in part, because $p_{10,1}$ and $p_{11,1}$ are

³ The computational expression is

$$p_{10,12}(p_{12,3}p_{34}p_{41} + p_{12,4}p_{41}) + p_{10,4}p_{41}.$$

effectively zero, and because p_{81} is non-zero (p_{81} is .086, or .087 corrected; when none of the personality variables is included in the model, p_{81} is still .086, uncorrected.) Furthermore, if one computes the indirect effects of father's occupation on son's occupation and income at panel II (for example), tracing these effects through the variables and paths in Figure 1, one finds that the motivational indexes WO and MO are less effective as intervening variables than are the son's panel I socioeconomic achievements.

For constant years of educational achievement (Table 3), the net indirect effect of father's occupation on son's panel II occupation through index WO is .002; while the effect via son's panel I occupation is .076. Table 3 also illustrates that the net effect of father's occupation on son's panel II income through index MO is effectively zero (-.000). Similarly, for constant social origin statuses, the indirect effects of education on panel II achieved statuses (Table 3) via the motivational indexes are the smallest values in their respective columns (viz., .023 and -.003).

These data hardly support the proposition that achievement-related motivations are highly relevant to the course of social mobility. At least for two separate indexes of achievement orientations of adult males, such motivational personality components

are less able to explain how social origins or completed schooling facilitate status attainment than are earlier career achievements. Perhaps, had motivation been measured in adolescence—say during schooling, achievement orientations would have more influence on careers. Perhaps these earlier unmeasured forms of the orientations can account for some of education's net effects and panel I's achievements.

Figure 2 explores this possibility, and its causal assumptions diverge from Figure 1 in important ways. It includes father's occupation and the number of siblings in the family of origin as predetermined variables. One expects the predetermined variables to be negatively correlated, the net effect on education of family size to be negative, and the direct effects of the sibling variable to be zero in the post-education period (Blau and Duncan, 1967:298, 307; Featherman, 1971b). Antecedent to education in the causal sequence, and dependent on the predetermined family background variables are two unmeasured, hypothetical variables M and W . Each might be thought of as an earlier form of the corresponding achievement orientations crystallized in adolescence and reflected in variables MO and WO. But M and W also reflect the subjective propensity to respond evaluatively to objective achievements up to panel I (i.e., edu-

Table 3. Indirect Effects of Father's Occupational Status and Son's Education, Each Net of Other, on Panel II Achievements

Source Variable	Indirect Effects via:	Dependent Variables	
		Occ-NORC II	Income II
Father's occupation, net son's education	Occ-NORC I	.076	.014
	Income I	--	.005
	Subj. achievement	--	--
	Work orientation	.002	--
	Material orientation	--	-.000
	Occ-NORC II	--	.012
Son's education, net father's occupation	Occ-NORC I	.349	--
	Income I	--	.150
	Subj. achievement	--	.051
	Work orientation	.023	--
	Material orientation	--	-.003
	Occ-NORC II	--	.096

Source: Figure 1 and Table 2.

cation, occupation, and income). Note that variable SA, and not MO or WO, is affected by the social background and prior achievement variables. This representation posits that WO and MO are relatively valid (uncontaminated by endogenous factors in the model) indicators of W and M , respectively. Variable SA, however, is a less valid (or more contaminated) indicator of the earlier motivations.

In Figure 2, education is a function of the two social background variables and the two unmeasured achievement orientations; while occupation at panel I depends on education and W , and income at panel I on education, concurrent occupation, and M . Occupation at panel III is represented as an outgrowth of education, prior occupation and W .

Since the model in Figure 2 contains unmeasured variables, ordinary least squares regression procedure cannot be used to estimate the path coefficients. Details are given in the Appendix. Suffice to say that estimation was possible through algebraic manipulation of a reduced form of Figure 2, coupled with the assumption that the correlations of the unmeasured variables W and M with WO and MO (respectively) were equal max-

imally to the reliability coefficients of WO and MO. This is to say that the validity coefficients in Figure 2 ($p_{10,13}$ and $p_{11,14}$) were assumed to be as large as permitted by the reliability with which the reflections were measured (.841 and .717, respectively). Overidentifying restrictions permitted a check on the model, which was generally consistent with the data.

The path coefficients in Figure 2 indicate no statistically significant direct effect of the motivational construct W beyond panel I ($p_{8,13}$ is effectively zero). Thus, W is not able to transmit the effects of social origins to occupational destinations at the end of the follow-up period. In addition, the direct effect of the father's occupation on the son's at panel III ($p_{81} = .074$) is statistically significant, indicating that the "lagged effects" of the father's occupation are not a manifestation of the adolescent achievement orientations and cannot be explained away by them.

Although the path from W to panel I occupational status is significant ($p_{4,13} = .289$), it is relatively less important for determining occupational prestige than in schooling ($p_{43} = .565$). Moreover, the unmeasured adolescent orientations are less effective intervening variables between social

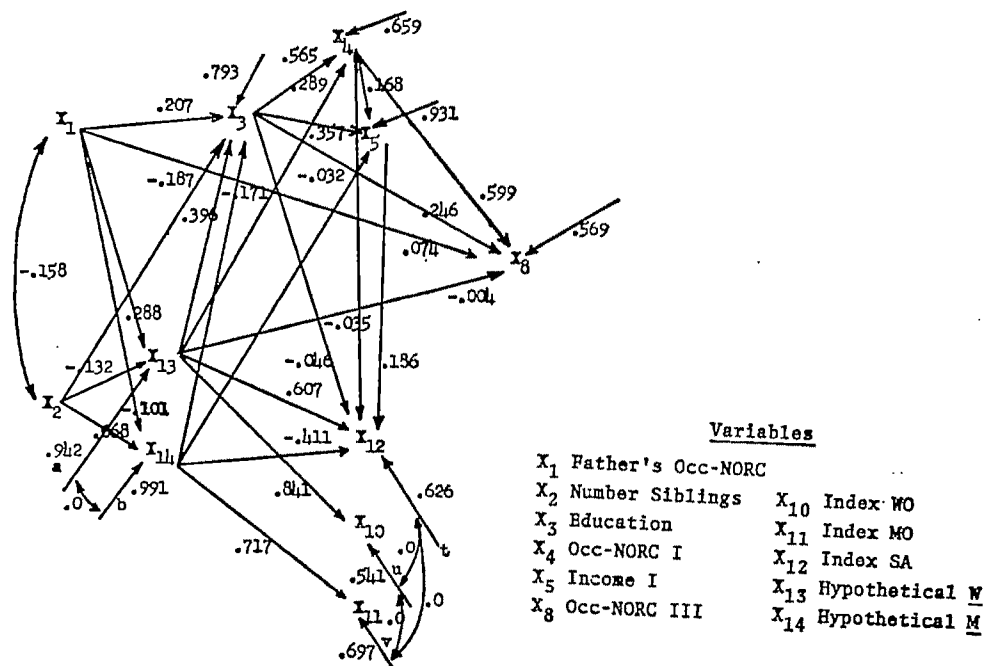


Figure 2. Achievement Orientations and Status Attainment: Hypothetical Variables

origins and panel I achievements than is the education variable. For example, holding constant the number of siblings, the indirect effect of father's occupation on son's occupation at panel I through W is .083; while the effect traced through education is .191. However, the latter path contains a motivational component. Splitting the .191 net effect of father's occupation on son's occupation at panel I through education into that owing to motivations ($.074 = p_{43}p_{8,14}p_{14,1} = p_{43}p_{8,13}p_{13,1}$) and that owing to father's occupation ($.117 = p_{83}p_{31}$), one finds that the motivational component represents only 39%, while 61% of the indirect effect is exerted through non-motivational pathways. Hence, relative to education's ability to transmit the status effects of family to the son's later attainments, the motivational variables are much less potent as intervening variables.

But what of the motivational variables' ability to enlarge the explanation of educational attainment? In Figure 2 both W and M intervene between the predetermined family of origin variables and education. High W is indicative of sons from high status backgrounds and/or smaller numbers of siblings, whereas high M is associated with lower status and/or larger numbers of siblings. The combination of variables X_1 and X_2 account for only a small proportion of the variance in the unmeasured variables, which (by assumption) are temporally coincident and correlated.

The direct effects of W and M on education are of opposite sign ($p_{8,13} = .396$ and $p_{8,14} = -.171$), and the path from W is relatively the most important net effect on years of schooling obtained. The addition of the two motivational variables adds 17% to variance explained in education, over that contributed by the family of origin statuses alone. But in a decomposition of the effects of father's occupation and number of siblings, net of each other, on education, the direct effects are much larger than their respective indirect effects through the unmeasured motivational variables. For example, the direct effect of father's occupation on education (p_{31}) is .207 and the total of indirect effects via W and M is .131; for number of siblings, the direct effect (p_{32}) is $-.187$ and the indirect effects through the

intervening personality variables total $-.064$. Whether these coefficients indicate "high" or "low" relevance for the personality variables is the point at issue. Figure 2 does not encourage introducing achievement orientations into models of educational achievement, where these orientations are hypothesized to arise from family socialization experiences and to function as intervening variables between social origins and destinations.

The hypothetical motivational variables are apparently relevant to educational achievement, in that they add 17% to the coefficient of multiple determination of education. However, these motivational variables have little effect in explaining the components of years of formal schooling which are highly relevant to later socioeconomic career achievements, since the paths from the motivational constructs to actual achievements are nearly zero in most cases and/or substantially smaller than the direct effects of education on the achievements, holding constant achievement orientations. Neither can these constructs be explained by family socioeconomic characteristics, as shown above and illustrated by the substantial residual paths $p_{13,a}$ (.942) and $p_{14,b}$ (.991) in Figure 2.

Thus, these data make some case for the "relevance" of achievement orientations, if one sees such adolescent motivations as unrelated to family socioeconomic status and number of siblings, as having specific power to determine years of schooling, and as providing no explanation for how years of schooling influence later socioeconomic achievements. Such restrictions seem inconsistent with current theories, and call in question the empirical relevance of achievement orientations for explaining social mobility.

DISCUSSION

No support appears for the hypothesis that achievement orientations are highly relevant to the status attainment processes in our population. The indexes of achievement orientations fail to function as intervening variables capable of explaining how social origins (or intervening achievements) affect the mid-career socioeconomic statuses.

This paper has confronted the questions

of the validity and reliability of measurement to the degree permitted by the data. The motivational indexes did not appear to fail as intervening variables because the constructs were unreliable. It might be that the measurements were invalid, though the relationship between validity and the strength of association between motivations and achievements has some interesting ramifications.

Suppose one represents the association between motivation and achievement as in Figure 3, with W being an unmeasured motivation, WO , its imperfect reflection, and occupation, the achieved status. If, as in the data corrected for attenuation, the correlation $r_{8,10} = .42$, it can be written in terms of the diagram in Figure 4 as $r_{10,13}r_{8,13}$, or $p_{10,13}p_{8,13}$. The path, $p_{10,13}$, is the validity coefficient; and, in the three examples in Figure 3, as the degree of measurement validity increases, the ability of the unmeasured motivational construct to predict achievement actually decreases.⁴ So, had the validity estimates in Figure 2 been lower, the potency of the constructs W and M as intervening variables would have been expected to increase.

⁴The author is indebted to O. D. Duncan for this observation (cf. Duncan, Featherman and Duncan, 1968:Chapter 7).

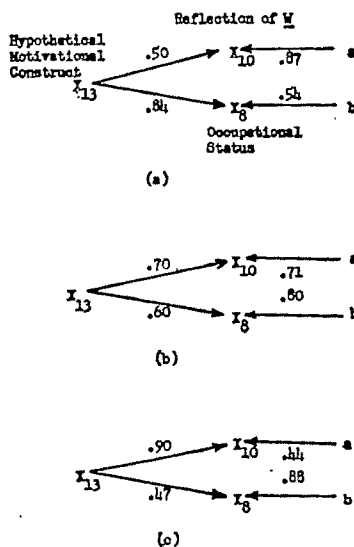


Figure 3. Illustration of Relationship Between Validity and Effect of Hypothetical Construct on Achievement, Assuming $r_{8,10} = .42$ and $r_{ab} = 0.0$

This aspect of model estimation does not vindicate the social psychology of social mobility, however. For the paths from motivational constructs like W and M to the achieved statuses will be large only when the motivational constructs relevant to the adolescent achievement orientations are quite unlike those relevant to adults. While some social psychologists argue that the components of achievement orientation are multiple (Kahl, 1965) and that different components may be potent at different points in the life cycle (Veroff, 1969), none has developed a theory which includes predicted *low* coefficients for validity and/or stability as well as the causal mechanisms by which these orientations arise and subside.

Such a developmental theory is needed to produce data for the claim that motivations are highly relevant to social mobility. This study's motivational dimensions may not be the essential ones, but the burden for finding others rests on those who grant efficacy to personality factors in mobility processes. They need to define these constructs apart from the achievements they are presumed to predict, to construct their theories to make explicit the causes and effects of these intervening motivational variables and to incorporate into these theories expectations for temporal stability of motivational constructs (life cycle specificity) and construct validity (susceptibility to direct measurement). Such a burden is not light, but it should move social science away from the imprecision which severely hampers the testing of long-standing propositions about the relevance of motivation to social mobility.

APPENDIX

Since the model represented in Figure 2 of the text entails unmeasured variables, the ordinary least-squares procedure used in Figure 1 to estimate path coefficients is inappropriate here. Instead, estimates for Figure 2 are derived from its reduced form, represented below as Figure 4, and by the following structural equations:

$$X_{10} = p_{10,13}X_{13} + p_{10,u}X_u$$

$$X_{11} = p_{11,14}X_{14} + p_{11,v}X_v$$

$$X_{12} = p_{12,13}X_{13} + p_{12,14}X_{14} + p_{12,3}X_3 +$$

$$p_{12,4}X_4 + p_{12,5}X_5 + p_{12,t}X_t$$

First, the equation for X_{12} is multiplied

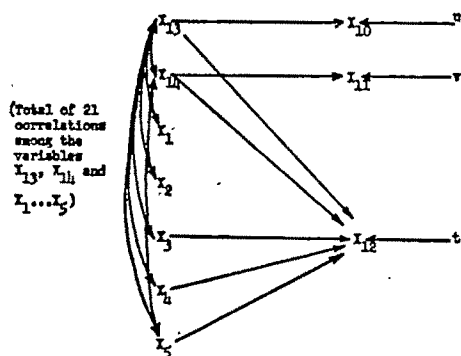


Figure 4. Reduced Form of Figure 2 with Estimated Hypothetical Variables X_{13} and X_{14} (See Figure 2 for definitions)

by each of the $X_1 \dots X_5$, X_{10} , and X_{11} variables, summing over all cases and dividing by N . This yields seven equations in the unknowns $r_{13,1} \dots r_{13,5}$, $r_{13,10}$, $r_{13,11}$, $r_{14,1} \dots r_{14,5}$, $r_{14,10}$, $r_{14,11}$, $p_{12,13}$, $p_{12,14}$, $p_{12,3}$, $p_{12,4}$, and $p_{12,5}$.

Second, it is assumed that the correlations $r_{10,13}$ and $r_{11,14}$ are equal to the reliability coefficients for variables X_{10} and X_{11} (.841 and .717, respectively), on the argument that the "validity coefficients," $p_{10,13}$ and $p_{11,14}$, could not exceed the reliability for the measured reflections of the hypothetical motivations. With this assumption, (and that of zero correlations among the residuals u , v , and t), it is possible to calculate the unknown correlations $r_{13,1} \dots r_{13,5}$ and $r_{14,1} \dots r_{14,5}$ from the known correlations $r_{10,1} \dots r_{10,5}$ and $r_{11,1} \dots r_{11,5}$. Also, the unknown correlation, $r_{13,14}$, is derived from the algebraic path equivalent for the observed value of $r_{10,11}$ (or, $p_{10,13}p_{11,14}r_{13,14}$). With the value of $r_{14,13}$ one can then compute the remaining unknown correlations, $r_{13,11}$ and $r_{14,10}$.

With these calculations, the seven equations yielded from X_{12} in the first step are stated in the five unknown path coefficients. Several arbitrary techniques of estimating overdetermined systems of equations such as this are available (cf. Blalock, 1970; Land, 1970; Christ, 1966:407-411). A somewhat rough and ready procedure is adopted which produces unbiased estimates for recursive models in which errors are assumed to be uncorrelated (Goldberger, 1970). A subset of the estimating equations is selected such that the five unknowns are stated in five

equations (viz., the expressions for $r_{3,12}$, $r_{4,12}$, $r_{5,12}$, $r_{10,12}$, and $r_{11,12}$). The remaining two correlation identities ($r_{1,12}$ and $r_{2,12}$) are used to check for "goodness of fit," even though this check is essentially arbitrary, since there is no determinant criterion to judge whether the deviation of a calculated value from the observed correlation is large or small.

Third, from the set of eight equations yielded from the structural equation for X_8 in Figure 2 (these eight equations are in four unknown paths), four were selected on the line of argument stated above, leaving the remaining four for the "goodness of fit" check; the estimating equations are the identities for r_{84} , r_{83} , r_{81} , and $r_{8,12}$.

Fourth, the "goodness of fit" of the model is assessed by subtracting the calculated or estimated values of correlations not used in the estimating procedure above from their known or observed values. Below are listed these deviations:

$r_{1,12} - .025$	$r_{28} \quad .018$
$r_{2,12} \quad .040$	$r_{58} - .021$
$r_{8,10} \quad .009$	$r_{8,11} - .002$

For the purposes of this exercise, the model is judged (arbitrarily) to be acceptable. Of course, no theory is proven by this demonstration; rather the data do not force a rejection of the model in Figure 2.

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HIGH SCHOOL CONTEXT AND COLLEGE PLANS: THE IMPACT OF SOCIAL STRUCTURE ON ASPIRATIONS *

JOEL I. NELSON

University of Minnesota

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The controversy surrounding the contextual effects of high school status on college aspirations is reviewed. Some of the ambiguity in research findings may be traced to the failure to consider school conditions which foster and hinder college aspirations. Research on high school students in the metropolitan Minneapolis-St. Paul area shows that highly intelligent students are most likely to be found in high status schools. Hence, it may be easier for adolescents at any level of intelligence to get higher grades in low status schools where competition is lean. Within any given category of measured intelligence, attending a high status school appears to raise scores on a less potent predictor of aspirations, school status, and lower scores on a more potent predictor of aspirations, academic rank position. Since rank is an important predictor of college aspirations under all conditions, controlling for rank increases the correlation between school status and aspirations. The analysis suggests that although the effects of school status tend to cancel each other out and the net effect on aspirations is small, the theoretical importance of school status cannot be discounted.

SIX years ago, William Sewell and J. Michael Armer (1966) published a critique of the hypothesis that the social status of high schools influences college plans of adolescents. Marshalling data from public high school seniors in Milwaukee, the authors showed that school status, as measured by the percent of high status students in the school, explained less than two percent of the variance in aspirations after individual students' social class position, intelligence scores and sex were controlled.

The Milwaukee research was the first to reverse a host of earlier studies which supported an association between aspirations and school status (Boyle, 1966; Campbell and Alexander, 1965; Rogoff, 1962; Turner, 1964; Wilson, 1959; Coleman, 1961). This

earlier work suggested the importance of the normative functions (Kelley, 1952) of school status, which set and reinforce group standards on the value of aspiring to go to college. It advanced the hypothesis that school status—by creating a particular climate or developing particular relationships—contributed to adolescents' aspirations over and above their socio-economic position.

Although Sewell and Armer's findings have been duplicated in other settings (McDill, Meyers and Rigsby, 1967; Hauser, 1969), the results do not coincide with what is generally known about college aspirations. Previous research suggests that high status students are more likely than low to plan on attending college (Sewell, Haller and Straus, 1957), and that fellow students influence college aspiration decisions (Kandel and Lesser, 1969; Haller and Butterworth, 1960). Since high status students chiefly in-

* Funds for this research were supplied from Project 2716H, Agricultural Experiment Station, University of Minnesota.

habit high status schools, school status should explain some variability in college aspirations (Valkonen, 1969).

The design of the Sewell-Armer research could have obscured the important and complex role of school status. Research should consider the two-fold consequences of social structure: the *normative* which sets standards and the *comparative* which provides criteria for evaluating behavior (Kelley, 1952; Davis, 1966). Each set must be analyzed since both may work against each other in schools: that is, in high social status schools, students benefit, in their aspirations, from interacting with others disposed toward college but suffer comparatively by having to compete with them.

The Milwaukee research suggests the comparative function of schools in indicating that more highly intelligent students attend high status than low status schools. Previous research suggests that the higher the average level of student intelligence in a school, the less likely any one student will feel academically competent (Myer, 1970). As for performance, an adolescent scoring in the middle intelligence range (compared to all Milwaukee adolescents) will probably rank near the top of the class in a low status school where the competition is lean, and near the bottom in a high status school where the competition is intense. These different positions are a function of the nature of ranking systems: no matter how many adolescents of varying intelligence attend a school, some will appear towards the top of the distribution and others towards the bottom (Coleman, 1959). Now, if adolescents of varying intelligence more likely rank higher in a low status than a high status school, an important point follows for Sewell and Armer's research design: when one controls for intelligence in such studies, one, in effect, compares the aspirations of highly ranked adolescents in low status schools with lesser ranked adolescents in high status schools.

Previous findings show a strong association between academic work and college aspirations (Berdie, 1954:107-111). The higher the rank, the more likely the students will consider college. This relationship holds among students of varying intelligence. The finding supports the principle that local rank systems strongly influence social behavior.

In short, for individuals of a given intelligence, attending a high status school simultaneously raises scores on one predictor of aspirations (i.e. school status) and lowers scores on another predictor of aspirations (i.e. rank).

These contradictory influences may make the net effect of school status negligible—not because school status is unimportant but because its effects are complex and contradictory. This paper will include rank in the analysis of context effects to suggest the importance of the two consequences of school status.

RESEARCH DESIGN AND ANALYSIS

Sample. The data for this research were drawn from materials collated in 1967 by the Student Counseling Bureau, University of Minnesota. As part of a state-wide testing program the Bureau annually administers an aptitude test and questionnaire to each junior in every Minnesota high school. Like Sewell and Armer's, the present research is limited to the schools in the state's largest urban center—here, the forty-five public high schools in and around Minneapolis and St. Paul. The analysis involves 17,976 students.

State-wide, the respondents appear reasonably to represent the population of Minnesota's junior year adolescents. Minnesota has among the lowest drop-out rates in the nation: for every hundred students starting high school, about ninety-two percent finish (Howell, 1968).

Measurement. We used both parents' education and not the father's occupational position to measure socio-economic status. The Bureau received poor responses to the item on father's occupation; twenty percent of the sample merely checked "other." Three positions for the parents' education were delineated: high—where one parent had some college and the other was at least a high school graduate; intermediate—all other cases where one parent had some college as well as all other cases where at least one parent had completed high school; low—where both parents had only some high school or less.

The structural measure of school status was obtained by calculating the percent of

high status students in each school. The school categories were divided into three groups consisting of schools with high status students in the ranges of 33% and less, 34-45%, 46% and more. We selected these cut-points to equalize the number of schools and students in each category. Originally, our analysis was carried out with five categories of school status, and five categories of individual status, but the resulting numbers in particular cells did not suffice for stable estimates.

The other major variables were measured in standard fashion. We took an indication of college aspirations from those who answered "college or university" to a question on what, if any, further schooling was planned after high school. Intelligence scores were gathered from the respondent's performance on the Minnesota Scholastic Aptitude Test, a modified form of the Ohio Psychological Examination (Berdie, et al., 1962). Rank position reflected the student's relative grade point average in his school and was taken directly from education records. We divided both rank and intelligence into equal thirds for the analysis.

RESULTS

The results show, as predicted, that controls for intelligence, in evaluating a school's context effects, confound the ranks of adolescents in various schools and hence blur the direct influence of school status on aspirations.

Table 1 presents the baseline data for evaluating the influence of school context on aspirations independent of rank position. These findings conform to Sewell and Armer's research. Aspirations and school status are moderately related. With socio-economic position and intelligence separately controlled, the association diminishes; with the same variables jointly controlled, the association is negligible. The data parallel the Milwaukee results and support the contention that, stringently controlled, the school context has little if any uniform influence on college aspirations.

We had thought that rank partly confounded the Milwaukee findings because there were more highly intelligent adolescents in high status than low status schools. An

Table 1. Correlations (Gamma) Between High School Status & College Aspirations by Sex with Separate & Simultaneous Controls for Socioeconomic Status & Intelligence

Control Variables	Correlations Between High School Status & Aspirations	
	Males	Females
Zero Order	+ .33	+ .33
Socioeconomic Status	+ .20	+ .18
Intelligence	+ .17	+ .20
Socioeconomic Status and Intelligence	+ .07	+ .08

examination of the detailed data (not required here) for Table 1 shows that in Minneapolis-St. Paul, as in Milwaukee, intelligence and school status are related. Among males, for example, 48.1% in high status schools are in the high ability category whereas this is true for 35.0% in medium status, and 29.0% in low status schools. A similar pattern holds for females. This distribution suggests: first (by definition) high status students prevail in high status schools and regularly score higher in measured intelligence; second, within any category of socio-economic position, students from high status schools still score higher in intelligence. The findings support the skewed intelligence distribution in schools of varying status.

If ability is concentrated in high status schools, then adolescents at any level of intelligence should rank lower in a high status school than a low status school. That is, bunching intelligence in high status schools increases the competition for—and probability of attaining—high academic rank. Table 2, which reports the percent attaining a high rank position by intelligence categories in each school, shows in part the extent of the relationship—that more adolescents in any intelligence category are likely to rank higher in a low status than a high status school. Among males, for example, in the low intelligence category, 5.8% rank high in a low status compared to 1.4% in a

Table 2. Percent of Students Attaining a High Academic Rank by High School Status, Intelligence and Sex

High School Status	Low Intelligence	Middle Intelligence	High Intelligence
Males:			
Low	5.8 (1274)	28.0 (1018)	69.3 (674)
Middle	2.5 (947)	15.8 (1056)	69.3 (1055)
High	1.4 (576)	11.1 (944)	53.5 (1396)
Females:			
Low	11.9 (1206)	47.9 (1085)	85.5 (794)
Middle	6.2 (804)	29.0 (1093)	75.2 (1245)
High	3.2 (476)	20.8 (869)	66.4 (1430)

high status school; for females, the percentages are 11.9% and 3.2%. Generally, within each intelligence category, rank and school are more closely related for females than males.

Note that the adolescent's intelligence rather than his socio-economic position, accounts for the different rankings in various schools (see Table 3). In each socio-economic category, students are only slightly more likely to attain high rank in a low status than a high status school. However, this slight edge seems merely to reflect ability: when intelligence and socio-economic position are simultaneously controlled, the correlation between school status and rank is about equal to when intelligence alone is held constant. The finding argues that the distribution of intelligence is the key contingency for rank in schools of varying social status.

Table 3. Correlations (Gamma) Between Rank Position and High School Status with Separate and Simultaneous Controls for Socioeconomic Status and Intelligence by Sex

Control Variables	Correlations Between Position and High School Status	
	Males	Females
Intelligence	-.27	-.30
Socioeconomic Status	-.10	-.13
Intelligence and Socioeconomic Status	-.29	-.33

Tables 2 and 3 suggest that in comparing adolescents of a given intelligence across school statuses one is in effect comparing lower ranked with higher ranked adolescents. Rank, therefore, must be introduced into the analysis of contextual effects. Our research shows that rank uniformly affects aspirations. A few findings will illustrate its power. For example, among males in the middle intelligence and socio-economic categories in middle status schools, 52.7% of the low rank students aspire to go to college compared to 74.2% in the middle rank and 79.2% in the high rank; for females in the same categories the percent who aspire to go are 28.2%, 39.0% and 55.3% respectively. The rank-aspiration association, with sex, socio-economic status, intelligence and school status partialled out, is .48.

These data suggest the importance, in comparing school status differences, of what Davis (1966) and Kelley (1952) call the comparative function of reference groups: the function which gives standards for judging behavior. In schools with many intelligent adolescents, students tend (at any intelligence level) to rank lower; and in each such school, lower rank is related to a lower frequency of college aspirations. Hence, the comparative function of school contexts is to reduce aspirations as the school status level increases. This fact tends to blur the normative function of schools which set and reinforce standards for aspirations.

Table 4 which gives a glimpse into the normative process, reports the zero order correlations of college aspirations and high school status, and the correlations with rank standardized for each control category of

Table 4. Correlations (Gamma) Between High School Status and College Aspirations by Socioeconomic Status, Intelligence and Sex With and Without Controls for Rank

Categories of Intelligence and Socioeconomic Status	Males		Females	
	Correlations Between High School Status & College Aspirations		Correlations Between High School Status & College Aspirations	
	Rank Not Controlled	Rank Controlled	Rank Not Controlled	Rank Controlled
Low Socioeconomic Status				
Low Intelligence	-.09	+.02	-.07	+.09
Middle Intelligence	+.07	+.20	+.07	+.16
High Intelligence	-.01	+.21	+.05	+.19
Middle Socioeconomic Status				
Low Intelligence	+.10	+.15	+.27	+.33
Middle Intelligence	+.08	+.24	+.04	+.12
High Intelligence	+.02	+.19	+.15	+.21
High Socioeconomic Status				
Low Intelligence	+.20	+.27	+.17	+.33
Middle Intelligence	+.11	+.26	+.16	+.29
High Intelligence	+.15	+.32	+.07	+.11

intelligence, sex and socio-economic status. Although the statistics are erratic over eighteen categories, the gammas are uniformly higher when rank is controlled than when it is not. The correlation of aspirations to high school status is .07 (see Table 1) with controls for status, intelligence and sex; whereas the same correlation with rank as a fourth partial is .20.

How, then, does school status explain college aspirations? We showed that it is negatively related to rank ($-.31$) with SES and intelligence partialled out and that rank in turn is positively related to aspirations, with SES, intelligence and school status partialled out (.48). Coupled with the positive relationship of school status to aspirations shown in Table 4, the associations suggest the following: that the direct effects of school status on aspirations are being cancelled out by the negative indirect effects of school status on aspirations related through rank placement.

The findings agree with Sewell and Armer's observations that school status has no significant *net* effect on aspiration variability, but they explain the case differently. Sewell and Armer suggested that school status is theoretically unimportant because it fails to predict aspirations—over and above other variables. Our analysis suggests that school status has no net effect on aspirations because of its complex and contradictory in-

fluence. The fact that school status effects cancel each other out should not detract, however, from this variable's important *theoretical* significance.

CONCLUSIONS

The above data are critical for reopening the debate on the effects of high school status and for illustrating some structural analysis difficulties. In discussions of methods in context research, the recommended steps of analysis are to: (1) identify a structural variable and correlate it with a relevant dependent variable; (2) subject the correlation to an array of controls which might account for individual explanations of the dependent variable; (3) assign the remaining portion of that correlation to variation in the social structure (Blau, 1960; Davis, Spaeth and Huson, 1961). Step two is critical. It "equalizes" or "matches" the groups involved to eliminate member differences as sources of explanation. Sewell and Armer's use of intelligence as a control variable followed this procedure. Yet as our data illustrate, using controls introduces other confounding factors in the analysis—here the indirect effects of rank. Control variables are central to analyzing structural effects. But they shouldn't be recommended simply for approximating tight experimental designs. Control variables have consequences which must be carefully considered.

For this reason, further work on structure or context analysis requires extended consideration of rank and other contingency effects. Such work could be an antidote to the approach—which, in stratification research, generally ignores local contexts and views all stratification structures as equal. In context analysis, we may be developing a central principle of social stratification—that individuals may, involuntarily, be ranked at the bottom of an unequal system which, talent and ability notwithstanding, can adversely effect their aspirations and behavior. For the high schools examined, our data in part support this broader principle of stratification and social structure.

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SIZE AND GROWTH AS DETERMINANTS OF ADMINISTRATIVE-PRODUCTION RATIOS IN ORGANIZATIONS

GERRY E. HENDERSHOT AND THOMAS F. JAMES

Vanderbilt University

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Previous research has usually found a negative relationship between organization size and the ratio of administrative to production workers. That research has almost always relied on cross-section data, so that the effects of growth over time could not be considered separately from the effects of size at one point in time. This study uses data on U.S. school districts at two points in time to investigate the interrelationships of size, growth and administrative-production ratios. We find that both size and growth have small but significant negative relationships to administrative-production ratios.

MOST previous research on the subject has found a negative relationship between organization size and the ratio of administrative to production personnel; i.e., the larger the size of an organization, the smaller the number of administrative workers relative to production workers. For example, negative relationships of this kind have been reported in studies of hospitals (Anderson and Warkov, 1961; James, 1972), industries (Bendix, 1956; Rushing, 1967a; Rushing, 1967b; Haire, 1959), universities (Hawley, *et al.*, 1965), Canadian schools (Holdaway and Blowers, 1971), government agencies (Blau, 1970), and a sample of mixed types of organization (Haas, *et al.*, 1963). While there has been at least one reported finding of a *positive* relationship in American public school systems (Terrien and Mills, 1955), the preponderance of evidence points to a negative relationship between organization size and the administrative-production ratio. While recurrently found, the relationship seems not to be especially strong; where interpretable statistics have been presented, they indicate statistically significant but low correlations.¹

Nearly all the studies cited above used cross-section data; i.e., the measures of administrative-production ratios and size were specific to one point in time. In those studies

the independent variable has been *size*; little has been done to investigate the effects of changes in size over time, or *growth*, on administrative-production ratios. While the difference between size and growth is sometimes overlooked in the study of organizational structure, they are conceptually distinct factors which may operate differently on administrative-production ratios.

In a recent study of Canadian school systems, Holdaway and Blowers measured administrative-production ratios over a five year period, and reported no consistent tendency for the ratios to increase or decrease. While their study is indeed longitudinal, it takes as the dependent variable *time* itself, not growth; while time may be an important variable (especially when it can be taken to indicate age), growth is theoretically more important in determining organization structure (Noell and Heydebrand, 1971). Haire's work on the case histories of four organizations is a longitudinal study of growth of the kind needed, but the small size of his sample makes generalizing the results difficult. A large sample, longitudinal study of the effects of growth on administrative-production ratios has not previously been done.

This paper reports such research on the effects of organizational growth and size on the ratio of supervisors to teachers in American school districts. The basic data sources were publications of the United States Office of Education (1954; 1960) which provide information on the numbers of prin-

¹ We say this on the basis of limited information since in most of the earlier studies simple chi-square, or non-parametric tests were performed. But, for example, James (1972) reports a product-moment correlation of $-.32$ ($r^2 = .10$).

cipals, supervisors, teachers and students in all large school districts and a sample of smaller districts. Included in this study were all districts in cities with population of 25,000 or more, for which the necessary data were reported in both the 1949-50 and 1955-56 school years. A total of 299 districts are included.

For each district in the study measures were computed of size and the ratio of supervisors to teachers in both 1949-50 and 1955-56, and of growth between those two dates. Size was measured by the number of students enrolled.² The supervisor-teacher ratio was measured by the number of principals and supervisors, divided by the number of teachers.³ Growth was measured by the proportional (%) increase in student enrollment between 1949-50 and 1955-56.

In addition to the above variables, two other change variables were computed. Following the recommendations of Bohrnstedt (1969) we created residualized indexes of change in both size and the administrative ratio.⁴ This enabled us to build a multiple regression model of the longitudinal effects. We will present data using both the raw change scores (tabular analysis) and the residualized scores (regression analysis) in this report.

FINDINGS

Table 1 shows the supervisor-teacher ratios for four size-classes and two dates

² We take student enrollment as the measure of system size merely because the total number of employees of all types is not furnished. Assuming that the number of employees is directly related to the number of students in the system, then enrollment is satisfactory for our purposes.

³ We use the number of teachers as the base of our ratio assuming that the school administration is concerned primarily with the activities of the teachers. The coordination of other activities we assume to be relatively unimportant in that they are probably handled through routine, easily planned procedures.

⁴ The regression equation of the administrative ratio (time 1) by the administrative ratio (time 2) predicting the time 2 scores was:

$$Y = 3.352 + .4335(X) + e \quad (R = .46).$$

In the case of the regression of size time 2 on size time 1 predicting size time 2 the equation was found to be:

$$Y = 2767.93 + 1.087(X) + e \quad (R = .99).$$

Table 1. Principals and Supervisors per 100 Teachers, by Enrollment: U.S. School Districts in Cities of 25,000 or More, 1949-50 and 1955-56, and Product-Moment Correlations Between Supervisor-Teacher Ratio and Enrollment in the Same Years.^a

Enrollment	1949-50	1955-56
All districts	6.29 (299)	6.07 (299)
Less than 7,000	6.89 (85)	6.55 (51)
7,000 to 10,999	6.41 (78)	6.41 (85)
11,000 to 19,999	6.00 (68)	5.76 (75)
20,000 or more	5.69 (68)	5.73 (88)
Correlations (N=299)	-.13 ^b	-.13 ^b

^a Number of cases on which means are based are shown in parentheses.

^b Probability < .05.

along with product-moment correlations between size and the ratio at both points in time. Note in both years the product-moment correlation is $-.13$ ($p < .05$), indicating a stable negative relationship and providing added evidence of a general negative relationship between organization size and the administrative ratio.

Table 2 considers the effects of growth. Districts are grouped by the proportional change in enrollments between 1949-50 and 1955-56; to control for the known effect of size, change is considered separately within size categories. The measure of change in the supervisor-teacher ratio is the difference between the ratios at the two dates, averaged over all the districts in a size-growth category.

Considering all districts regardless of size (the first line of Table 2), note that districts whose enrollments grew slowly (less than 12.5%) had an average increase in the supervisor-teacher ratio; that is, the number of principals and supervisors increased more than the number of teachers. In districts which grew more rapidly, however, the ratio *decreased* on the average; the number of principals and supervisors increased less than the number of teachers. With few exceptions the same pattern is observed within

Table 2. Change in Principals and Supervisors per 100 Teachers 1949-50 to 1955-56, by Enrollment in 1949-50, by Enrollment Growth (%) 1949-50 to 1955-56: U.S. School Districts in Cities of 25,000 or More.^a

Enrollment 1955-56	Enrollment Growth 1949-50 to 1955-56				
	All lev- els	Under 12.5%	12.5-24.9%	25.0-37.4%	37.5% or more
All districts	-.22 (299)	.27 (89)	-.50 (81)	-.16 (64)	-.62 (65)
Less than 7,000	-.39 (85)	.26 (27)	-.67 (24)	-.33 (18)	-1.16 (16)
7,000 to 10,999	-.33 (78)	-.13 (23)	-.50 (24)	-.35 (16)	-.36 (15)
11,000 to 19,999	-.06 (68)	.48 (17)	-.56 (16)	.08 (19)	-.31 (16)
20,000 or more	-.05 (68)	.54 (22)	-.22 (17)	-.02 (11)	-.64 (18)

^aNumber of cases on which means are based are shown in parentheses.

size categories. Thus, to the extent that one can generalize this finding, it suggests that the effect of organization growth on the administrative-production ratio depends on the magnitude of growth. Slow growth, it appears, tends to produce an increase in the ratio, but rapid growth tends to produce a decrease.

A plausible interpretation of this pattern may be that when organizations grow at a slow rate, their component parts have sufficient time to adjust themselves to the new conditions of increased size. Because larger size brings disproportionate increases in administrative difficulty,⁵ the administrative component tends to increase somewhat more rapidly than the production component, resulting in an increase in the administrative-production ratio. When organizations grow at a rapid rate, however, the various organizational components have insufficient time to adjust; the "natural"

balance of the functional parts is distorted.

Under such conditions the component of the organization most directly and immediately affected by the stimulus to grow will grow most rapidly, and increase its relative numerical importance. If the stimulus to growth most immediately affects production workers (as the post World War II Baby Boom most immediately affected school teachers), then that component will increase relative to the administrative component, and the administrative-production ratio will decline (as it did in rapidly growing school districts). Rapid growth most immediately affecting the administrative component (as, for example, a large increase in funds appropriated to a school district with no increase in enrollment), might have the opposite effect.

This interpretation suggests that low administrative production ratios may be, in many cases, transient phenomena, the result of a recent history of rapid growth. With more moderate growth rates the "normal" relationship between administration and production will be reestablished by an increase in the relative numbers of the former.

Furthermore, this interpretation suggests that comparisons of the administrative-production ratios of organizations of different size may be confounded by differences in their recent history of growth. If, for instance, the large organizations in a sample had recently experienced rapid growth (a likely situation), and the small organizations had been relatively stable in size, then the relationship between *growth* and the administrative-production ratio might give rise to a spurious negative relationship between *size* and the same ratio. In such cases recent growth history must be controlled to investigate the independent effect of size.

Table 3 gives an analysis of this type, where the 1955-56 supervisor-teacher ratios are presented for various categories of size (1955-56) and growth (1949-50 to 1955-56).

Note that only among the most rapidly growing districts is there a consistent negative relationship between enrollment and the supervisor-teacher ratio. In slower growth categories the relationship, though suggested by the trend of ratios, is not so clear.

⁵ We refer the reader to Blau's article (1970) for the connection between size and administrative difficulties. The connection comes through the relationship of system size and differentiation.

Table 3. Principals and Supervisors per 100 Teachers, by Enrollment in 1955-56, by Enrollment Growth (%) 1949-50 to 1955-56: U.S. School Districts in Cities of 25,000 or More.^a

Enrollment 1955-56	Enrollment Growth 1949-50 to 1955-56				
	All lev- els	Under 12.5%	12.5-24.9%	25.0-37.4%	37.5% or more
All districts	6.07 (299)	6.30 (89)	6.27 (81)	6.16 (64)	5.44 (65)
Less than 7,000	6.55 (51)	6.65 (25)	6.70 (14)	5.80 (9)	6.91 (3)
7,000 to 10,999	6.41 (85)	6.54 (24)	6.57 (31)	6.48 (17)	5.76 (13)
11,000 to 19,999	5.76 (75)	5.66 (18)	5.74 (15)	6.12 (21)	5.54 (21)
20,000 or more	5.73 (88)	6.20 (22)	5.95 (21)	5.72 (17)	5.37 (28)

^aNumber of cases on which means are based are shown in parentheses.

Perhaps in a sample of organizations with declining or stable size, the relationship would disappear altogether, or be reversed. In any case, there is an apparent need for some control on growth when considering the effects of size on the administrative-production ratio.

Thus far we have shown that size and growth seem related to the administrative-production ratio, and that growth may affect the size-administrative ratio relationship, but we do not know the statistical significance of these findings. First, we cannot compare the two independent variables except by crudely estimating the relationships' strengths; and second, the raw change scores may indeed be accounting for at least part of the observed growth relationship.⁶

To examine the longitudinal effects of growth we constructed the following multiple regression model:

⁶Bohrnstedt indicates that the procedure of subtracting a time 1 score from a time 2 score often leads to a negative relationship between initial standing and change (1969:116). This phenomenon is due to regression effects.

$$Y = a + b_1X_1 + b_2X_2 + e,$$

where:

Y = the residualized change in the administrative ratio, X_1 = the residualized change in organizational size, and X_2 = organizational size at time 2 (1955-56).⁷

Table 4 indicates the values found by least-squares regression to be the best estimates of the standardized parameters of b_1 and b_2 in the above equation. The multiple correlation here is .129 ($p < .08$). The correlation between X_1 and X_2 was .10. Thus, while the model is not especially strong, we may be reasonably sure that the relationships discussed above are not due solely to the peculiarities of raw change scores.

That the effects of growth are of some importance is born out also. When size (X_2) is dropped from the equation leaving growth (X_1) as the sole predictor, the multiple correlation drops only slightly to .116 ($p < .04$); but when size is used as the sole predictor the multiple correlation drops considerably to .069 ($p < .22$).

CONCLUSIONS

This brief paper makes three points. First, it provides added evidence for the existence of a general negative relationship between organization size and the administrative-production ratio. Second, it shows

⁷We use size at time 2 here for the obvious reason that size time 1 will be uncorrelated with the residualized growth scores.

Table 4. Results of Regression Analysis: Growth in Administrative Ratio by Growth in Enrollment (1949-50 to 1955-56) and Enrollment (1955-56).

Independent Variable	Standardized Beta Weight
Growth in enrollment (1949-50 to 1955-56)	-.11
Enrollment (1955-56)	-.06
Multiple Correlation = .13 ($p < .08$) (N=299)	

that among school districts the rate of organization *growth* is also negatively related to the administrative-production ratio, independent of size. Third, it suggests that differences in recent histories of growth may confound comparisons of administrative-production ratios in organizations of different size, and demonstrates that such a confounding effect exists for school districts.

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HOUSEHOLD COMPLEXITY IN NINETEENTH CENTURY FRANCE *

WILLIAM L. PARISH, JR. AND MOSHE SCHWARTZ *

University of Chicago and Tel Aviv University

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The study of family change during industrialization has been hindered by the lack of adequate historical data. This paper attempts to overcome this obstacle by further refining Coale-Burck measures that can be used with existing census data. It shows the validity of these measures for France, uses them to analyze the effects of land holding, agriculture, illiteracy, and dispersed settlement on household complexity, and then analyzes the effects of household complexity on fertility, migration, suicide, divorce, religiosity, and care for the aged during the nineteenth century.

DURKHEIM was wrong when in his course on the family, he cavalierly dismissed LePlay as a mere propagandist. Of course, LePlay was a propagandist; his main object was to restore to France what he believed could save her, namely the paternal authority of the traditional family. He believed that a strong paternalistic family could "tame" the young generation and restore respect for moral law. Far from hindering his perception, his commitment probably sharpened it. Expectedly, and as we shall show, his assessments were not always accurate; but he was nevertheless able to assess events more accurately than many of his contemporaries. To the insights of conservative and reactionary thinkers (Burke, Bonald, deMaistre) about the problems of post-revolutionary society, LePlay added fieldwork with careful, empirically sound observations of families in a large number of European societies.

Marc Bloch, a man of a completely different political temper from LePlay, wrote what is still considered the authoritative study on French rural history (1952). He described the nuclearization of the family and assigned physical locations to nuclear versus complex families much as LePlay, though his analysis of causes was radically different.

Our purpose is to test some propositions

concerning French families, mostly from LePlay, but also from Bloch and Durkheim. These propositions refer to the existence and location of three types of families in rural France and to the antecedents and consequences of each type. The data with which we shall test the propositions are historical French census data and vital statistics.

LePlay's Family Types

LePlay described three family types in human society and especially in France: (1) the "unstable" family—LePlay's derogatory term for the nuclear family; (2) the stem family, and (3) the patriarchal or joint family.

(1) The nuclear family is characteristically composed of a parental couple and their children who will leave the parental household after they are grown. All parental property is divided equally between sons and daughters—or at least between all sons; daughters receive a dowry.

(2) In the stem family, one child, male or female, marries and stays within the household while the others leave (occasionally, those who renounce marriage stay in the household).¹ Eventually, when the parents are too old, the younger couple assumes responsibility for the household. Typically, the principal heir keeps at least all non-movable property. The other sons

* We are grateful to Remy Clignet, Lloyd Fallers, Morris Janowitz, Duncan MacRae, and Vera Zolberg for suggestions on an earlier version and to Florence Levinsohn for helping us both to think and write more clearly.

¹ The exact inheritor varied from region to region. For example, it was the eldest son in Dordogne, the youngest son in Quimper (Morbihan), and the eldest daughter in some places of the Pyrénées (France 1909).

and daughters inherit a lesser part of the property and are paid in cash or cattle, but not in land (LePlay, 1870:12). LePlay contends that this form of inheritance, once characteristic of most of France, was disappearing under the influence of the Napoleonic Code which instituted compulsory equal sharing of the inheritance. Bloch (1952) denies this, vigorously contending that equal inheritance was the predominant custom in France before the Revolution—except among the nobility, for which primogeniture prevailed. Furthermore, Bloch saw the Napoleonic Code as a consequence rather than as a cause of the changes in inheritance practices.²

(3) In the patriarchal or joint family, the sons and sometimes daughters remain under the parental roof after marriage, creating, thereby, a complex multigenerational unit. When the household can no longer support all its members, some leave, taking with them cash or movable property such as cattle, to create a new patriarchal unit. LePlay is not very clear about inheritance rules in this type of family. Presumably the arrangements are quite complex since the head of the household is considered the trustee more than owner of the means of production. In any case, as with the stem family, this mode of succession is more likely to insure the integral transmission of family land.

Antecedents of each type. Though neither presents a closely reasoned argument, LePlay and Bloch seem to have three major explanations why complex stem and patriarchal families occur in particular regions and why they disappear over time. First, LePlay generally emphasizes that most complex families will be found in agriculture, primarily among those who own or have long-term leases over their land and, thus, an interest in passing the land intact to their sons (LePlay, 1870: 50, 59, 105). Without either landholding or long term leases, cashcroppers and laborers would more likely have nuclear families. Generally, we might expect more complex families among peasant agriculturists: (1) because extra labor is needed at planting and harvesting times, (2) because of di-

versified economic activities such as animal tending and weaving (Wolf, 1966:66-67), (3) because sons cannot easily, in a fully settled society, find farm land other than through inheritance, (4) finally because the long agricultural apprenticeship probably promotes obedience to the father (Zelditch, 1964:492-97; Goode, 1963:375).

For LePlay, the contact with modernist (revolutionary) ideas that literacy provided undermined traditional ideas of family inheritance; and, eventually, families split apart (LePlay, 1870:192). Similarly, Bloch seems to see the spread of nuclear households as a long term cultural trend, but one which began long before the French Revolution. Starting in the plains, it spread only slowly to the more "backward," less literate mountain regions where more complex families were still to be found until very recently. Literacy, then, can be seen as an index of the degree to which a region responds to a national trend. A long term trend toward nuclearization, preceding both the Revolution and industrialization, is illuminated by three observations: (1) While prevalent in most of France in the early Middle Ages, lineages had declined by the eleventh century. (2) The rule of indivisibility of the "manse," which meant one could not dissolve large family estates, had weakened considerably during the thirteenth century. (3) The "communautés taisibles"—large kinship-based agricultural communities with as many as seventy members that were found in central France and the Massif Central—began to disappear outside Paris as early as the sixteenth century, and were almost completely absent from regions of dispersed habitat, such as Poitou, by the nineteenth century (Bloch, 1952:167-170).

As a third argument, Bloch implies that where poor soil compelled peasants to settle at some distance from one another in order to find sufficient soil to cultivate, they turned to their families for support and assistance. Where good soil allowed peasants to settle in villages, their closeness, with the need to control crop rotation, and maintain elongated fields for strip plowing caused people to turn to the community for social support and assistance. Solidarity of the community, thus, tended to substitute for family solidarity

² Desnottes also held this opinion (France, Institut National, 1954:58).

(Bloch, 1952:165-171).³ Poor lands and dispersed habitat correspond to enclosure regions in the Massif Central and some parts of the West, while strong village collectivities are found in the open field regions with open and regularly shaped, elongated fields in the North and East of France, including Champagne and Bourgogne. Bloch, in addition, notes a third region intermediate from the point of view of village solidarity (Garonne, Poitou, Berry) where fields are open, but irregularly shaped (Bloch, 1952: 49-50). In other words, the regions tend to correspond to LePlay's nuclear, joint and stem regions.

Though not specifically discussing France, other authors like Simmel and Redfield provide a fourth explanation for change in family structure. They stress the power of urban life to weaken primal solidarities. To the extent that the obverse is true, that rural life promotes family complexity, this should show up in a multivariate analysis including such factors as agricultural labor, literacy, and dispersed habitat.

Consequences of each type. For LePlay, the consequences of more complex stem and joint families include greater fertility, migration, stability, and security and, for Durkheim, greater social integration. (1) Not having to divide the land, stem and joint families can have more children without fear that their fortune will be decimated by too many inheritors. At least, according to LePlay, this was the case until the Napoleonic Code forced equal sharing of the inheritance (LePlay, 1870:25, 74, 75, 80n). (2) Because non-inheriting sons who wish to marry must leave the family, higher fertility brought more out-migration in the stem family regions than other regions. In nuclear family regions, land transactions are frequent. Those who have inherited subdivided plots, too small to provide subsistence, often sell them; and the younger generation will often wait for an opportunity to buy more land, thus reducing out-migration. In joint families, sons remain working together on the same plot of land. Both nuclear and joint family sons will flee the land in short periods

of agricultural crisis, but they will return once the crisis is over. Only the stem family provides the steady, long term migration necessary to a modern industrial labor force—one that will stay in cities long enough to learn a skill then remain to use it. (LePlay, 1870:104, also Habakkuk, 1955).

(3) For LePlay, complex families provide a home for potential outcasts like the old and those kin who can't afford or don't want to marry and set up their own home. Further, though the patriarchal family is too tradition oriented for LePlay, he believes that both stem and patriarchal families foster traditional values which are more comforting than the "over-innovativeness" of the "unstable" nuclear family (LePlay, 1870: 10). For Durkheim, complex families are more integrated simply because they bring people into greater social interaction with significant others (Durkheim, 1951:198-202). These ideas can be tested in part by examining the relations among suicide, divorce, religious values, and family type.

METHODS

The main body of our data comes from the French census of 1856, 1876, and 1901. Before 1856 the number of households is not reported in censuses available to us.⁴ After 1901, as modernization spreads there are too many disturbing influences for our ecological analysis. The 1962 census is used at points, however, to validate our nineteenth century measures and show that regional differences persist today.

Sample. We expect complex households mainly in rural areas and in agricultural occupations. Because the 1856-1901 censuses give number of households only for the overall population, our indices are weighted averages of all occupations, of both urban and rural populations, and of any other characteristics which vary in the total population. To avoid the heterogeneity of large cities, and to eliminate the skewed distribu-

³ At one point Bloch imputes communal habits to the diffusion of the plow (1952:56-57). Elsewhere he argues just the reverse 1963:648 and *passim*).

⁴ Also, according to Camp (1961:30-31), the 1856 census was the first true French census, for it abandoned the practice of using estimates based on vital statistics and established a single day as the effective day of the census. The 1876 census was even more accurate, and prior to 1962, the 1901 census "was probably the most nearly perfect of all French censuses" (Camp 1961:31).

tions and curvilinear relationships that would result from including a few highly urban départements (administrative units) in a predominantly rural sample, we selected only départements which were predominantly rural and agricultural during the 1856–1901 period. We arbitrarily defined a predominantly rural département as one in which more than 50 percent of the population lives in communes (townships) with less than 2,000 population in 1901. A predominantly agricultural département is one in which more than 40 percent of the male breadwinners were engaged in agriculture in 1901.⁵ We have also excluded those départements for which data were unavailable in one or more years. With these exclusions, 67 of the total 87 départements in late nineteenth century France were included. Since these 67 départements are spread throughout France, the pattern of selection should not introduce any unusual biases into the results.

Indices. Because fertility varies greatly from region to region within France and because number of children helps determine household size, we cannot use household size to indicate complexity. A region with mostly nuclear households and large numbers of children could appear the same as a region with stem families and fewer children. Hence, we have chosen two indices of complexity, both of which exclude children.

The first is the average number of adults per household (APH) or A/H , where A is the total adult population age 20 and over, and H the number of households. This index is similar to one used by Burch (1970:67), though he terms anyone over 15 adult; whereas we call adult only those age 20 or over.⁶ If everyone followed the nuclear residence rule and moved into a separate household at marriage, most families would consist of husband-wife pairs with children under age 20. Except for some variations, such as widowed and divorced parents living alone or children over 20 not yet married

living at home, the average number of adults per household should, then, be near 2.0. To the extent that children do not leave at marriage, but instead form stem, joint, or other complex households, the number of adults per household will rise above 2.0. Some households fail to follow exact residence rules—for example, some people never marry and others never have children—but an index based on exact rules should give an approximate estimate of complexity. In 1876, for example, the average adults per household ranged from 2.25 adults in the least complex département to 3.14 adults in the most complex. This difference of less than one adult may not seem large, but we don't expect it to be. Catching households at all points of the domestic life cycle, the index can only be an average of complexity at each stage of the life cycle. In some households destined to be stem or joint, but in which the parents have died and children are not yet old enough to marry, there are only two adults. In others, one brother has married but the other has not. In a few, both parents are living in the same household with their married son or sons. This mix makes an average difference of one adult per household worthy of consideration (see Burch 1970).

The second index of complexity is the number of marital units per household (MUH) or $(Mm + Wm + Wf)/H$ where Mm is the number of married males, Wm the number of widowed or divorced males, Wf the number of widowed or divorced females, and H the number of households. The number of married males indicates the number of husband-wife marital units in the population.⁷ Should everyone follow a strict nuclear residence rule, and should all adults marry, the following conditions would result: No married individual would live with anyone but his spouse and unmarried children. All widowed and divorced persons would live alone. The number of husband-wife pairs plus the number of widowed and divorced males and females would exactly equal the number of households—there would be no other households. And the index MUH would

⁵ As the general trend in France during the second half of the nineteenth century was toward urbanization and industrialization, it is safe to assume that départements which were still rural and agricultural in 1901 were so in 1856.

⁶ The 20 year adult age seems more appropriate for a population with an average female marriage age near 24 and a male marriage age near 29 in 1856 (Camp 1961:53).

⁷ As the numbers of married males and married females are the same, save for enumeration problems, one could be substituted for the other with little change in result.

equal 1.0.⁸ To the extent that marital units—married siblings or parents and married children—join in the same household, the total number of households (the denominator) will be reduced and the index will rise about 1.0.⁹ In 1876 the index ranged from .95 to 1.29 marital units per household. Again this range, though small, is significant (see Burch 1970, Table 2, $e_0 = 40$, $GRR = 1$ or 2 for similar ranges).

Having constructed these two measures, we are confronted by several questions: In the French census, were households defined in a useful and consistent manner? What of people who never marry, and, thus, fall outside the ideal nuclear, stem, and joint household patterns? How does the inclusion of never-married adults in aggregate measures bias the results? Finally, how does variation in marriage age from département to département influence the results?

First, because the census definition of household (*ménage*) includes non-kin such as servants and other permanent lodgers, it does not coincide exactly with LePlay's or our preferred definition of the household as a unit bound by birth and marriage. Domestic servants, however, do not seem numerous. The average proportion of servants in 1866 was only 5.7 percent, in 1876 6.5 percent, and 1901 2.5 percent. More important, the range among départements was sufficiently small not to bias the results seriously. For example, the largest percentage for any single département was 11.2 percent in 1866 and 4.1 percent in 1901.¹⁰ Finally, because only two-thirds of all servants were more than 20 years old, the number of adult servants who would increase the adults per

household and the marital units per household is even less than the preceding percentages (1891 Census, p. 432). The problem of servants in the household, then, seems negligible.

How are non-kin members of the household treated in the French census? Although it is unclear how persons in hotels and boarding houses were dealt with prior to 1901, in that year permanent residents of such places were counted as separate units while transients were counted as part of the owner's household (1901 Census, p. 64). Since our sample is predominantly rural départements, which should contain few overnight hotel guests, this factor is unlikely to change the results. Fortunately, persons living in such collective households as asylums, hospitals, military camps, boarding schools, poor houses, seminaries, and prisons, were recorded separately in all censuses. Persons living alone in their own dwelling units were included and constitute from 10 to 15 percent of all households between 1856 and 1901. A few of these people, widowed and divorced and living alone, follow a strict nuclear residence rule. The existence of such households appropriately reduces our measures. A few others, bachelors and spinsters, also reduce our measures—but more on these below. In sum, we deal with non-collective households consisting of a single person or family living together in a distinct residential unit (*appartement*). Happily, except for a few households composed solely of unmarried adults, the great majority of households appear to be family units.

From 1856 to 1901, about 10 percent of the total population never married. To the extent that this non-marrying population lived in isolated households, it unduly lowered both indexes MUH and APH. However, because bachelors in isolated households are added to the MUH denominator but not to the numerator, whereas they are added to both the APH numerator and denominator, such bachelors lower MUH more than APH. Conversely, because unmarried adults in a family household are excluded both from the numerator and denominator of MUH, but included in the APH numerator, additional unmarried adults living with parents or siblings will enlarge APH but leave MUH unaffected. Because

⁸ If everyone follows a strict nuclear residence rule, but some adults fail to marry and live alone, MUH will be below 1.0.

⁹ The index may also be conceived as the ratio of actual (P/H) to expected $(P/(Mn + Wm + Wf))$ household size under a nuclear model or as $(P/H)/(P/(Mm + Wm + Wf))$. To the extent that actual household size exceeds that predicted by the nuclear model, then this ratio rises above 1.0. Of course, the P in the last expression cancels out so that the expression is identical to the index MUH.

¹⁰ Data from 1866 census, p. II; 1876 Census, p. II; 1901 Census, v. 4, p. 175. Because in the later correlation analysis, the 1876 and 1901 household data produce almost identical results, it must be that the larger proportion of servants in 1876 introduces little bias.

the number of single adults over 20 is much larger than the number of single person households (the ratio was 1.6 single adults to each single person household in 1891), it must be that the latter alternative of continued residence with parents or siblings is the more common one. With either alternative, if there are more bachelors, spinsters, or a later marriage age in one département than in others, the index APH will be disproportionately large relative to MUH.¹¹ Nevertheless, since it is difficult to separate bachelors and since the discovery of regions in which bachelors continue to live with their families is interesting in itself, we have used the original measures with bachelors and spinsters included.

To evaluate the total effects of non-kin and unmarried adults in the household, it is best to turn to two checks on the validity of these measures. The first is a comparison with detailed data on household composition in 1962. The second is an 1856 comparison with the areas that LePlay, through detailed observation and historical study, designated

as nuclear, stem, and patriarchal family regions.

1962 households. For the first time in France, the 1962 census presents data not only on the number of households, but also on the number of secondary families, ascendants, descendants, and other kin in the household. It is, thus, possible to check whether our two indexes do indeed measure household complexity. In the census, primary and secondary families are distinguished by whether the family head is also the household head. A family is either (1) a couple and any unmarried children aged less than 25 or (2) one person with at least one unmarried child under 25. Hence, a family is composed of at least two persons. Other household members, considered "isolates" rather than parts of families, are classified according to their link to the household head. Ninety percent of these are adults aged 25 or over. "Ascendants" are single parents, grandparents, or great-grandparents. "Descendants" are unmarried children over 25, married children without spouse, or grandchildren without parent or spouse. "Other kin" are any kin outside the preceding two categories, most likely unmarried brothers. Table 1 shows secondary families, ascendants, descendants, and other kin per one hundred rural households.

¹¹ Indeed, the 1901 zero-order correlation of .60 between MUH and APH increases to .82 when the percentage of older bachelors is added in a multiple regression. Also, a later marriage age in the départements with many permanent bachelors and spinsters adds to this effect.

Table 1. Correlations for 1962 Measures of Household Complexity

	MUH	APH	APH (rural)	Household members outside primary family*				
				Secondary families	Ascend- ants	Descend- ants	Other kin	Friends
MUH								
APH	.87							
APH (rural)	.79	.96						
Secondary families	.92	.94	.93					
Ascendants	.92	.89	.87	.95				
Descendants	.22	.63	.72	.51	.40			
Other kin	.36	.72	.79	.61	.52	.94		
Friends	.23	.26	.25	.21	.22	.11	.30	
Mean (67 depts)	1.07	2.16	2.23	5.20	8.02	1.25	5.75	1.10
Standard deviation	.06	.12	.16	3.30	3.13	.34	2.42	.50

*These measures are the number of secondary families, ascendants, etc. per 100 rural households.

Source: 1962 French Census, Notes 2,11; Table D21.

In the 1856 to 1901 censuses, the measures MUH and APH were moderately well correlated with each other ($r \approx .62$). Table 1 shows that in 1962 the two measures were even more highly correlated ($r = .87$)¹² and that the rural APH is highly correlated with measures for the total population of each département (row 2). Second, Table 1 shows that both MUH and APH are highly correlated with the proportion of secondary families and ascendants (columns 1-3, rows 3, 4). Thus, both measures do capture this aspect of household complexity. In addition, the high correlation (.95) between secondary families and ascendants suggests that most secondary families occur in stem family situations with the secondary family usually the son and daughter-in-law of the parent family. This would occur as follows: Parents are considered ascendants only if the parent's spouse and unmarried children are absent. Otherwise, parents are considered part of a primary or secondary family. Quite probably, until one spouse dies, the father is considered the household head, the parent couple being the primary family, and a married child the secondary family. Or the father may almost always remain household head; the widowed mother may be counted as ascendant while her son is elevated to household head. In either event, if secondary families and ascendants are but different stages in the same stem family cycle, then they should appear in the same départements and should be highly correlated as they are in Table 1.

Third, Table 1 shows that APH is more highly correlated than MUH with descendants and other kin (columns 1-3, rows 5, 6). This, combined with data presented later, suggests that families with many adults per household are more likely to contain unmarried siblings and unmarried children over 25, or to occur in areas where more people never marry or marry late. The high correlation ($r = .94$) between other kin and descendants further demonstrates this.¹³

¹² The lower correlation in earlier years may be the result of more bachelors, servants, and lodgers, who tended to increase the size of APH more than MUH.

¹³ The averages at the bottom of Table 1 indicate that unmarried siblings (other kin) contribute more to the number of adults per household

More specifically, these households, high on APH and with many unmarried siblings and unmarried children, occur in areas that LePlay designated as "patriarchal" but actually consist of stem or nuclear families with unmarried brothers, sisters, and children attached. This, however, can be shown better by an analysis of household type by region.¹⁴

LePlay's Family Regions

Though life expectancies and marriage ages vary somewhat from place to place within France, the 1851 life table for the whole country (Keyfitz and Flieger 1968: 312) allows some estimate of the marital units and adults per household that would prevail in a département composed completely of nuclear, stem, or joint families. Using Coale's methods (Coale, 1965; Burch, 1970), and noting that MUH is equivalent to the ratio of observed family size to expected nuclear family size, for 1851, nuclear, stem, and joint family départements should have had 1.00, 1.23, and 1.44 marital units per household and 2.27, 2.79, and 3.25 adults per household.¹⁵ These 1851 figures along with the distribution of observed MUH and APH for 1856 appear in Figure 1. Each point in Figure 1 identifies the position of a département. The figure shows, first, that very few départements have many households more complex than stem. Only one département, Landes (highest on APH), is closer to an expected joint than stem size, but this is only for the APH figure and likely the result of other factors, soon to be discussed. At least by 1856, then,

than unmarried children (descendants). One might expect many descendants (unmarried children) in stem family regions, but if children either marry or leave the household this need not occur. Table 1 indicates that it does not occur.

¹⁴ The last row of Table 1 shows that although the number of "friends" in the household is slightly correlated with APH and MUH in 1962, it is insufficiently correlated and, on the average, not prevalent enough (next to last row) to be a major determinant of these two measures.

¹⁵ MUH is comparable to the ratios given in Burch's (1970) Table 2. The expected number of households is computed from the 1851 female life table assuming a marriage age of 25 and an average birth age of 30. Actual female marriage and birth ages are somewhat closer to 24 and 29 (Camp 1961:53; Keyfitz and Flieger 1968:303), but would alter the results only slightly.

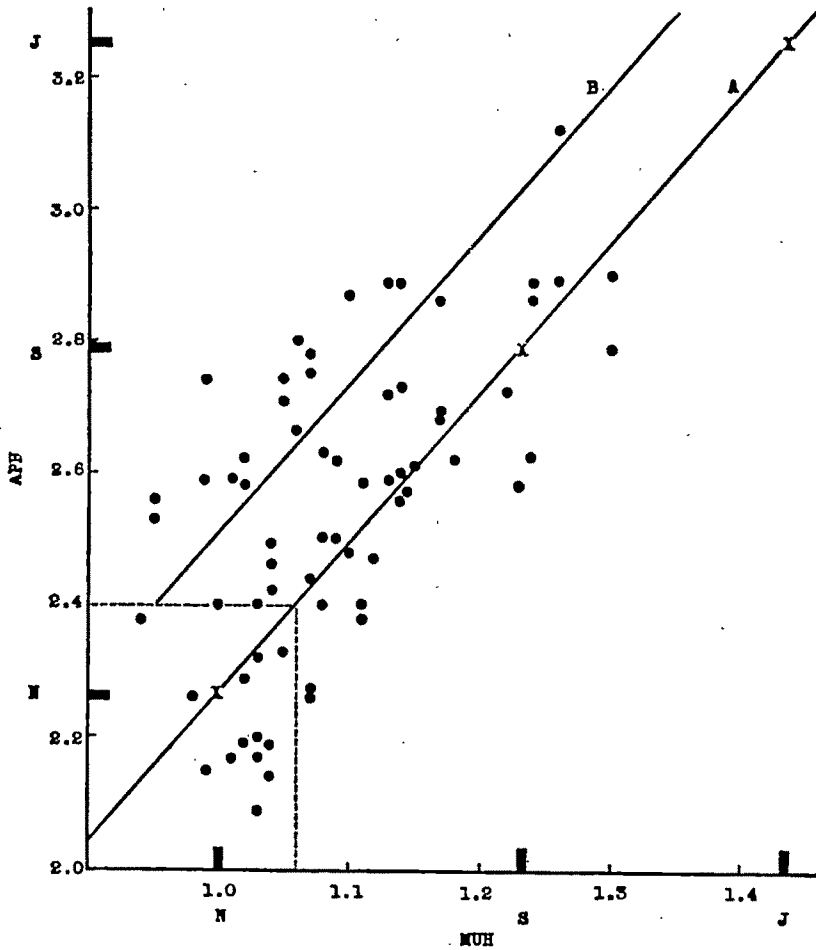


FIGURE 1. DISTRIBUTION OF DEPARTMENTS BY AVERAGE MUH AND APH.

Notes: Expected household values are indicated by N = nuclear, S = stem, and J = joint.

LePlay's patriarchal families were not true joint families.

Stem families apparently did exist in 1856, for Figure 1 shows départements scattered across the nuclear and stem ranges on both measures of household complexity. Since there is no distinctive gap between the nuclear and stem family départements, the selection of a dividing point must be arbitrary. Those départements which lie approximately halfway between the full stem value and the completely nuclear value tend to be départements situated geographically between the nuclear northeast and stem southwest of France. They must, then, include both types of families. To achieve a good correspondence with LePlay's categories however, we have chosen 1.06 MUH

and 2.40 APH as the division (dotted line) between nuclear and stem families. Those départements above 1.06 MUH and 2.40 APH might be termed simply stem, but they are a less than homogenous group. Some départements have many more adults per household than would be expected from their marital units per household. Line A of Figure 1 indicates the expected APH for each MUH.¹⁶ Were there little variation in marriage age or in the proportion of unmarried adults in a population, APH would be a constant proportion of MUH and all départements would cluster closely about line

¹⁶ It runs through the three intersection points of the expected nuclear, stem, and joint values or through points (1.00, 2.27), (1.23, 2.79), and (1.44, 3.25).

A. However, as already indicated, when there are many bachelors and late marriages, the adults per household figure will be larger than the marital units per household, and the points of Figure 1 will rise above line A. This is exactly what occurs. Line B, a line 0.24 points above line A, separates our départements in the Massif Central and Brittany which LePlay generally labelled patriarchal. These patriarchal départements are not characterized by households in which parents and married children or married brothers live together. Instead the households are nuclear or stem families with co-resident unmarried brothers, sisters, and adult children. Landes, largest on APH and large on MUH, probably includes mostly stem families and a few large patriarchal families with unmarried kin attached.

As of 1856, then, there were distinguishable family types in different parts of France, but they were not exactly as LePlay described them. The patriarchal families may have had married brothers living together with the father around 1800 or before, but more likely LePlay mistook large households with several unmarried siblings for ideal joint families. LePlay at no place gives a complete listing of all regions by family type. Moreover, his discussion concerns provinces, the census units which were replaced by départements with the Revolution. Nevertheless, from scattered references in *L'Organisation de la Famille* (1870:22, 24, 27, 31, 38, 50, 71, 73, 86) we have derived the list of 38 départements by family type. He distinguishes nuclear family regions quite clearly, but stem and joint regions less clearly. All the *non*-nuclear regions are at one point or another referred to as stem family regions. The patriarchal family regions are then separately referred to as patriarchal regions (1870:27). It may be, then, that LePlay saw a combination of stem and patriarchal families in certain regions; and, as a consequence, we cannot expect sharp distinctions between stem and patriarchal départements. All eight of his nuclear départements are nuclear by our measures. But almost half of his twenty-one stem départements are either nuclear (three départements) or patriarchal (seven départements) and slightly over half of his nine

patriarchal départements are stem (five départements) by our measures. Either because LePlay found stem and patriarchal households regionally intermixed or because he mistook large households for patriarchal households, his stem-patriarchal distinctions and curs correspond only poorly. Still, the distinction between nuclear and more complex households is quite accurate.

Overall, the fit with 1962 data as well as the less perfect fit with LePlay's designations convinces us that though they do not allow sharp distinctions between stem and patriarchal families, the measures are valid indicators of complexity. In the following discussion, we thus continue to use both measures, noting only that MUH indicates somewhat more the presence of stem families; whereas APH indicates somewhat more the presence of patriarchal families—that is, families with unmarried siblings and unmarried adult children.

ANTECEDENTS OF HOUSEHOLD COMPLEXITY

LePlay, Bloch, and others point to landholding, rural residence, dispersed habitat, and illiteracy as major features keeping complex households together. These features can be checked by a cross-section analysis of households in 1856 and 1876 France.

Landholding. To the extent that stem and joint families result from the attempt to keep land through two or more generations, they should be associated with the ownership and long-term control of land. In France, they should be specifically associated with land ownership and share cropping. Land ownership may be weakly associated with household complexity, for the census tends to place small holders in the more prestigious category of land owner even when they spend most of their time working tenanted land or working for other farmers.¹⁷ Such small owners will have insufficient land for a son or brother to farm jointly. Sharecropping should promote complex households because leases in sharecropping tended to be long term, often descending from father

¹⁷ Auge-Laribe (1912:132) indicates that 34 percent of those recorded as land owners in 1882 were in fact primarily hired hands. LePlay (1870:105) called them "indigent property owners."

to son. For example, for the Landes region, D'Artiques (1881) mentions fifteen-year leases which were automatically renewed even across generations unless major problems occurred. Similarly, Marsac (1903:84) speaks of fourth generation leases in the Limousin region. Second, sharecroppers tend to grow several crops at once; while cashcroppers, attempting to turn a large profit each year, tend to grow a single, highly saleable crop. Cashcroppers, therefore, suffer more from market instability and in a bad year are more likely to go into debt and lose their leases. Such conditions make cashcropping uncondusive to large households extending over two or more generations. Similar comments apply to agricultural laborers who own little or no land.

The relevant 1856 census data are somewhat weak because of poor classification criteria and because of large numbers simply omitted from the occupation classifications.¹⁸ Nevertheless, the data do provide an initial test of the effect of landowning and long-term tenure. Table 2 shows that some kinds of landholding are related to one measure of household complexity and not the other. Nevertheless, to the extent that kinds of landholding are related, they are related as

¹⁸ See the 1856 Census, p. xxxviii. Also, percent agriculturists correlates less with marital units per household in 1856 ($r=.23$) than in 1876 ($r=.35$) or any later year.

expected: Owners and sharecroppers are more likely than cashcroppers and laborers to live in stem and joint households.

In later census years, the number of sharecroppers and cashcroppers were combined so that it is impossible to examine the detailed effects of landholding and lease tenure. However, the proportion of owners and sharecroppers among agriculturists is sufficient to create a positive correlation between household complexity and the number of agriculturists in the population.

Other Antecedents

Agricultural labor, dispersed farmsteads, rural residence, and illiteracy purportedly promote non-nuclear households. Table 3 assesses the role of each of these factors through a multiple regression analysis. It shows, first, that, as predicted, agriculture and illiteracy lead to more complex households. Second, the proportion of rural dwellers is poorly measured because the distinction between rural and urban is only that between communes (townships) with more than 2,000 population. Nevertheless, the consistent negative relation to household complexity suggests that residence in at least small towns contributes to the formation of complex households. Indeed, though the explanation is ad hoc, it may be that the slightly more prosperous agriculturists, who can support large households, prefer life in

Table 2. Type of land holding and household complexity, 1856.

Dependent variable	Standardized regression coefficients for independent variables					Percent variance explained (R ²)
	Type of land holding					
	Owner	Share- cropper	Cash- cropper	Laborer	Illiteracy	
MUH		.25	-.40		.49	.63
APH	.24	.35		-.21	.37	.50

*Blank cells indicate a regression coefficient that, if included, would be between .10 and -.10 and, for a sample, statistically insignificant. The land holding variables are the percent of owners, sharecroppers, cashcroppers, and agricultural laborers among all employed males. Illiteracy is the percent of illiterates among all males obtaining marriage certificates in 1856. Though indicating less effect for literacy, unstandardized regression coefficients rank the land holding variables similarly. Zero order correlations and standard deviations appear in Appendix A.

Table 3 Antecedents of household complexity, 1876.

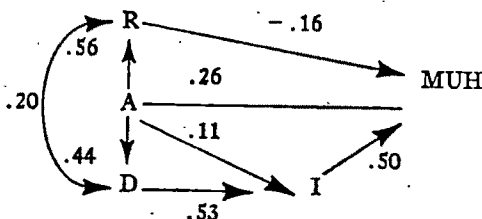
Dependent variable	Standardized regression coefficients for independent variables*			Percent variance explained (R ²)
	Agriculturists	Illiterates	Rural dwellers	
MUH	.26	.50	-.16	.38
APH	.47	.43	-.17	.47

*"Agriculturists" is the percentage of employed males in agriculture. "Illiterates" is the percentage males applying for marriage licenses in 1876 who are unable to sign their names. "Rural dwellers" is the percentage of the total département population living in communes with less than 2000 population.

town or that the better agricultural regions which support large families also support large populations. In any event, the average effect of rural residence is considerably less than the average effects of agriculture and illiteracy and, given the nature of ecological correlation, perhaps not worthy of much consideration.

Finally, dispersed settlement has little or no direct effect on complexity and is, hence, deleted from Table 3.¹⁹ Highly dispersed living apparently makes it more difficult to send children to school, which promotes greater illiteracy, and, hence, indirectly promotes complex households. However, Bloch was referring to the direct effect of dispersed living on family solidarity. We must con-

¹⁹ The addition of dispersion would add less than .01 to the total variance explained while the standardized regression coefficient for dispersion would be near zero (.01 to .04). Its indirect effect on marital units per household through illiteracy appears in the following path model including Rural population, Agriculturists, Dispersion, and Illiteracy:



clude, then, the supports offered by family solidarity were insufficient to keep people in common households in the nineteenth century.

Nevertheless, a few puzzles remain. In the earlier centuries of which Bloch was speaking, there was little literacy anywhere. Hence, dispersion of settlement could not have had an indirect effect through literacy. Could it be then that literacy really measures links to the wider economic and social system and that sons and brothers who have these links are more likely to leave for outside opportunities? If so, dispersed settlements being further away from communication, transportation, and market centers might have been spuriously associated with complexity even before the spread of literacy. Moreover, access to outside economic opportunity explains enough to enable us to dispense with Bloch's settlement explanation.

Agricultural Households

Though the preceding discussion indicates that agricultural and illiterate households are more complex than others, it relies on indirect evidence provided by ecological correlation. Not until 1962 do we have direct evidence on agricultural households. The 1962 data indicate two things. First, as late as 1962 agriculturists generally had more adults per household—nationwide the average for agriculturists was 2.75 adults per household; whereas the average for the total population was 2.16.²⁰ Second, there is variation among agriculturists. The départements where they continue to have complex households are predominantly those which LePlay designated as stem or patriarchal—among agriculturists the average adults per household in nuclear départements in 1962 was 2.55, in stem départements 2.82, and in patriarchal départements 2.79.²¹

It appears, then, that in 1962, as in the nineteenth century, agriculture tends to pro-

²⁰ These are département averages giving equal weight to each département.

²¹ Though the patriarchal départements had fewer adults per household than stem départements in 1962, they had more in 1856—2.69 versus 2.61 in the total agricultural and non-agricultural population:

mote complex households. Much of the decline in household complexity between the nineteenth century and the present must have resulted from the decline of those engaged in agriculture. Moreover, within agriculture there may have been little change. The number of adults per household in 1962 in agricultural households is larger than that for the total 1856 population (2.76 versus 2.55) and possibly almost as large as that for the agricultural population in 1856—for which we have no data.²² Two explanations are possible: either it takes a long time for the agricultural population to adopt industrial family values or agricultural life offers enough inducements to continue the complex household tradition despite the knowledge that the rest of the society has abandoned it.

However, agricultural occupations cannot explain all the variation in 1962 household complexity, for some agricultural households are more complex than others. It must be, then, that LePlay and Bloch were correct in emphasizing the traditions of certain regions. Or else these complex household départements are unique in ways that we have not yet measured.

In any event, this whole section on the antecedents of different types of families shows that complex family households continue to be promoted by (1) land ownership or long-term land tenantry, (2) agriculture in general, and (3) the heritage of particular regions.

CONSEQUENCES OF COMPLEX HOUSEHOLDS

We now want to examine LePlay's and Durkheim's suggestions that complex households influence fertility, migration, social integration, personal security, and "traditionalism."

Fertility

LePlay complains that the new laws of equal inheritance cause stem families to

²² Because of changes in the age structure, we would expect more adults per household in 1962 even if everything else remained equal. The 1962 agricultural population and 1856 total population figures are about equally close to the expected number of adults in stem households at the two dates (93 and 91 percent of the expected number).

limit births in the fear that an extra son would split the family's property. Most contemporary analysts, however, date the decline of births from before the Revolution and before the new inheritance laws (Camp 1961:100). However, once the percentage of agriculturists and illiterates is controlled for 1876, there is a negative relation between the marital units per household and fertility ($\beta = -.29$).²³ To some extent, then LePlay may have been right. That stem households have fewer instead of the same number of children as nuclear families may be more perplexing. Perhaps if family property is not enough for subsistence—and indeed in nuclear regions many own minuscule farms—the concern with division of property hardly exists. Or, if the land is so small that extra children will automatically work away from the farm, and if these children will occasionally send back money, more children might provide more support in old age. In contrast, patriarchal families continue to have relatively high fertility. Once agriculture and illiteracy are controlled, there is a positive relation between adults per household and fertility ($\beta = .36$). With the knowledge that second and third sons will tend to remain in the household as unmarried laborers even when grown up, patriarchal families may find extra sons more advantageous than disadvantageous.²⁴

Not only are the reasons for fertility limitation by type of family just somewhat plausible, but the means of limitation are not those usually expected. Stem households limit fertility not by delaying marriage, as was the Irish pattern, but by other means.

²³ The fertility measure is the crude index of number of births per total population in 1876. However, maps of a better fertility measure, births per 1,000 married women aged 15–69, suggest similar results. (See Camp 1961:185).

²⁴ Also a higher birth rate can, to a degree, create more adults per household regardless of the specific motivations involved. As long as unmarried children remain within the family and as long as the average marriage age is higher than the age of adulthood (here age 20), a higher birth rate will eventually lead to more persons in the household between age 20 and the marriage age. Hence, a higher birth rate can be a cause in addition to just an effect of more adults per household. We can not tell exactly which process was in effect in 1876.

(They marry just as early as the nuclear households.) Though the patriarchal households delay marriage and, accordingly, should have lower fertility, they in fact have higher fertility. This suggests that by the latter half of the nineteenth century alternative methods of birth control were widespread and could be used either to preserve land intact or increase the number of family laborers with little regard to the age of marriage.

Migration

Since stem départements have fewer births, one of the conditions for high out-migration is missing. With only about two children per family in these regions, there will be few excess children to be dispatched to outside jobs.²⁵ Still, the slight excess population in patriarchal regions could leave for urban jobs. LePlay and Habakkuk suggested that they would not leave, or if they left temporarily during times of rural hardship, they would return once these hardships had passed. Nuclear family migration should be similarly sporadic. Only stem families could promote the steady, long term migration essential for a modern, urban work force.

An ideal test for the effect of stem and patriarchal households on migration would examine out-migrants and households in rural communes alone. We can only approximate such a test; but taken together our four tests imply that, in late nineteenth century France, although stem households had little effect on migration, patriarchal households did inhibit long term migration.

The 1876 census presents two measures of short term migration: (1) the proportion of people who left minus those who entered rural communes between 1872 and 1876, and (2) the proportion of people who left each département minus those who entered

between 1872 and 1876. The 1881 and 1891 censuses present two measures of long term migration: (1) the 1891 census gives the total number of people who were born in one département but currently live in some other. As a percentage of all people born in a département, this number gives the proportion who have left, and who continue to live outside their native département. These inter-département migrants might be attracted to nearby cities, particularly to Paris close by nuclear family départements in the North. We therefore should obtain a measure which controls for the pull of cities outside the département. (2) The 1881 proportion of persons migrating between communes *within* the same département comes close to being such a measure. Again, it is the proportion who have migrated sometime during their lifetime, and who continue to remain outside their native commune.

Though fertility is not a strong determinant of migration, agriculture and illiteracy are. Including agriculture and illiteracy in regressions with household complexity, then, leads to the standardized regression coefficients in Table 4. Short-term, net migration is irregularly related to measures of household complexity while long term migration is related only to the adults per household. As LePlay suggests, stem households may inhibit short term flights from the land. From 1872 to 1876, the stem family départements lost fewer migrants than the nuclear départements. In the long run, though, and under the slow population growth conditions of nineteenth century France, stem families failed to promote the long term migration which they are supposed to. Stem family départements have no more lifetime out-migrants than nuclear family départements. Patriarchal households may have been associated with a small spurt in migration between 1872 and 1876, but in the long run they have inhibited migration—either by keeping members within the household or by giving them a place to return once they had tried migration to the city.²⁶

²⁵ With only two children per family, and a one-half chance that each birth will be a boy, about one-half the families will have one son, one-fourth two sons, and one-fourth no sons. Since the last two proportions are equal, most excess sons should be able to find land on a nearby farm where there is no son, and there should be little need for inter-commune or inter-département migration.

²⁶ The careful reader will note that though we speak here of migration as a result of household

Table 4. Standardized regression coefficients for household complexity and migration.

	Short term migrants		Long term migrants	
	Net rural out-migrants 1872-1876	Net départemental out-migrants 1872-1876	Lifetime département out-migrants 1891	Lifetime within département migrants 1881
MUH	-.16	-.28	-.07	-.08
APH	.27	-.03	-.32	-.46

*From regression equations containing agriculture, illiteracy, and household complexity as mutual predictors and migration as the dependent variable. Appendix B shows the basic correlations for the table. MUH and APH are for 1876.

Sources: 1876 Census, table 8; 1881 Census, table 3; 1891 Census, pp. 132-33.

This analysis only begins a thorough migration analysis. There are other measures of migration and other features of départements, such as the development of transportation networks, to be considered. However, we can hazard some generalizations. When the rate of population growth is slow or when, à la LePlay, stem families limit births to avoid land division, they may inhibit some short term flights from the land, but they will have little effect on the long term flow of workers into urban industry. Similarly, nuclear families will not produce any more long term migration than stem families. In contrast, even with their slightly higher birth rates, patriarchal families will limit long term migration and may withhold potential workers from urban industry.

Suicide. In *Suicide*, Durkheim argues that a large household is more strongly integrated because, by bringing many people—adults or children—into frequent interaction, it strengthens the “collective sentiments” of its members. Indeed, he shows that household size and the suicide rate are negatively correlated (1951:198-202). Durkheim’s mistake, however, is his use of a demographic rather than sociological definition of persons. After the seventeenth century, the conception of children changed drastically. No longer regarded as small adults who ate at

the same table, participated in most grown-up activities, and had adult personalities, they acquired a separate status which entailed lengthy schooling and special training (Ariès, 1962). Fortunately, our measures allow us to distinguish between the effects of adults and those of children on suicide.

Our suicide rate is the average for 1878, 1879, and 1880 (among the same years Durkheim examined). As in Durkheim’s original data, household size is strongly related to suicide ($r = -.66$). The number of adults per household is at least as strongly related ($r = -.70$). When we control for the number of adults per household, however, the birth rate is unrelated to suicide (partial $r = .03$). Therefore, in places and times similar to nineteenth century France, adults, but not children, contribute to the integration of household units.

However, we have not solved the whole problem. The number of adults per household can either be considered to have a significant effect in itself (following Durkheim) or to be an index of another variable, household tradition. An adherent of LePlay’s views would say that it is not the size of the household that creates integration; it is the tradition of stem and patriarchal families, with its paternal authority, its moral code, and its capacity to fight successfully the “original evil” which every new generation brings with it. We have no measure of these cultural factors, but we can at least test for the impact of parents—the carriers of tradition—on suicide.

complexity, we earlier suggested illiteracy increased household complexity by restricting out-migration. Migration is, thereby, both cause and effect. It is frequently so mentioned in the modernization literature, and we have simply deferred detailed analysis of this problem.

As previously shown, index MUH is slightly more highly correlated with the presence of old parents in the household, while index APH is more correlated with the presence of other kinsmen, likely to be unmarried siblings. Hence, index MUH should measure more closely the importance of lineal tradition while index APH is closer to measuring sheer size. Since the correlation of index APH with suicide is much higher than that of index MUH ($-.70$ vs. $-.36$), we conclude, with a modification Durkheim's view, that sheer number of adult persons contribute most to household integration and to lower suicide rates.²⁷

Divorce

Just as more adults per household lead to less suicide, we might extrapolate further from Durkheim that greater household integration would lead to less divorce. Similarly, from LePlay's view of stem and patriarchal families as being traditional, we might infer that more complex families have less marital disruption. But we know today that, in simple societies, kin solidarities beyond the nuclear family (e.g., strong lineages) may actually promote divorce (Ackerman, 1963).

Table 5 presents a mixed picture. Once agriculture and literacy are controlled, households with more marital units appear more prone to divorce whereas households with more adults are less so. We are reluctant to push ad hoc explanations too far, but several interpretations are possible. First, following Durkheim, the integration provided by adults interacting in a household could restrain divorce. Even families with many marital units may not have sufficient adults for great interaction and integration. However, because départements with many adults per household are ones with later marriage ages and more adults not marrying, it could

Table 5. Divorce and household complexity, 1901.*

Dependent variable	Standardized regression coefficients for independent variables			Percent variance explained (R^2)
	Agriculturists	Illiteracy	Household complexity	
Divorce	-.69	-.33	.23 (MUH)	.62
Divorce	-.49	-.13	-.31 (APH)	.63

*The divorce rate is the number of divorces in 1901, 1902, and 1903 divided by the number of married males in each département.

be that the more careful selection of a spouse at a later age and the economic costs involved in marrying inhibit divorce in these départements. Also, tensions between mother and daughter-in-law might promote divorce in those households likely to contain the husband's parents, namely those with several marital units. By contrast, the wife's position might be enhanced, in families with other adults, by the presence of the husband's unmarried siblings. With no children of their own to threaten the family's resources and with little authority vis-a-vis the wife but with adult labor to add to the family's income, the wife's position might be more secure.²⁸ The evidence contradicts LePlay; once we control for the effects of agriculture and illiteracy, stem families (indexed by MUH) actually promote rather than inhibit divorce.

Religion

LePlay finds the nuclear family particularly anti-tradition in the field of religion. During the last part of the nineteenth century, one of the most important arenas of battle between the secular and the Church was the school system (Thabault, 1971: 192). Thus, the proportion of primary school students enrolled in religious schools

²⁷ Suicide is not spuriously correlated through its association with the urban sector, for controlling agriculture and illiteracy has relatively little effect on the correlations. For example, with agriculture included in a multiple regression, the MUH standardized regression coefficient is only reduced to $-.19$ and the APH coefficient to $-.56$.

²⁸ Compare discussions of intra-family tensions and divorce in Chinese families (Freedman, 1966) and in Irish families (Arensberg and Kimball, 1940).

during 1875-76 should constitute an acceptable measure of the strength of religion. Its correlation with household complexity is both low and contradictory. With marital units per household, it correlates $-.19$; whereas, with adults per household it correlates $.19$. Other variables such as agriculture and illiteracy do not confound the relationship. Stem families are slightly less religious than nuclear families; whereas, patriarchal families are slightly more religious.

On ceremonial questions such as the proper time for marriage, the stem family regions probably are, as LePlay suggests, slightly more orthodox than the nuclear family regions (see Camp, 1961:157-59). But on practical questions of schooling and politics they are less religious than either the nuclear or patriarchal family regions. This may be an artifact of the historical legacy which leads to greater anti-clericalism in many départements where stem families predominate. For example, Labrousse's (1946) map of 1848 voting patterns shows widespread leftism in many of the stem family départements of southern France. This was French Revolution style leftism (Gauch Montagnarde) which is almost invariably associated with anti-clericalism. Indeed, by arguing that anti-clericalism was a strong *tradition* in southern France, one might argue from the marital units figure that stem families were more "traditional" and that LePlay was correct. But, this would not warm his conservative soul.

Old people

In only one area are LePlay's arguments about the consequences of stem and patriarchal families strongly confirmed. Both are more likely than nuclear families to care for parents in old age. The data are for 1962, but the proportion of those over 75 in old people's homes should indicate the extent to which children aid their parents either by giving them sufficient money to live alone or by bringing them into their own homes.²⁹ In 1962, there were more resi-

dents of old people's homes from nuclear families. The proportion has a moderately high negative correlation with both marital units per household ($-.61$) and adults per household ($-.53$). Nowhere is the proportion of the elderly in old people's homes very high. In one département it reaches 14 percent, but elsewhere it is usually 6 to 9 percent. Moreover, the relationship is almost tautologically true from the definitions of nuclear and complex household. Still, LePlay's assertion that nuclear households would neglect parents is somewhat confirmed for 1962.

Overall, then, except for the care of aged parents and possibly the restraint on births, LePlay's image of the stem family is not confirmed. Under the conditions of late nineteenth century France, stem families reduced births, reduced short term but did not promote long term migration, and were not especially cohesive and traditional. They only slightly reduced suicide, promoted divorce, and were politically irreligious. Patriarchal families conformed much more closely to LePlay and Durkheim's image of them, though often not for the reasons they proposed. Patriarchal families do promote births, retard long term migration, reduce suicide and divorce, are more religious, and protect aged parents. They provide the conservative ideal of stability and security, but retard the movement of sons to more profitable employment. By contrast, the orderly change or individual security which LePlay desired and expected from stem families is hardly to be found.

SUMMARY

This paper makes four major contributions. First, it refines measures of household complexity which may be used with a wide

chronically ill as well as in old people's homes. The proportion in old people's homes could be increased by a lack of children to support them and by the wealth of the département which can be used to support old people's homes from taxes. However, neither the birth rate in 1901, which should index the number of adult children present to support old people, nor wealth of the département (Goguel 1970:177) are related to present proportions in collective households ($r=.09$).

²⁹ The data are actually the percentage over age 75 in collective households which includes a few in prisons, monasteries, and hospitals for the

variety of data including censuses. Moreover, it validates these measures by showing that they correspond to more detailed results from the 1962 French census and by showing that they correspond in part to LePlay's distribution of household types.

Where the distributions do not correspond, plausible explanations are available. First, LePlay only vaguely distinguished between stem and patriarchal regions and, second, he was probably misled into seeing an ideal joint family in the large families of nineteenth century France that included several unmarried siblings and adult children. These measures are valuable because they can be used to extend the study of households into areas where the data previously seemed unsuitable—and particularly, into the nineteenth century where census data do exist.⁸⁰ For the nineteenth century, they can relieve us of the tedium involved in reconstructing households from manuscript censuses or parish registers. And, since more areas can be covered, we can appreciate the differences among regions even within a single society.

Our second contribution is to better describe the nineteenth century baseline which existed at the beginning of industrialization. The paper reaffirms that nuclear families existed over most of northern France, if not most of Europe, before industrialization. Stem families existed in the southwest of France and a pseudo-joint or patriarchal family with unmarried siblings and adult children was present in other areas. But, at least by the 1850's, in no place were there many ideal joint families with several married sons and a father living together.

The third contribution is to specify further what maintains household complexity and, by implication, what will lead to household change during industrialization. Landholding or long term leasing of land, agriculture in general, and illiteracy promoted complex households in France. Agriculture and landholding may simply create the desire to keep land within the family as well as to give the father economic power over his son so that he can keep him at home. Illiteracy may

deny sons formal knowledge that could offset the superior experience of their fathers; thus the sons remain subservient. Or, sons in illiterate départements may not be attracted to urban centers or even other rural areas because of their isolation from labor markets and news of outside opportunities. More surprising, although the decline in agriculture has led to fewer stem and patriarchal families in the total society, among agriculturists the three family traditions have proven relatively long lasting in their original areas of concentration. As late as 1962, regional differences of the nineteenth century persisted, even if on a muted scale. In spite of modern communications and transportation, industrialization does not expunge the family traditions of those who remain in agriculture, but only the traditions of those who move to cities and engage in modern work. When literacy is almost universal, it is unclear why the differences among agricultural households should persist. Evidently the three separate regions maintain a heritage which is yet untapped by common explanations of household change.

Fourth, we have specified some of the consequences of stem and patriarchal families. Perhaps because stem families feared the new inheritance laws or because of a tradition which preceded the Revolutionary laws, stem families reduce fertility. Patriarchal families, possibly because of the knowledge that most grown sons will remain on the farm as unmarried laborers, tend to be more fertile. However, neither family type has many children and the stem families marry earlier than the patriarchal. Therefore, knowledge of birth control must have been widespread by the last half of the nineteenth century.

The migration data suggest that, when fertility is low, stem families may inhibit short term flights from the land; but they do not promote a long term flow of rural migrants into urban industry—at least not relative to nuclear families.⁸¹ When popula-

⁸⁰ The major obstacle in most censuses will be to determine the degree to which non-family members such as servants and lodgers as well as non-families such as army barracks, asylums and prisons were included in the "household" category.

⁸¹ Since the population growth rates in pre-1850 England and late nineteenth century Japan were also low, we should reexamine the claims that in these societies stem families and indivisible inheritance provided an abundant and committed labor force (see Habakkuk, 1955, Vogel, 1967).

tion growth is high, stem families may more readily dispatch extra members to urban centers, but they may also move towards the French patriarchal form whereby unmarried sons and daughters simply stay on as unmarried family workers. If so, like joint families, they would retard rather than promote the steady flow of laborers to modern industry or the growth of a skilled, committed labor force. One would see, rather, people moving back and forth between rural home and urban work as the seasons and fluctuating markets dictate (see Gonzales, 1961).

Patriarchal families are more traditional than stem or nuclear families in that they are more religious. But it is not just greater religiosity or traditionalism that makes them less subject to suicide and divorce. Rather, it seems that by bringing more adults into constant interaction, the patriarchal family either fixes the individual so firmly in the responsibilities and ways of thinking of a primary group or gives him so little time for individual brooding that he has little inclination to commit suicide. In the nineteenth century, interaction with children was insufficient to prevent suicide because they were not sociologically persons; it remains to be seen whether in the twentieth century they provide sufficient meaning and responsibilities to inhibit adult suicide.

Both stem and patriarchal families might seem less likely to divorce; but surprisingly, stem families have even more divorce than nuclear families. We lack detailed data, but possibly friction arises in stem families between mother-in-law and daughter-in-law; whereas, patriarchal families, with late marriage and fewer surviving mothers-in-law, lead to a comfortable existence for husband and wife amidst unmarried siblings who provide labor but consume relatively little. A change in household type, then, will have widespread effects ranging from questions of personal security to the provisions of a modern labor force. Should we extend the analysis to look at other differences among families such as child socialization, the consequences might be even more wide ranging.

In sum, this paper provides refined measures for historical demography and demonstrates the usefulness of these measures for

understanding the causes and consequences of household complexity in one developing society.

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Appendix A. Correlations of 1856 measures used in table 2.*

	MUH (1)	APH (2)	O (3)	S-C (4)	C-C (5)	L (6)	I (7)	$\bar{X}$	S.D.
(1) MUH								1.09	0.0856
(2) APH	616							2.55	0.2307
(3) Owner	117	208						32.75	15.9471
(4) Share-cropper	627	558	-188					5.92	7.8885
(5) Cash-cropper	-395	-104	-537	-166				7.94	7.5929
(6) Laborer	-070	-351	-540	-064	017			20.94	9.8389
(7) Illiteracy	634	532	-230	662	069	044		35.83	18.0784
(8) Agriculture	303	332	507	335	-204	095	237	67.41	10.0450

*The decimal point before each correlation coefficient has been deleted.

Appendix B. Correlations of measures used in tables 3 through 5.*

Year	Measure	1876		1901		1876				1901				RM	DM	OM	M	S	RS	BR	$\bar{x}$	S.D.
		MNH	APH	MNH	APH	A	I	R	D	A	I	R	D									
(1) 1876	MNH																				1.10	0.0868
(2) 1876	APH	656																			2.65	0.2232
(3) 1901	MNH	928	517																		1.08	0.0823
(4) 1901	APH	640	881	604																	2.42	0.1977
(5) 1876	Agricul- turalists	347	520	340	602																61.84	9.9805
(6) 1876	Illit- erates	579	574	543	465	351															21.35	14.0974
(7) 1876	Rural dwellers	032	126	065	223	557	081														79.19	8.2960
(8) 1876	Disper- sion	384	440	351	356	446	584	199													56.52	21.9254
(9) 1901	Agricul- turalists	461	845	457	609	890	434	614	511												58.54	8.3867
(10) 1901	Illit- erates	618	570	595	508	387	932	150	625	496											16.60	8.0790
(11) 1901	Rural dwellers	134	196	159	288	630	171	961	281	709	243										74.49	9.2579
(12) 1901	Disper- sion	417	491	411	441	585	601	452	918	691	665	529									45.98	18.1699
(13) 1872-76	Rural migrants	-018	249	-065	181	112	126	027	004	078	139										15.57	15.4803
(14) 1872-76	Dept. migrants	034	263	071	268	454	263	384	358	466	298			423							0.34	1.3550
(15) 1891	Out- migrants	-259	-353	-243	-210	034	-422	-329	-248	-001	-319		-148	097	236						17.12	4.7500
(16) 1881	Migrants	401	-305	-126	-535	-551	-112	-166		-477	-174			-306	-326	-034					27.56	7.7827
(17) 1878-80	Suicide	-363	-699	-282	-621	-557	-387	-264	-442	-587	-410	-537	-518								1.14	0.7814
(18) 1902-4	Di- vorces	-341	-712	-278	-674	-725	-528		-454	-745	-364		-584			169					2.90	0.5616
(19) 1876	House- hold size	448	894	305	769	571		249		534	591	306						-659			3.79	0.4348
(20) 1876	Birth rate	128	558	013	437	412	469	122	220	306	453	148		246	155	-164	-378	-370	764		2.47	0.4050
(21) 1876	Church schools	-190	194	-247	010	037	148	-115	460	062	184	-073		029	214	-237	-133	-252	216	205	42.29	11.8079

*The decimal point before each correlation coefficient has been deleted.

**This is the natural log of the divorce rate.

THE PROCESS OF ENTRY INTO FIRST MARRIAGE *

GUDMUND HERNES

University of Bergen

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A mathematical model is developed for the process of entry into first marriage. The members of a cohort are seen as subject to two types of forces. One is the increase in social pressure to get married as the percentage of the cohort already married increases. The other is the declining marriageability of members of the cohort as they grow older. With some additional assumptions, this gives rise to a non-homogenous diffusion model. The model is illustrated with U.S. Census data, and factors contributing to deviation between observed and predicted distributions are discussed. Other applications of the diffusion model are suggested.

It is an every day observation that the percentage of a cohort that marries increases with age. It is also well known that the marriage rate is slow during the first marriageable years. It accelerates in the late teens and declines again by the

mid-twenties. This paper develops a model for the process of entry into first marriage of a cohort's members as they age.

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THE MODEL

Let us first consider intuitively what forces may affect a cohort's members, and how these forces change with time. In general they are of two kinds. One is the social pressure to marry: we all know this pressure increases with the increase in the percent of a cohort already married. The other is the capacity of a cohort's members to marry as they age; here we believe that marriageability declines with age. Let us discuss these two forces in turn.

Single people experience the social pressure to marry in various ways as the percentage already married increases. Much social interaction is age-graded. As a larger percent of the cohort marries, single people will have reduced interaction with old friends. They will be invited less often to parties, dinners or trips, partly because of the awkwardness of diverging interests, partly because of the threats to the established couples. When singles are invited, hosts often play the role of matchmakers. Thus the psychological experience of the undesirability of celibacy increases as more of the same age group enter into wedlock. With increasing age the psychological experience of being unmarried as a kind of deviance is heightened by popular culture. For example, as a woman remains in the pool of the not yet married, her social definition changes from maiden to spinster or even reject. In the process, she may pass through the panic-age, trying harder than ever to catch a husband. In her social environment people will say, "what she needs is a man," and perhaps she will be pitied for lacking marital bliss. Men who do not marry may have their masculinity questioned, and even be suspected of homosexuality. In newspapers the unwed are reminded of their state by advertisements for "singles' weekends" at resorts where "fun doesn't stop at sundown." For us the major point is that the more or less subtle sanctions against the not yet married increase as the percentage of their married cohort increases. At least we shall assume this for the age span we will consider in the ensuing empirical analysis.

The simplest way to incorporate this assumption in a model, is to suggest that the social pressure on the not yet married is

proportional to the percentage of their cohort already married, and, furthermore, that the rate of change into the marriage state is proportional to this pressure. At the individual level the rate of change in the probability of getting married would then be:

$$\frac{dp_i}{dt} = qP_t \quad (1)$$

where p_i is the probability that individual i in the cohort will marry in the small time interval dt ; P_t is the proportion of the cohort already married at time t ; and q is the parameter of conversion into the marriage state.

However, it is hard to obtain observations about probability changes at the individual level. Therefore let us translate the individual probabilities into rates of change at the aggregate level.

Let us assume

$$\frac{dp_i}{dt} = \frac{dp_j}{dt} \quad (2)$$

for all i and j . That is, we do not assume the probability of getting married is the same for all cohort members, but that the rate of change in their respective individual probabilities is the same. To get the rate of change in the number married, we add the individual rates of change for those not yet married:

$$\frac{dm}{dt} = \sum_{i=m+1}^n \frac{dp_i}{dt} = (n-m) \frac{dp_i}{dt} = (n-m)qP_t \quad (3)$$

where m is the number married, and n is the number in the cohort. If we now divide (3) through by n , we obtain

$$\begin{aligned} \frac{dm}{ndt} &= \frac{d\left(\frac{m}{n}\right)}{dt} = \frac{dP_t}{dt} \\ &= \left(\frac{n-m}{n}\right)qP_t = q(1-P_t)P_t. \end{aligned} \quad (4)$$

Thus the last two terms in (4) yield an expression for the rate of change in the proportion married as a function of the proportion already married, the proportion not yet married, and the parameter of conversion. Note that the rate of change in individual probabilities in (1) differs from

the rate of change in the proportion of the cohort who enter into first marriage expressed in (4). Also we can speak in percentages rather than proportions if we multiply (4) by one hundred. Later, in our empirical analysis, we will calculate in percentages.

So far our model (4) looks like the differential equation for a logistic curve. However, not only social pressure affects the probability that a person will marry. Marriageability generally declines with age. There are several reasons for this, both individual and social. One is the effect of aging on personal attractiveness. Younger people are generally considered more attractive than older people. Youthfulness is much valued in our society. Much effort goes into appearing young. Also, the mean age difference between grooms and brides increases with the spouses' age at marriage. A demographic effect is that a cohort may be depleted of its most eligible marriage candidates: many will marry early, leaving a pool of the less attractive not yet married. At the same time that the number of potential marriage partners in the same age-group declines, their variability is likely to decline, making it harder to find a good match. As they age, the unmarried are viewed as having reduced appeal for marriage. Being spouseless petrifies an individual's habits. Cherished lifestyles and ingrained idiosyncracies make people less open to marital adjustment. As the English proverb goes, "He that marries late marries ill." Finally, age-correlated factors may affect marriageability. For example, those who remain unmarried may also be those without good jobs. In short, the longer people stay unmarried, the less fit they seem for marriage. They may be past their prime, though this "prime" may occur at different ages in different population subgroups as in, for example, education groups.

In mathematical terms these observations suggest that what we have called the conversion parameter for entry into first marriage, is itself a declining function of time:

$$q = f(t). \quad (5)$$

Next we must consider the specific nature of $f(t)$. Assume simply that each person starts out with a certain marriage potential, A , but that this potential declines with a

constant proportion b for each time unit, where b is the same for all individuals. For the population as a whole we then have

$$f(t) = Ab^t \quad (6)$$

where A is the average initial marriageability, and $b < 1$ its constant of deterioration.¹ This expression is analogous to constant depreciation of capital. Thus over time marriageability is a geometric progression: it decreases with a constant proportion for each time unit.

Inserting (6) in (4) we have

$$\frac{dP_t}{dt} = Ab^t(1 - P_t)P_t. \quad (7)$$

If we transpose, we can carry out the integration

$$\int_0^t \frac{dP_t}{P_t(1 - P_t)} = \int_0^t Ab^t dt \quad (8)$$

which gives

$$-\log \frac{1 - P_t}{P_t} \Big|_0^t = \frac{Ab^t}{\log b} \Big|_0^t. \quad (9)$$

When P_0 is a positive quantity, this expression can after exponentiation and rearrangement be written

$$P_t = \frac{1}{1 + \frac{(1 - P_0) \exp(A/\log b)}{P_0 \exp(Ab^t/\log b)}} \quad (10)$$

At first, this expression looks unwieldy. However, since both A and b are constants, we can always find a number such that

$$\log a = \frac{A}{\log b}. \quad (11)$$

If this is substituted into (10), the equation for P_t becomes

$$P_t = \frac{1}{1 + \frac{(1 - P_0)a}{P_0 ab^t}} \quad (12)$$

which, if we let $k = P_0/(a(1 - P_0))$, reduces to

$$P_t = \frac{1}{1 + \frac{1}{ka^t}} \quad (13)$$

¹ We have

$$\Sigma A_i b^t = \Sigma (A + \epsilon_i) b^t = nAb^t + b^t \Sigma \epsilon_i = nAb^t.$$

Taking the average gives Ab^t .

The curve corresponding to this equation looks somewhat like the logistic, but its inflection point is in general not midway between zero and its upper asymptote, so the limbs of the curve are not symmetric about the inflection point, as the logistic curve is constrained to be. Equation (10) or (13) can be viewed as describing a non-homogenous diffusion process. Later, we shall return to this point.

Note that P_0 is well defined and not equal to zero, but

$$P_0 = \frac{1}{1 + \frac{1}{ka}} \quad (14)$$

where k and a are estimated quantities. In practical terms this means that in an empirical curve fit we must take as t_0 the first year of the process.² Note also that

$$\lim_{t \rightarrow \infty} P_t = \frac{1}{1 + \frac{1}{k}} \quad (15)$$

since $b < 1$. Thus the percentage ever married approaches (16) as an asymptote as the cohort ages.

Thus of the two forces influencing the unmarried, the first is the increase in social pressure that accompanies the increase in the percentage of their cohort already married. "A bachelor is like a man in winter without a furcap," as the Russians say. The second is the decline in marriageability with age. Samuel Johnson said that "they that have grown old in a single state are generally found to be morose, fretful and captious, tenacious of their own practices and maxims." These two forces have opposite effects: one increases the pressure to marry, the other reduces the capacity to marry. We have put the common wisdom of our maxims in mathematical form. Having formalized the "verstehende" sociology, we shall now see how well the model fits the data.

EMPIRICAL ANALYSIS

How does our model correspond to real data? What we need to answer this question

is the cumulative percentage of a cohort married as it ages. We also need a technique to estimate formula (10's) parameters, P_0 , A and b , from these percentages—or, equivalently, k , a and b in (13) from which P_0 and A can be calculated. Once they are estimated, we can compute the predicted percentage married from the model for increasing values of t , and compare these calculated values to the observed ones. The better the model, the closer will be the fit between observed and calculated cumulative percentages.

The quantity

$$g_t = ka^t \quad (16)$$

in equation (13) has the general form of the so-called Gompertz function, named after the French mathematician Benjamin Gompertz who in 1825 attempted to fit it to the age curve of mortality. Since we have a simple procedure for estimating the parameters of the Gompertz function, we can rewrite (13) as

$$ka^t = \frac{P_t}{1 - P_t} \quad (17)$$

and then use this procedure for the Gompertz function to find estimates for formula (13's) parameters or formula (10's).

Raymond D. Prescott (1922) developed a simple procedure for estimating the parameters of the Gompertz curve from cumulative observations over time.³ Since the curve has three constants, we need three equations to find them. Equation (17) can be rewritten

$$\log g_t = \log k + \log ab^t. \quad (18)$$

If we divide the data into three equal sections, we can estimate the constants by the following formulas:

$$b^T = \frac{\sum_3 \log g_t - \sum_2 \log g_t}{\sum_2 \log g_t - \sum_1 \log g_t}; \quad (19)$$

$$\log a = (\sum_2 \log g_t - \sum_1 \log g_t) \frac{b-1}{(b^T-1)^2}; \quad (20)$$

$$\log k = \frac{1}{T} \{ \sum_1 \log g_t - \frac{(b^T-1)}{(b-1)} \log a \}, \quad (21)$$

² An alternative is to describe the process by its obverse, i.e. as leaving the single state rather than entry into marriage. Then we could let the percentage at time 0 equal 100.

³ There is also a reference to G. King (1909) for this procedure in the literature—compare Wolff (1970; 29 f.). It would also have been possible to use an iterative least squares procedure to obtain estimates of the parameters. See Hoem (1970).

where Σ_1 represents the sum of logarithms of observed cumulative percentages (for our purposes transformed by equation (18)), of the first section, Σ_2 the corresponding sum of the second section, Σ_3 the sum of the third section, and T the number of observations in each of the three sections. This estimation procedure is also described in Croxton, Cowden and Klein (1967). Furthermore, these formulas can be used for any cumulative distribution: one does not need cumulative *percentages*.

The data for our empirical analysis are taken from the U.S. census of population 1960. We found the most complete time series for the cohort of white women born 1920-24: cumulative first marriages are listed from ages fifteen to thirty-eight (U.S. Bureau of Census, 1968: Table 24, p. 71). In Table 1 we list marriage age in the first column, and the observed cumulative first marriages in the second. In the third we list expected values calculated by the above procedure, and in the fourth the difference between observed and calculated values. As the table shows, the fit is good: the largest

Table 1. Cumulative First Marriages for White Women Born in 1920-24 in the United States: Observed and Calculated Percentages.

Year	Observed	Calculated	Difference
15	1.80	1.68	0.12
16	4.60	4.32	0.28
17	9.60	9.35	0.25
18	17.60	17.30	0.30
19	27.50	27.67	-.17
20	38.10	39.06	-.96
21	48.30	49.90	-1.60
22	57.50	59.22	-1.72
23	65.30	66.70	-1.40
24	71.60	72.51	-.91
25	76.60	76.94	-.34
26	80.60	80.31	0.29
27	83.30	82.88	0.42
28	85.20	84.86	0.34
29	86.70	86.40	0.30
30	87.90	87.61	0.29
31	88.70	88.57	0.13
32	89.40	89.34	0.06
33	90.00	89.96	0.04
34	90.50	90.46	0.04
35	90.90	90.88	0.02
36	91.20	91.21	-.01
37	91.40	91.50	-.10
38	91.60	91.73	-.13

Estimated parameters: $A=1.046$
 $b=0.855$

Asymptotic value: 92.99

Source: U.S. Bureau of Census, 1968.

deviation is found for age twenty-two at which point the percentage married is *over*-estimated by 1.72%. The degree of fit is seen more clearly in Figure 1, where the observed curve of cumulative first marriages and the percentages calculated from the model both are drawn.

However, it would be of greater interest if we could fit the model to different subgroups in the general population and compare both the degree of fit and the estimated parameter values. We could also study how subgroup parameter values have changed over time, and then analyze these changes.

Table 2 lists the observed and calculated distributions of cumulative first marriages by sex and color for those thirty-five years old in 1960.⁴ This table shows that the values calculated on the basis of the model deviate most for whites, particularly female, in that the model *over*-estimates the percentage married around the age of twenty. The fit is somewhat better for nonwhites, though the model *under*-estimates the percentage married around the age of eighteen.

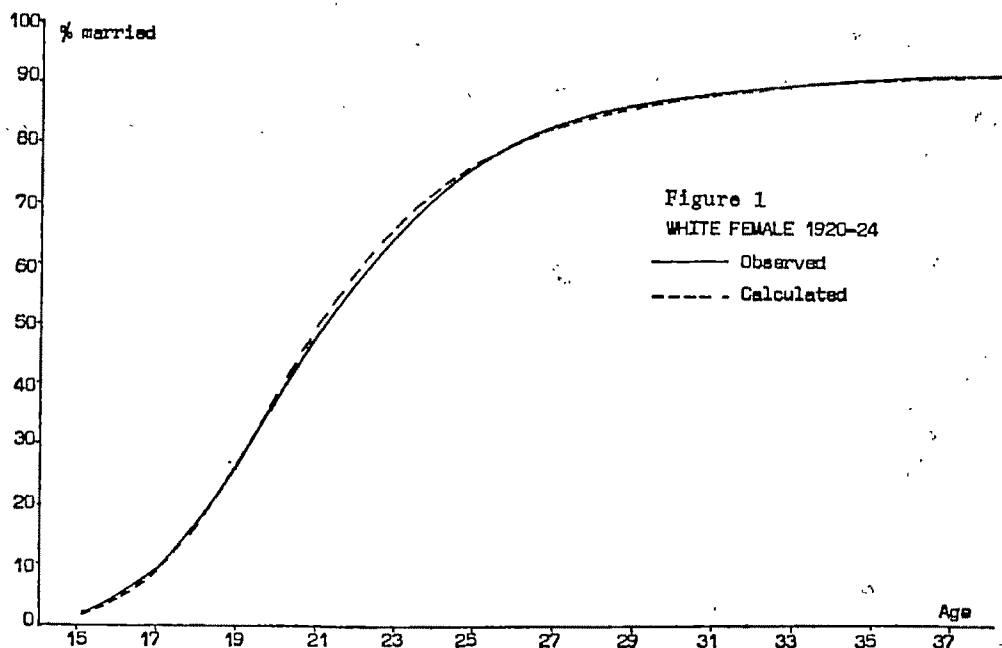
In Table 3 we have calculated the same distributions for those seventy-five years old in 1960. Generally the fit is better for this older generation of Americans. Here the greatest deviation is found for nonwhites.

Before we compare the parameters estimated from the distributions in Tables 2 and 3, let us discuss factors affecting the fit between the observed and calculated curves.

There are several possible sources of the deviation between observed and calculated distributions. We can divide them into three broad categories: historical effects, heterogeneity of cohorts, and the demographic effects of social norms.

As an example of the first, we can consider the percent over-estimated of white males thirty-five years old in 1960, entering first marriage between the ages of seventeen and twenty-one (see Table 2, column three). These men were born in 1925, and many were in the armed forces during the Second World War, from about 1942 to 1946. In

⁴The number married for each age is found in U.S. Bureau of Census (1966 a), in Table 2, pp. 30-36. The total number in each cohort is found in U.S. Bureau of Census (1966 b), in Table 1, on pages 11, 13, 21 and 23. From these numbers we can calculate the percentages listed in Tables 2 and 3.



effect many were removed from the marriage market between the ages of seventeen and twenty-one—precisely that age-group for which we found the greatest discrepancy between the values calculated by our model and the observed values in Table 2. Not surprisingly the same effect manifests itself for

white females—most strongly for the year 1945; while there is an under-estimation for 1942. Observe also that non-whites were practically unaffected. This may be because a smaller percentage of non-white men were inducted in the armed forces. Or it may be that their marriage patterns differ from the

Table 2. Cumulative First Marriages for U.S. Citizens 35 Years of Age in 1960 by Sex and Color (Obs=Observed, Calc=Calculated, Dif=Difference Between Observed and Calculated)

Age	White Male			White Female			Nonwhite Male			Nonwhite Female		
	Obs	Calc	Dif	Obs	Calc	Dif	Obs	Calc	Dif	Obs	Calc	Dif
14	0.19	0.13	0.06	0.95	1.07	-.12	0.49	0.67	-.18	3.28	4.75	-1.47
15	0.57	0.46	0.11	3.30	3.02	0.28	1.70	1.56	0.14	8.23	8.58	-.35
16	1.26	1.32	-.06	8.05	7.17	0.88	3.31	3.27	0.04	15.39	14.17	1.22
17	3.08	3.31	-.23	15.80	14.45	1.35	6.78	6.18	0.60	23.93	21.47	2.46
18	6.71	7.15	-.44	25.79	24.89	0.90	11.78	10.60	1.18	32.97	30.01	2.96
19	11.55	13.49	-1.94	35.20	37.18	-1.98	17.12	16.58	0.54	40.58	39.05	1.53
20	18.78	22.34	-3.56	45.36	49.39	-4.03	23.14	23.86	-.72	47.69	47.84	-.15
21	31.19	32.87	-1.68	57.95	60.01	-2.06	32.03	31.87	0.15	55.72	55.82	-.10
22	43.85	43.74	0.11	68.31	68.49	-.18	40.66	39.97	0.69	63.13	62.71	0.42
23	54.34	53.73	0.61	75.40	74.94	0.46	48.46	47.61	0.85	68.72	68.47	0.25
24	62.78	62.20	0.58	80.17	79.74	0.43	54.65	54.44	0.21	73.17	73.17	0.00
25	69.28	69.01	0.27	83.61	83.30	0.31	60.14	60.33	-.19	76.87	76.97	-.10
26	74.52	74.34	0.18	86.11	85.94	0.17	64.71	65.29	-.58	79.88	80.04	-.16
27	78.46	78.45	0.01	87.99	87.93	0.06	68.44	69.40	-.96	82.36	82.50	-.14
28	81.63	81.61	0.02	89.41	89.45	-.04	71.86	72.78	-.92	84.12	84.50	-.38
29	84.12	84.06	0.06	90.61	90.61	0.00	74.95	75.56	-.61	85.63	86.12	-.49
30	85.98	85.96	0.02	91.50	91.52	-.02	77.55	77.84	-.29	87.27	87.44	-.17
31	87.46	87.46	0.00	92.25	92.24	0.01	79.49	79.72	-.23	88.58	88.53	0.05
32	88.60	88.64	-.04	92.84	92.82	0.02	81.44	81.27	0.17	89.58	89.44	0.14
33	89.58	89.59	-.01	93.29	93.28	0.01	83.05	82.57	0.48	90.40	90.19	0.21
34	90.34	90.36	-.02	93.67	93.66	0.01	84.51	83.65	0.86	91.15	90.82	0.33

Source: Calculated from U.S. Bureau of Census, 1966a, and 1966b.
Compare footnote 4.

Table 3. Cumulative First Marriages for U.S. Citizens 75 Years of Age in 1960 by Sex and Color (Obs=Observed, Calc=Calculated, Dif=Difference Between Observed and Calculated)

Age	White Male			White Female			Nonwhite Male			Nonwhite Female		
	Obs	Calc	Dif	Obs	Calc	Dif	Obs	Calc	Dif	Obs	Calc	Dif
14	0.34	0.34	0.00	1.13	1.76	-.63	0.59	1.00	-.49	3.84	6.37	-2.53
15	1.09	0.78	0.31	4.07	3.74	0.33	2.59	2.01	0.58	10.77	10.41	0.36
16	1.99	1.63	0.36	7.64	7.09	0.55	4.06	3.70	0.36	17.83	15.74	2.09
17	2.90	3.10	-.20	12.88	12.10	0.78	6.31	6.26	0.05	24.43	22.18	2.25
18	4.61	5.46	-.85	20.05	18.73	1.32	10.29	9.81	0.48	30.55	29.33	1.22
19	7.58	8.90	-1.32	27.80	26.52	1.28	14.90	14.33	0.57	38.30	36.68	1.62
20	12.41	13.53	-1.12	36.19	34.82	1.37	20.70	19.65	1.05	46.08	43.80	2.28
21	18.85	19.23	-.38	44.19	42.92	1.27	27.16	25.49	1.67	52.52	50.36	2.16
22	26.05	25.73	0.32	51.12	50.34	0.78	32.85	31.49	1.36	56.95	56.19	0.76
23	32.60	32.62	-.02	56.79	56.80	-.01	37.76	37.37	0.39	60.96	61.23	-.27
24	39.32	39.51	-.19	61.74	62.27	-.53	42.11	42.90	-.79	65.22	65.53	-.31
25	46.18	46.04	0.14	66.42	66.80	-.38	47.26	47.93	-.67	68.77	69.15	-.38
26	52.18	51.99	0.19	70.09	70.52	-.43	51.05	52.41	-1.36	71.64	72.18	-.54
27	57.35	57.27	0.08	73.14	73.56	-.42	55.06	56.33	-1.27	73.79	74.71	-.92
28	61.87	61.85	0.02	75.73	76.04	-.31	58.41	59.72	-1.31	75.64	76.83	-1.19
29	65.78	65.77	0.01	77.84	78.08	-.24	61.74	62.65	-.91	77.56	78.61	-1.05
30	69.05	69.11	-.06	79.57	79.75	-.18	64.38	65.15	-.77	79.97	80.11	-.14
31	71.77	71.92	-.15	81.02	81.12	-.10	66.23	67.29	-1.06	80.75	81.37	-.62
32	74.07	74.30	-.23	82.41	82.26	0.15	69.21	69.13	0.08	82.44	82.44	0.00
33	76.39	76.31	0.08	83.42	83.22	0.20	71.90	70.69	1.21	83.95	83.35	0.60
34	78.31	78.02	0.29	84.33	84.01	0.32	74.23	72.04	2.19	85.80	84.12	1.68

Source: Calculated from U.S. Bureau of Census, 1966a, and 1966b.
Compare footnote 4.

whites'; for example their bride and groom age differences may be greater and may vary more. Such phenomena may have attenuated the effects of war. When large numbers of eligible candidates suddenly are released on the marriage market, as by demobilization, we may also get "bumps" on the curve of observed cumulative first marriages, and deviations from the calculated curve.

A second source of deviation is the heterogeneity of cohorts. Such heterogeneity may be of different kinds. The first has to do with the difference between "forced" and "voluntary" marriages. That is, especially in the younger age categories, a fairly high proportion of the marriages occur because the female is pregnant. We may expect the model to fit better if this group is removed from consideration, or considered separately, since the source of these marriages is not explicit in the model. In empirical analysis we can circumvent this by truncating the lower tail of the cumulative distribution, i.e. by starting the analysis at the age of, say, eighteen instead of fifteen. Another source of heterogeneity is the different marriage habits of subgroups in the population at large. We have already taken note of ethnicity. But it is also well known that those who get a higher education on the

average marry at a later age than those who do not. It can be shown, for example, that the marriage capacity of white women born from 1920-24 increases with increasing education, while this capacity declines somewhat faster for those with more education.⁵ Urban and rural residents may differ similarly. Indeed, the differences between whites and nonwhites in Table 2 may to some extent be due to the larger proportion of non-white rural residents.

Finally, the demographic effects of social norms may cause a deviation. In most societies men generally marry women younger than themselves. However, cohorts may be of different sizes, forcing either a narrowing or a widening of the age-gap between spouses. When cohorts differ widely in size, this may distort the estimates of the marriage capacity of a given cohort. In such cases we may expect the calculated distribu-

⁵ The data for this analysis are found in U.S. Bureau of Census (1968), in Table 24, p. 71. The estimates for *A* and *b* are:

Education	<i>A</i>	<i>b</i>
Less than 8 yrs	0.63	0.878
8 yrs	0.74	0.887
1-3 yrs hi-school	0.85	0.879
4 yrs hi-school	1.31	0.856
1-3 yrs college	1.44	0.852
4+ yrs college	1.56	0.843

tions to deviate from the observed for either males or females, or both. There may also be "bumps" on the curve of cumulative first marriages in societies where marriage is prescribed to occur at a given age.

With these reservations in mind, let us turn to a numerical illustration of how our model can describe the differences between sexes and racial groups, and their changes over time. In Table 4 we list the parameters for the process of entry into first marriage estimated from the distribution in Tables 2 and 3.

Looking first at the estimated asymptotic values for the percentage of cohorts that will ever marry, we see that for both racial groups and for both sexes, there has been an increase historically. At the same time the first rows in Tables 2 and 3 show a decrease in P_0 . That is, the percentage married at age fourteen, whether observed or calculated, is smaller for those who were thirty-five in 1960 than for those who were seventy-five. However, of greater interest are the parameters governing the process over time, i.e., the initial marriage capacity A and its constant of deterioration b .

As for marriage capacity A , we can draw several conclusions from Table 4:

1. For both cohorts marriageability is greater for men than women.
2. Marriageability is greater for whites than nonwhites.
3. There has been an increase in initial marriage capacity for all groups.
4. The increase has been greater for whites than nonwhites.

Table 4. Estimated Parameters for Entry into First Marriage of U.S. Citizens, 35 & 75 Years of Age in 1960, by Sex & Color (A=Marriageability, b=Constant of Deterioration, V=Asymptotic Value for the Percentage Ever Married)

	75 Years			35 Years		
	A	b	V	A	b	V
MALE						
White	0.87	0.873	88.83	1.33	0.869	94.27
Nonwhite	0.76	0.882	80.84	0.91	0.880	90.02
FEMALE						
White	0.83	0.873	88.68	1.14	0.861	95.58
Nonwhite	0.57	0.889	89.32	0.67	0.893	94.80

5. The increase has been greater for men than women.
6. The difference between the sexes is greater for nonwhites than whites, but has increased most for whites.

If we turn to the constant of deterioration b , we can conclude:

1. The differences between the groups are small.
2. The changes over time are small.
3. For both cohorts the marriage capacity of whites declines faster than the nonwhites' (i.e., lower b 's).
4. For all groups except nonwhite females thirty-five years old in 1960, the increase in marriage capacity has to some extent been offset by a slightly faster rate of deterioration.

By this mode of analysis, a cohort's marriage history is condensed in three parameters, and can economically be compared to that of another cohort. One could also study subgroup differences in the population by conditional projection. One can, for example, keep two of the three parameters for one group and substitute the third from another.

It would take us too far to elaborate and interpret the above results in terms of this century's social and demographic changes. A complete analysis would call for more homogenous subgroups. An elaboration would require much supporting evidence. Since our purpose has only been to show how to apply the model to real data, this task is for future research. Instead we shall comment in general on the type of model test we have effected, and on the model itself.

DISCUSSION

The general problem with our type of analysis is that the fit between the observed and calculated curve of first marriages in itself is not a strong test of the model. Since the model is the only link between the data and the assumed substantive causal processes (measured by the parameters), it is hard to tell whether the resulting estimates (e.g., those of Table 4) actually reflect the causal forces involved. The model, which contains the common wisdom we began with, is clearly admissible by the data. But by the above analysis, we have not eliminated

other causal processes that might generate curves of cumulative marriages with the same rough shape.

One way to do so, is to specify alternative causal processes and fit the resulting models. A model generating a logistic curve, for example, systematically gives a much worse fit, and hence can be eliminated. But one could make assumptions generating a Gompertz or doubly exponential curve.⁶ We have fitted fifteen different cohorts with both the Gompertz curve and the model presented here in (10). In ten of them the mean squared deviation was less with our model. In the five cases where the Gompertz curve gave a better fit, it did not on the whole do *as much better as* our model did in the other ten cases. But it would of course be premature to take this as conclusive empirical evidence that our model is preferable to one generating a Gompertz curve.

A much stronger test of the causal forces represented by the parameters would be to derive alternative measures of them, or to confront other empirical consequences of the model with data. For example, one could try to derive other measures of different groups' marriage capacity and find whether they have the same overall structure as the estimates of marriageability in Table 4. This

⁶ I first fitted a Gompertz curve directly to the cumulative percentages of first marriages; the model in (10) was developed later. After this paper was first submitted to the *ASR*, I was made aware of a paper by Ansley J. Coale (1971) who independently hit upon the idea of graduating the curve of cumulative first marriages by a doubly exponential curve. He uses data different from ours (German, Dutch, Hungarian, Taiwanese, Australian, Swedish, and American) and finds that the same basic curve of first marriages "is virtually universal. . . . Moreover, the uniform age structure of nuptiality occurs in societies in which most marriages are arranged by families with little regard for the preference of bride and groom, and in societies in which marriages typically result from the self-selection of mutually preferred partners." (p. 10). Coale suggests that there is a fraction of a cohort which is not in the marriage pool, and that those in the pool experience "a risk of marriage that arises from zero at the conventionally defined minimum age to a maximum risk that is maintained until the last marriages that occur to the cohort." (p. 13). Thus the assumptions about the causal forces underlying the process differ from ours: we explicitly try to build a model incorporating simple sociological assumptions which generate a curve of the general shape which is universally observed.

task must remain for future research. But until this is done, not only can models with a different mathematical form give as good a fit, but the parameters we have estimated can also be interpreted differently.

But we can make a theoretical argument in favor of the model expressed in equation (7) or (10): we can link it to diffusion theory. As mentioned, equation (10) can be considered a model for a non-homogenous diffusion process. In the classic diffusion model (generating a logistic curve), the parameter of conversion is considered a constant, and its differential equation looks like (4). Our model is like a diffusion model in that it considers the rate of change in the number married ("the haves" in diffusion models) to be proportional to those already married as well as to the number not yet married ("the have nots" or "susceptibles" in diffusion models). However, implicit in the assumption of a *constant* of conversion, is the assumption that the diffusion takes place in a *homogenous* population. But most populations are not homogeneous, and this should explicitly be allowed for in the construction of the mathematical model.

Heterogeneity in this context may be of two basic kinds: structural, as when some *capacity is differentially distributed* in the population, and dynamic, when the *population changes as the process goes on*.

In this article, we made the latter assumption: that as marriage over time is diffused through a cohort, the marriageability of those not yet married changes. In particular we made a simple assumption about the nature of this change: that marriage capacity declines with a constant proportion per time unit. Note that we could have specified the nature of the change differently. Many, for example, would argue that in the conception of a "prime age" for marriage, lies the notion that marriage capacity first increases and then begins to decline, so that we have a skewed distribution for marriageability over time, looking somewhat like the lognormal rather than the negative exponential we chose in (6). One can easily incorporate such conceptions in the model by changing his assumption about the specific nature of (5). The general differential equation for the kind of

diffusion process we have in mind would then be

$$\frac{dP_t}{dt} = f(t)(1 - P_t)P_t \quad (22)$$

where $f(t)$ is to be specified and where the equation's general solution is simple. Note also that this equation is not the equation for a deterministic process, though the form is the same as some deterministic equations for diffusion. By the assumptions we have made in equations (1) - (4), it is the equation for a probability process; but the realism of these assumptions must be evaluated in each particular application.

The particular specification we have made of $f(t) = Ab^t$ might be useful also in analyzing the diffusion of other phenomena. For example, if we have a situation in which some characteristic declines throughout the population (i.e., is non-homogenous with respect to social space) or declines with time (e.g., is non-homogenous with respect to age), the model is often appropriate. Suppose we have a population of widely differing incomes, then the marginal cost (in utility terms) of acquiring an expensive good is largest for those of low income, and the capacity to acquire the good declines as we descend the income ladder. The diffusion of dish-washers, for example, can perhaps be described by such a process. We might also apply such a model to the political mobilization of voters. And one need modify the model only slightly to add situations in which some capacity *increases* in social space or with age. Take, for one example, an invention of great use made at the bottom of the social ladder; or, for another, the changing capacities for learning with increasing age; or for still another, situations in which the "have nots" are put at a competitive disadvantage with an increase in "haves," as with the introduction of new machinery in a competitive industry. Finally, the specification $f(t) = Ab^t$ has the advantage that the logistic curve, which is the model most often used to describe contagious diffusion processes, is a special case: for $b = 1$.

Thus the theoretical argument in favor of our model is essentially that change in the number married can be seen as a form of diffusion through a cohort over time.

CONCLUSION

In this article we developed a model for the process of entry into first marriage. The members of a cohort are seen as subject to two forces. One is the increase in social pressure to get married as the percentage married of the cohort increases. The other is the declining capacity of members of the cohort to get married as they age. With some additional assumptions, this gives rise to a model for a non-homogenous diffusion process. The use of the model was illustrated by real data, and the fit found to be good. We discussed factors contributing to deviation between observed and calculated distributions. However, much empirical work must be done before the model is adequately tested. If this article can stimulate such research, as well as other empirical applications of the model expressed in (22), it will have accomplished its purpose.

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SEX AND SOCIAL PARTICIPATION *

ALAN BOOTH

University of Nebraska

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To compare the extent and quality of participation by men and women in friendship dyads, voluntary associations and kin relations, we drew on interview data from a sample of adults in two urban communities. While males had more friends than females, we found female friendship relations to be affectively richer. Men exceeded women in the number of voluntary association memberships, but not in commitment of time to group activities. Extensive kinship resources were found to effect men's and women's affiliations differently. We found no evidence to support recent claims that male bonds are stronger than women's.

ANTHROPOLOGIST Lionel Tiger (1969) holds that men have a biologically transmitted and socially learned propensity to form bonds with other males which are stronger and more stable than female affiliations. His analysis, which includes formal voluntary associations, informal friendship groups and work groups, cites examples of male dominance in the polity, economy, and military.

Social scientists have discovered noteworthy exceptions to Tiger's thesis. For example, Blau (1961) found that widowhood decreases friendship participation more among men than among women. Babchuk and Booth (1969) noted that voluntary association participation was more variable and less stable among men than women. However, no systematic comparisons of male and female social participation patterns have been reported.

This investigation explores likeness and difference in the type, extent and quality of men's and women's social participation in friendship dyads, voluntary associations and kin relations. While other social participation settings exist, these are probably among the more visible and crucial in western societies.

Origins of Sex Differences in Participation

Sexually differentiated social behavior seems nearly universal. Summarizing studies

of pre-literate and relatively non-industrialized societies D'Andrade (1966:201) states "The cross-cultural mode is that males are more sexually active, more dominant, more deferred to, more aggressive, less responsible, less nurturant and less expressive than females."¹ Studies in industrialized societies show an institutionalized male dominance in many families and a female tendency to accommodate (Stuchert, 1963), particularly in lower-strata families (Brim, *et al.*, 1962). In game experiments, Bond and Vinacke (1961) find males using exploitative strategies and females accommodative ones. Labor force studies indicate that . . . "The typical employed woman . . . is not wholeheartedly in competition with the employed man." Wolfe, 1954:236). Females can, of course, be exploitative and instrumental as in finding a mate or protecting their children; but their model response in the U. S. seems accommodative and noncompetitive.²

Biological and cultural factors bring about these behavior differences (Kolhberg, 1966). Although we don't know enough about the effect on human behavior of sex hormone variations in timing and concentration (Dornbusch, 1966:216), Levine's studies (1966) suggest that differences in sexual behavior may be rooted in biology.

Non-biological explanations for sex differences center on early socialization. Females

¹ Note that in a few cultures these differences don't exist, and in others they are reversed (D'Andrade, 1966).

² A comprehensive study of associated personality differences is consistent with these findings (Bennett and Cohen, 1959). However, there is evidence that "masculine patterns of thought" are deteriorating.

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learn to behave maternally even toward their husbands and elderly parents; while males specialize in instrumental activities (Parsons and Bales, 1955:23). In one study, college women reported that their parents: hastened the boys' emancipation from the family and retarded the girls, gave boys "more frequent opportunities for independent action" and held girls "to a more exacting code of filial and kinship obligations" (Komarovsky, 1950). Boys particularly from the middle-class compete in teams far more than girls.³ In some areas, female team sports are even illegal.⁴ Team competition fosters and provides social conditions favorable to friendship formation. For teams are small and require prolonged contact, high frequency of interaction and member homogeneity.⁵ Furthermore, in team activities a boy learns group procedures and practices which he can later apply to role performance in voluntary associations. Thus, early independence training and team activities develop social initiative in males. Coordinate normative structures define adult male social behavior in aggressive terms.⁶ The women's liberation movement, however, may be signaling a shift in these modes of behavior. How, then, does the predilection to aggress or accommodate effect patterns of social participation?

METHOD AND DATA

Our data were provided by eight hundred non-institutionalized adults forty-five years and older residing in two Midwestern urban areas (Lincoln and Omaha, Nebraska). Al-

³ The term "team" includes all interschool, rival groups: forensic leagues, chess clubs and athletic teams.

⁴ Many states have laws forbidding interschool athletic competition to female teams. Most city recreation departments organize far more male than female teams at all age levels.

⁵ Bates and Babchuk (1961) suggest that the above four sociological elements constitute the preconditions for primary relations to develop; while social-psychological factors actually make up the primary tie. The dimensions they suggest are (1) predisposition to enter into a wide range of activities with one another; and (2) affectivity. The latter incorporates the mutual expression of endearment, the repetition of private traditions and the freedom to make demands on one another.

⁶ The term aggression refers to the propensity to initiate activity and not to the adverse or hostile content of an act.

though the age of the sample limits our ability to generalize about younger people, it offers several advantages. First, by middle age patterns of social participation have been firmly established. Second, we could study the effect of the severe change in social relations that occurs between middle and old age on sex related participation differences (Rosow, 1967).

By drawing respondents from two urban areas, we had a range of occupational and ethnic backgrounds represented. The two urban centers also reflect divergent community origins, since many residents migrated from within the state.

We used two techniques to select the sample: (1) sampling from a list (city directories); and (2) sampling of areas. A screening interview identified the dwellings of eligible persons. An additional selection procedure insured that the respondents were representative with respect to age and sex (Paisley and Parker, 1965).⁷

Respondents were questioned in detail about their social participation patterns. Each was asked: "Whom do you think of as being your friends?" To exclude those of limited significance to the respondent, we asked: "How many do you consider your closest friends?" We then asked detailed questions about each friend.

⁷ The sex ratio used to insure representativeness was .89, the ratio of men to women in Lincoln and Omaha according to 1960 Census estimates. We incorporated this ratio in our sampling procedures. To our surprise, our sample had a sex ratio of .52. An analysis of screening interview data coupled with other 1960 Census data indicated that household data varied from what the population data suggested in at least two ways. First, women forty-five and older were twice as likely as men to be living without a spouse (the majority were single and widowed). Thus, the number of households without elderly men in residence exceeds what we expected from the overall population ratio. Second, women were twice as likely as men to be living with other relatives (often with the adult daughter's family) and unrelated individuals (other elderly women). Thus, the number of households with an exclusively female population of over forty-five exceeded our expectation. These two factors explain the disproportionate number of females in the sample. Comparing the social-economic, educational and age composition of the male-female sample with independent estimates (1960 Census), did not reveal other differences that would bias our analysis.

We used a similar procedure to narrow the number of kin. We asked, "How many relatives do you feel close to?"

The respondents were handed a cue card and asked about voluntary associations: "Do you belong to any organizations like the ones on this list?" The cue card contained examples of church, job, recreation, fraternal-service, civil political, youth, veteran, and community agency groups. A respondent was considered affiliated only if he could state when he joined, the group's purpose, and his commitment to it.

FINDINGS

Friendship

While the female's expressive-accommodative activities are important in primary relationships, the male's initiative and social skills should give him the quantitative edge in friendship resources. Studies of white-collar couples show that husbands are more likely than wives to initiate and maintain their mutual friendships (Babchuk and Bates, 1963; Babchuk, 1965). Such initiative may characterize the husbands' friendships outside the marriage as well. Men, then, were expected to have more close friends than women.

The data support our contention, but not convincingly (see Table 1). Males report a slightly higher number of friends than females. The difference is not statistically significant. Sex differences emerge more clearly only when we control for early socialization differences.

Blue-collar men and women report a like number of close friends. White-collar men report a greater number than white-collar women. This class difference may reflect the white-collar youth's greater part in organized team activities.⁸ Furthermore, white-collar males with white-collar fathers reported more friends than middle-class males of blue-collar origin, a finding consistent with a study showing an inter-generational as well as intra-generational continuity in social participation patterns (Baeumler, 1965:163-

⁸ While athletic teams draw heavily from all social strata (Hollingshead, 1949:194; Coleman, 1961:284) other teams draw most of their members from the higher strata (Hollingshead, 1949:199-203).

Table 1. Close Friends Reported by Men and Women - Means and Standard Deviations.

Respondent's	Males	Females	p ¹
<u>Status</u>			
Blue Collar	3.3(137) ² <i>3.0³</i>	3.2(230) <i>2.7</i>	-
White Collar	4.5(138) <i>3.8</i>	3.9(289) <i>3.2</i>	<.10
<u>Marital Status</u>			
Married	4.2(231) <i>3.5</i>	3.7(276) <i>2.8</i>	<.10
Single, Separated or Divorced	3.0 (23) <i>3.8</i>	3.7 (70) <i>3.2</i>	-
Widowed	2.0 (21) <i>2.4</i>	3.5(177) <i>3.2</i>	<.10
<u>Physical Disability</u>			
None	4.3(199) <i>3.8</i>	3.6(352) <i>3.0</i>	<.05
<u>Activities</u>			
Constrained	3.1 (75) <i>2.9</i>	3.5(172) <i>3.1</i>	-
TOTAL	3.9(275) <i>3.5</i>	3.6(525) <i>3.0</i>	<.25

¹One way analyses of variance were computed to obtain the probability of the observed differences.

²The number of cases upon which the mean is based appears in the parentheses.

³Standard deviations are in italics.

166). Early socialization is probably reinforced by later experience. Many white-collar occupations use committees, task forces and buzz groups extensively, activities which sharpen the skills necessary to cultivate friendships. Married men (see Table 1) report a greater number of friends than married women; whereas, the single, separated or divorced male reports approximately the same number as the female in these categories. The friendship ties of widowed females, though, far exceed widowers'.

The female widowed early and the male widowed at any age have a deviate status relative to the majority of their peers. Blau (1961) found of older people that a change in marital status which places them in a deviant position in their age or sex group brings a shift in interests and experiences that erodes old friendship ties. Thus, the female widow, sixty-five or older, should report more friends than her younger counterpart and than all widowers. Indeed, this

Table 2. Characteristics of the Friendship Relations Reported by Men & Women -- Means & Standard Deviations.¹

Friends	Males	Females	p ²
Opposite Sex	14.8(275) ³ <i>23.1⁴</i>	9.4(525) <i>19.8</i>	<.01
Same Age	54.2(275) <i>38.5</i>	60.9(525) <i>39.3</i>	<.05
Having Weekly Contact	13.4(275) <i>28.9</i>	28.2(525) <i>34.7</i>	<.01
Spontaneous Activities	46.3(275) <i>42.0</i>	59.9(525) <i>41.8</i>	<.01
Confiding in Each Other	38.7(275) <i>42.7</i>	52.8(525) <i>43.1</i>	<.01

¹The percent of the respondent's friends in each of the categories indicated was computed. The means of these values comprise the entries in the table.

²One way analyses of variances were computed to obtain the probability of the observed differences.

³The number of cases upon which the mean is based appears in the parentheses.

⁴Standard deviations are in italics.

category largely accounts for the observed differences. The friendship resources of the younger widow should compare to the male's. Unfortunately, there are too few middle-aged widowers to test this interpretation.

If he is physically handicapped the male's friendship resources drop to the female's level (see Table 1). Applying other controls, labor force membership, ethnicity, residential and occupational mobility, we found no other differences in the number of friends reported by men and women.

The majority of close friends described by respondents were the same sex. Males, however, had more friends of the opposite sex than did females (see Table 2), 14 percent as compared to 9. Men are encouraged early to make contact with females, women to be more passive in cross-sex contact. The early courting behavior probably influences cross-sex interaction throughout life. The fact that men have more friends of the opposite sex runs counter to Tiger's male bonding hypothesis. Women have the greater propensity for same-sex bonding.⁹

⁹A more detailed analysis of the respondents'

The sexes were alike with respect to their close friends' education and residence, but females tended to have more friends nearly their age (see Table 2). Men, particularly laborers, tended to have more older friends. Most were men they see at work; presumably many were their supervisors.

We considered two preconditions for men and women's primary relations: the tenure of the relationship and amount of interaction. The tenure was the same for both sexes, but women saw their friends more often (see Table 2). Women, even when employed, contacted 28 percent of their friends weekly and, males only 13 percent. Women of the lower class, however, did not visit more often than men.

We assessed the two social-psychological qualities of friendship ties, proposed by Bates and Babchuk (1961). The predisposition to enter into a wide range of activities with one another is reflected in spontaneity of interaction. Of each friend, the respondent was asked: "Have you ever done anything with this person on the spur of the moment?" The second component, affect, is often expressed in the freedom to make demands. We asked: "Have you ever talked to this person about personal difficulties that have made you worry or have been a problem to you?"

Female friendship ties were richer in spontaneity and confidences than males'. Women did things on the spur of the moment with 59 percent of their friends, men with 45 percent (see Table 2). Women confided in 52 percent of their friends, men in 38 percent.¹⁰ Although spontaneity and affect seemed to characterize the close ties of the middle-aged, married and white-collar respondents more than it did the elderly, unmarried and blue-collar respondents, the sex differences in spontaneity and confiding behavior persisted in each category.

In short, only healthy, white-collar married men had more close friends than women.

opposite-sex friendships may be found in a paper entitled, "Cross-sex Friendship," available from the author.

¹⁰ These differences suggest that men and women may define friendship differently. Women are more likely than men to label as "close" people they confide in and behave spontaneously with. Clearly more measures of intimacy are needed.

In all other categories females reported approximately the same number, except the unmarried whose resources exceeded the males'. When men surpassed women it appeared to be due to early socialization. There is little to suggest a socio-biological aggressive disposition that yields men greater success in friendship. The females' consistent number of friends is probably related to early training in socio-emotional roles. Women's dominance in the qualitative aspects of friendship is likewise the residue of early training. Clearly, men have no edge over women in either the number or strength of friendships.

Voluntary Associations

Evidence on male-female differences in voluntary memberships is mixed. Scott (1957) using data from a community study shows a male affiliation of 75 percent compared to 56 percent for women. Hausknecht's (1962:31-32) study using data from national probability samples shows only slight differences between the sexes. A third study of the adult population of a midwestern state shows a somewhat greater proportion of affiliated men than women (Babchuk and Booth, 1969). Our data suggest a more complex pattern than these.

While white-collar men belong to more voluntary associations than women, women spend as much time monthly on associations (see Table 3).¹¹ Women in all social strata show stable patterns of participation through life, whatever their condition (see Table 3).¹² Where female memberships exceed the male's (among the unmarried, unemployed and disabled), their monthly time in associations far exceeds the male's. Thus, voluntary association participation is very much like

¹¹ On other variables, the data generally replicate previous findings (Babchuk and Booth, 1969; Hausknecht, 1962; Hyman and Wright, 1971; and Scott, 1957 among others) which show that healthy middle-aged, highly educated, native born, white-collar and upwardly mobile members of the labor force belong to more formal voluntary groups than persons in other categories.

¹² While unmarried and disabled females are less active than the married and healthy, the decline in participation from these factors is far less for women than for men.

Table 3. Mens' and Womens' Participation in Voluntary Associations -- Means and Standard Deviations.

	Males	Females	p ¹
No. of Memberships	2.3(275) 2.4 ³	1.7(525) 1.8	<.01
Hrs/Mo Spent on Association Activities	9.0(275) 17.1	9.8(525) 20.5	-
Status			
Blue Collar			
Memberships	1.3(137) 1.8	1.1(230) 1.3	-
Hrs/Mo	6.8(137) 18.9	5.4(230) 11.2	-
White Collar			
Memberships	3.2(138) 2.7	2.2(289) 2.1	p<.01
Hrs/Mo	11.2(138) 14.8	13.5(289) 25.2	
In Labor Force			
Memberships	2.8(173) 2.8	1.8(159) 1.9	<.01
Hrs/Mo	10.9(173) 18.3	8.4(159) 18.6	<.25
Not in Labor Force			
Memberships	1.5(101) 1.8	1.7(366) 1.9	
Hrs/Mo	5.7(101) 14.3	10.5(366) 21.4	<.05
Marital Status			
Married			
Memberships	2.4(231) 2.5	1.9(276) 2.1	<.05
Hrs/Mo	10.2(231) 18.2	11.9(276) 23.8	-
Single, Separated, Divorced, Widowed			
Memberships	1.6 (44) 2.1	1.5(247) 1.6	
Hrs/Mo	2.8 (44) 7.1	7.6(247) 16.0	<.05
Age			
45-64			
Memberships	2.6(190) 2.8	1.8(333) 2.0	<.01
Hrs/Mo	10.4(190) 17.7	10.2(333) 21.2	-
65 +			
Memberships	1.6 (84) 1.8	1.6(188) 1.7	-
Hrs/Mo	5.9 (84) 15.3	9.2(188) 19.8	<.25
Physical Disability			
None			
Memberships	2.6(199) 2.6	1.9(352) 2.0	<.01
Hrs/Mo	11.5(199) 19.3	11.8(352) 23.7	-
Disabled			
Memberships	1.5 (75) 1.9	1.4(172) 1.7	-
Hrs/Mo	2.5 (75) 6.1	5.8(172) 10.9	<.05

¹One way analyses of variances were computed to obtain the probability of the observed differences.

²The number of cases upon which the mean is based appears in the parentheses.

³Standard deviations are in italics.

the participation pattern for friendship ties. While males do belong to more organizations than females, it is only characteristic of men whose early and adult socialization favors such participation.

Interpersonal and organization skills, a white-collar job, good health and marriage are valued attributes. Organizations seek men with such qualities. However, once disabled, old, or even divorced, the male's value to the organization declines.

These same features in the female have a different meaning for her participation in groups. First, white-collar status doesn't signify early training for group work. Second, selective (and discriminatory) recruitment practices concentrate women in jobs with limited control over personnel and material resources. They are valued primarily as menials. Third, being unmarried like being elderly, is common among women, and not a deterrent to participation. Unmarried and aged women are as common in voluntary groups as married and middle-aged women.

How are these patterns reflected in participation in different types of organizations? Men tend to belong to different types of groups than women (Babchuk and Booth, 1969). Females are more likely to join church and recreation groups; while men join governing boards, job-related and fraternal-service groups. Both join civic-political and youth-serving groups like the scouts. These findings are extended if we classify voluntary groups as instrumental, expressive, or mixed.¹³ Instrumental associations, groups organized to cope with the external environment,¹⁴ entail more aggressive than accommodative behavior on the part of members. Thus we expected that more men than women would belong. Moreover, in such groups women would be relegated to canvassing and secretarial duties.

Expressive groups are organized to prevent deviant behavior. Their activities, basically accommodative and nurturant in

character, include socialization and personality integration.¹⁵ Thus more women than men were expected to belong to such groups. However, expressive groups would be led by men. The third type stresses mixed activities; for example, the American Legion both lobbies nationally for "American" legislation and promotes local fraternal activities.

We classified respondents' voluntary associations by their instrumental or expressive objectives as follows:

Instrumental

- economic—professional, trade and commercial associations
- political—governing boards, protest groups and political parties
- military—veterans' organizations

Expressive

- educational—home-school associations and adult leaders of youth programs
- religious—altar societies, choir, missionary societies and other church-related groups
- recreational—card clubs, bowling leagues and fraternal-service organizations
- health and welfare—Heart Association, Red Cross, hospital voluntary societies and allied groups

We also classified secondary objections to identify instrumental-expressive organizations.

Forty-four percent of the men belonged to one or more instrumental group but only 13 percent of the women did so (see Table 4).¹⁶ Men dominated the leadership of instrumental groups as well; 21 percent compared to 7 percent for women held office or chaired a committee. Though women in mixed-mode groups were as likely as men to hold office, men tended to belong more often than women. In expressive groups, women were more active than men; 61 percent belonged to such groups compared to 53 percent for men. Thirty-five percent of the women compared to 25 percent for men had held office.¹⁷

¹³ Special interest and hobby groups, religious organizations and card clubs are examples of expressive organizations.

¹⁶ In the analysis of organization function the number of multiple memberships is so reduced that it is no longer appropriate to use means or other measures of central tendency; therefore we adopted a proportional analytic mode.

¹⁷ The sex differences are not influenced by including one particular type of organization like a church group in the category of expressive associations.

¹³ The instrumental-expressive organization analysis, formed by Talcott Parsons (Parsons, 1954: 386-439; Parsons and Smelser, 1956:16-18) was extended to voluntary associations first by Babchuk and Gordon (1962:37-38) and later by Booth *et al.* (1968).

¹⁴ Instrumental associations include governing boards, trade and professional associations, community action groups and similar organizations.

Table 4. The Affiliation and Leadership Patterns of Men and Women in Instrumental, Expressive and Instrumental-Expressive Organizations.

	Proportion Affiliated			Proportion Occupying Leadership Positions		
	Male(275) ¹	Female(525)	p ²	Male(275)	Female(525)	p
Instrumental Organizations	44% (122)	13% (66)	<.001	21% (57)	7% (36)	<.001
Instrumental Expressive Organizations	26 (71)	14 (72)	<.001	7 (19)	8 (42)	-
Expressive Organizations	53 (147)	61 (322)	<.001	25 (70)	35 (186)	<.01

¹The number of cases upon which the proportions are based appears in the parentheses.

²Chi squares were calculated to obtain the probability of the observed distributions.

Thus, the power distribution in voluntary groups is based on factors other than sex role distinctions.

Data on male-female membership in expressive groups is related to Tiger's (1969) thesis that males tend more to belong to all-male groups than females to all-female groups. In analyzing sexually exclusive organizations (usually found in the expressive category), we find that females are as likely to belong as males.¹⁸ Our data, then, do not support Tiger's hypothesis.

We then introduced job and marital-status variables. Each control variable influences male participation the same way: namely blue-collar, unemployed and unmarried men are less active in both instrumental and expressive groups than white-collar, employed and married men. While the three variables influence (in the same directions) the female's activity with instrumental groups, they do not affect her ties with expressive voluntary groups. Apparently her accommodative social behavior is sufficient qualification for participation in expressive groups.

Kinship

A synthesis of U. S. kinship studies shows that adult females are more active in their kin networks than males (Adams, 1968:4,

¹⁸ Forty percent of the males and 39 percent of the females reported affiliation with one or more such groups. Women did not differ from males in their number of sexually exclusive group affiliations.

169). Our study, extending earlier research, examines the social participation patterns of people whose contact with kin is cut by distance, death or choice. When kin contacts drop to once or twice a year, their value to an individual is limited. Without kin to support, do women nurture friends?¹⁹ Do they join more groups to develop intimate ties? Do their interests turn to more instrumental concerns? On the other hand, do extensive kin relations interfere with cultivating friends and joining voluntary groups?²⁰

As anticipated, women reported more close kin than men (see Table 5). Advancing age, physical disability and occupational mobility accounted for most sex differences in kinship resources. Women under 65, whatever their marital status, reported more close kin than men. With advancing age the female's kin resources declined to a level comparable to the male's, which does not change with age.²¹ Perhaps this finding was a function of the kin with whom men and women have close ties.

Women maintain intimacy with parents

¹⁹ Litwak and Szelenyi (1969) cogently analyze the different types of support emanating from kin, neighbors and friends in a highly mobile society.

²⁰ The relationships could be stated in reverse; that is; what effects do limited numbers of friends or memberships have on ties with kin? But, because kin tend to be ascribed affiliation more than the other two modes of participation, we chose kinship resources as the point of departure.

²¹ The decline in the female's mean number of kin is significant at the .01 level.

Table 5. Close Kin Reported by Men and Women - Means and Standard Deviations.

	Males	Females	p ¹
No. of Close Kin	3.5(275) ² 2.4 ³	4.3(525) 2.6	<.01
Age			
45-64	3.5(190) 2.3	4.6(333) 2.4	<.001
65 +	3.4(84) 2.6	3.8(188) 2.4	-
Physical Disability			
None	3.5(199) 2.4	4.5(352) 2.6	<.01
Activities Constrained	3.6(75) 2.8	4.0(175) 2.4	<.25
Occupational Mobility			
Not mobile	3.4(157) 2.2	4.6(269) 2.6	<.01
Downwardly	3.3(32) 2.0	3.9(65) 2.6	-
Upwardly	4.0(81) 2.6	4.3(170) 2.4	-
Status			
Blue Collar	3.3(137) 2.2	4.2(230) 2.6	<.01
White Collar	3.8(138) 2.5	4.4(289) 2.4	

¹One way analyses of variances were computed to obtain the probability of the observed differences.

²The number of cases upon which the mean is based appears in parentheses.

³Standard deviations are in italics.

and siblings as well as their family of procreation; men tend to restrict primary kinship relations to their family of procreation (Adams, 1968:199; Babchuk and Ballweg, 1971; Habenstein and Coult, 1965:16-19). With advancing age many parents and siblings die, drastically reducing kinship resources for women.²² Kin in the family of procreation are mostly young; the death of offspring is uncommon. Thus, the aging male's more or less exclusive orientation to his family of procreation accounts for the stability in kin resources.

²² The female's ties to her family of orientation are her sole source of financial, health care and related long term aid in time of crises (Litwak and Szelenyi, 1969). If both husband and wife try to maintain close ties with their respective families of orientation, it would lead to invidious comparisons and competition which could threaten the marriage. Since the conjugal relationship is vital to child rearing, an intimate tie with only one set of parents preserves the species.

Physical disability tends to reduce the female's kinship resources to the male's level (see Table 5). Apparently women need good health to maintain their high level of kinship interaction. Finally, the number of close kin reported by women who have not experienced inter-generational mobility exceeds that of men in the same category. Mobility (upward or downward) tends to reduce the female's kinship ties to a level comparable to the male's. In short, the male maintains a fairly constant kin resource level in the face of age, disability, and occupational mobility; while the female cannot maintain her initial high level of participation.²³

Data indicate that a decline in kinship ties does not prompt an increase in friendship or group participation. Depending on the number of their kin confidants, persons were classified as rich or modest in kin resources.²⁴ In every case, men and women with rich kin resources reported richer interpersonal resources in other areas (see Table 6). They belonged to more groups and gave more time to them and they had more close friends. Moreover, a greater proportion of their friendships were characterized by high levels of affect; that is, spontaneous and confiding interaction. Thus, individuals rich in interpersonal resources in one area were apt to be rich in others.

Males rich in kinship ties tend to have more extensive associations in other areas than women in the same category. They report more friends and more group involvement. Females, on the other hand, report fewer friends, but engage in spontaneous and confiding activities more often. Males with rich resources seem moved to maintain other associations, but not at a qualitative level. Females rich in kin resources develop fewer but more affective close ties.

²³ Applying control variables of social class, marital status, residential mobility, ethnicity and membership in the labor force did not increase our ability to explain sex differences in kinship resources.

²⁴ Our choice of confiding activity as a measure of affect over spontaneous behavior was arbitrary, as both discriminated in the same way. Thus, those having three or more such ties were classified as having rich resources; three being the first whole number above the mean number of kin confidants reported by the respondents.

Table 6. Quality of Kinship Resources and Other Forms of Social Participation.

	Modest Kinship Resources		Rich Kinship Resources		^{p1} Kinship Resource Differences		^p Sex Differences	
	Male	Female	Male	Female	Male	Female	Modest Kinship Resources	Rich Kinship Resources
Hrs/Mo spent on voluntary association activities	7.5(162) ² <i>15.4³</i>	8.4(218) <i>18.7</i>	11.2(113) <i>19.1</i>	10.8(307) <i>21.7</i>	<.10	<.25	-	-
No. of voluntary association memberships	2.0(162) <i>2.1</i>	1.4(218) <i>1.7</i>	2.7(113) <i>2.7</i>	1.9(307) <i>2.0</i>	<.05	<.01	<.01	<.01
No. of close friends	3.4(162) <i>3.1</i>	3.1(218) <i>3.0</i>	4.7(113) <i>3.8</i>	3.9(307) <i>3.0</i>	<.01	<.01	-	<.05
Mean proportion of friends with whom they "do things on the spur of the moment"	39.1(162) <i>41.8</i>	50.1(218) <i>42.7</i>	55.7(113) <i>40.8</i>	66.2(307) <i>38.8</i>	<.01	<.01	<.05	<.05
Mean proportion of friends in whom they confide	31.6(162) <i>41.7</i>	36.2(218) <i>42.5</i>	48.8(113) <i>42.2</i>	64.7(307) <i>39.5</i>	<.01	<.01	-	<.01

¹One way analyses of variances were computed to obtain the probability of the observed differences.

²The number of cases upon which the mean is based appears in the parentheses.

³Standard deviations are in italics.

CONCLUSION

In some cases, the male's social participation exceeds the female's. In others, the female's is as great or greater. Extensive friendship ties for males are linked to early socialization, primarily competitive team activities which are not universal in the male population. Female friendships are affectively richer.

While men participate in more voluntary groups than women, they do not join sexually exclusive groups more often. Men participate more extensively in instrumental groups, women in expressive groups, even leading them more often than men. Furthermore, men exceed women only in number of affiliations and not time spent in them monthly. Like friendship, many differences in voluntary group participation can be linked to the early male socialization. However, discriminatory recruitment of women into menial jobs also contributes.

Females maintain more kinship ties than

males. Their ties limit their participation in other social relationships, particularly those calling for strong affective investments such as friendship. Women with modest kin commitments are comparable to men in other affiliations.

Thus, sex-related behavior modes account for only select participation differences; for example, for men, early training fosters social participation. There is little evidence that a biological tendency to aggress accounts for the male's different participation. Of course, incipient aggressiveness may be nurtured by society. However, the influence of biology on social behavior needs to be clarified by further research.

Tiger's thesis that male bonds are stronger than women's is not supported. Seldom were men's relations with friends and groups more numerous than women's. They did not exceed women's in sexually exclusive organizations; nor had they more same-sex friendships. Women's kinship ties exceed men's

clearly. Moreover, theirs are stronger than the men's. Women are more spontaneous with friends and kin (Adams, 1969:199), and devote more time to the voluntary organizations to which they belong. Women retain active membership longer than men (Babchuk and Booth, 1969); indicating a greater stability in their affiliations. Furthermore, the fact that they maintain a constant number of friendships through life, despite adversity, indicates that affiliative stability is not limited to voluntary associations.

These sex differences probably will diminish in the years to come as sex-role distinctions diminish. The advance in women's civil rights is signaling the change.

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RELIGION, ECONOMIC DEVELOPMENT AND LETHAL AGGRESSION *

HUGH P. WHITT

University of Nebraska

CHARLES C. GORDON

Carleton University, Ottawa

JOHN R. HOFLEY

University of Winnipeg

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In a cross-national sample, the relationship between economic development and lethal aggression varies by religious tradition. Aggression decreases with industrialization in Protestant nations, increases with industrialization in non-Christian nations, and is unrelated to industrialization in Catholic nations. Regardless of religious tradition, increasing industrialization is accompanied by an increasing tendency for lethal aggression to take the form of suicide rather than homicide. Protestant nations show a tendency to express aggression as suicide, while Catholic and non-Christian nations have a greater tendency toward homicide. An interpretation based on a two-factor model of suicide and homicide is suggested.

THIS paper examines cross-national variations in rates of suicide and homicide as joint functions of religious tradition and level of industrialization. We reanalyze a sample of nations originally investigated by Quinney (1965) using a two-factor model based on the distinction between (1) the total volume of aggression in a population, and (2) the direction of aggression.

Quinney discovered that suicide varies directly and homicide inversely with the industrialization level, concluding from these findings that the two forms of violence are "causally separate phenomena" which cannot both be explained by the same theory. This interpretation runs counter to several

closely related theoretical perspectives (Straus and Straus, 1953; Henry and Short, 1954; Gold, 1958; Wood, 1961; Palmer, 1965), linking suicide and homicide in the same causal nexus.

A MODEL OF SUICIDE AND HOMICIDE

Straus and Straus, Henry and Short, Gold, and Wood begin with the assumption that both suicide and homicide are caused by frustration, thus, implicitly or explicitly, conceptualizing suicide and homicide, as alternative aggressive responses to frustration. Which alternative will be chosen is then explained by such factors as external restraint (Henry and Short, 1954), looseness versus closeness of the social structure (Straus and Straus, 1953), physical versus psychological (love-oriented) discipline (Gold, 1958), or alienation from versus com-

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mitment to the societal value system (Wood, 1961). Studies based on these theories have been primarily concerned with factors influencing the direction of aggression, i.e., whether it is expressed as suicide or as homicide.

Palmer (1965) suggests that populations are subject to variations in those factors which produce violent aggression *as well as* those which influence its direction. Similarly, Gibbs (1968) points out that "it is improbable that progress will be made in examining the association between suicide and homicide without considering the source and total volume of aggression."

These considerations suggest that two sets of causes may operate jointly to produce observed rates of suicide and homicide, and that the relevant dependent variables for research may not be suicide and homicide rates themselves, but two synthetic variables corresponding to (1) the total volume of aggression, and (2) the direction of that aggression. The first measure, which we shall call the "lethal aggression rate" (LAR), is defined as the sum of the suicide rate and the homicide rate. The LAR represents a crude measure of the total volume of lethal aggression in a population.¹ Although a number of direction measures are available (Cf. Porterfield, 1949), we shall use Gold's (1958) suicide-murder ratio or SMR, defined as the ratio of the suicide rate to the combined rate of suicide and homicide. The SMR measures the probability that, in a given population, an act of lethal aggression will be expressed as suicide rather than homicide.

The model to be tested here predicts that the total volume of aggression, as measured by the LAR, is a function of one variable, X_1 , while the direction of aggression, as mea-

sured by the SMR, depends on another, X_2 . The above theories may be regarded as special cases of a more general formulation based on frustration-aggression theory (Dollard *et al.*, 1939; Berkowitz, 1962). In these terms, X_1 becomes any factor which increases frustration, while X_2 , following Berkowitz (1962:324, 326) is any factor which increases the probability that the self will be blamed for frustration and hence become the target of aggression. Symbolically, then, the model may be expressed as follows:

$$\text{LAR} = S + H \quad (1)$$

$$\text{SMR} = \frac{S}{S + H} \quad (2)$$

$$\text{LAR} = f(X_1) \quad (3)$$

$$\text{SMR} = f(X_2) \quad (4)$$

where LAR = the lethal aggression rate,
 SMR = the suicide-murder ratio,
 S = the suicide rate,
 H = the homicide rate,
 X_1 = any factor which increases frustration, and
 X_2 = any factor which increases self-blame for frustration.

A DERIVATION FROM THE MODEL

Although variables such as frustration are extremely difficult, if not impossible, to measure adequately across socio-cultural systems, Palmer (1965) suggests that violent aggressive drives in populations are produced by "excessive psychological frustrations engendered by conflicts in roles and in cultures." Thus, X_1 might be measured cross-nationally in terms of role and culture conflicts.

Every socio-cultural system contains countless elements, each of which may be congruent, neutral, or contradictory relative to every other element. To first approximate the cross-national measurement of the frustration source, some simplification is necessary. A promising lead toward a two-element measurement of culture conflict is provided by Weber's (1958, 1963) far-reaching investigations of the interrelations between religion and economic activity. Weber (1958) maintains that, of the world religions, only ascetic Protestantism is fully congruent with

¹The LAR is an incomplete measure of deaths from violent aggression. A more complete measure might be constructed by including deaths from riots, revolutions, etc. However, the addition to the LAR which can be made by including these is minimal. According to Russett (1964:99-100), the highest death rate from domestic group violence (1950-1962) among the countries in our sample was in Hungary (10.3 per 100,000 per year), probably because of the revolution of 1956. In no other country in the sample does the inclusion of deaths from domestic group violence make a major difference.

the values implied by the modern urban-industrial complex. Both in its ethical prescriptions and in the psychological effects of its soteriology, Protestantism actively pushes men into economic pursuits. At the opposite extreme, implying values strongly contradictory to economic activity, are the Eastern religions, whose ethical and psychological consequences are escape from the institutional order, accommodation to the traditional system as eternally given, or the siphoning of worldly activity into non-economic spheres such as the pursuit of sacred knowledge or world conquest. Thus, for Weber, Protestantism is integrated with economic development, while Buddhism, Hinduism, Taoism, Confucianism, and Islam contradict economic growth, placing active "impediments" (Weber, 1963:269) in its path.

Unlike the Eastern religions, Catholicism does not strongly contradict economic pursuits; it is not, like Buddhism, actively hostile to the things of the world, nor does it, like Confucianism, channel activity away from the economic sphere. However, it fails to stimulate economic pursuits as does Protestantism (Parsons, 1949:517). While permitting economic activity, it remains suspicious. Thus, for the present argument we conclude that Catholicism and economic development are neutrally related.

If the Weber argument is correct, the following hypotheses about the relationship between the LAR and level of industrialization are implied: (1) In Protestant nations, the higher the level of industrialization, the less the frustration produced by contradictions between the religious and economic spheres of the socio-cultural system. (2) In non-Christian societies, the greater the industrialization, the greater the conflict between religious and economic sectors of the system. (3) In Catholic nations, the industrialization level and the degree of conflict between religion and economy should not be related. (4) Since culture conflict is expected to increase the lethal aggression rate, the relationship between the LAR and the industrialization level should be inverse in Protestant nations, direct in non-Christian nations, and non-existent in Catholic nations.

Turning to the direction of aggression, we see that the model predicts that the

suicide-murder ratio should vary directly with the extent to which the self is viewed as responsible for frustration. Self-blame for frustration is probably at a maximum in the absence of strong external behavior restraints (Henry and Short, 1954). As Berkowitz (1962) suggests, persons "who are the masters of their own fate" *must* blame themselves for their failures and frustrations, but those whose fate is controlled by strong external restraints over behavior can assign the blame for frustration to sources other than self.

Both religion and industrialization are undoubtedly linked to the degree of external restraint. In the case of religion, both Weber (1958) and Durkheim (1897) noted a tendency for Protestantism to produce individual initiative and personal autonomy. Coupled with this is the Protestant emphasis on independence training (McClelland; 1961; Lenski, 1963) and the greater tendency of Catholics to use physical rather than psychological punishments. Thus, whether we employ Henry and Short's (1954) external restraint or Gold's (1958) physical versus psychological punishments as an explanatory principle, we arrive at the hypothesis that the SMR should be higher in Protestant than in Catholic nations. Due to the heterogeneity of non-Christian perspectives, no clear-cut predictions of the SMR's of non-Christian nations will be offered, but we expect them to resemble those of Catholic nations.

Turning to the industrialization effects on the direction of aggression, several components of *increasing societal scale* point to a lessening of external restraint. Weakened kinship ties, increased social and geographic mobility, and similar processes clearly point to a decreased intensity in relation systems. The division of labor opens alternative life styles to individuals, decreasing external behavior restraints. Technological innovations increase control over the environment; and man's outcomes, less subject to unpredictable variations, can more easily be linked to his efforts.

Although this argument is admittedly tenuous,² it leads to the prediction that, with

² Some segments of industrial societies (e.g., blacks in the United States) no doubt experience considerable external restraint. The very low SMR's

religion controlled, the SMR varies directly with the degree of industrialization.

PROCEDURE

Table 1 shows the religious traditions, industrialization levels, SMR's, and LAR's of forty-seven nations. Measures of religious tradition were obtained from Coxill and Grubb's (1962) *World Christian Handbook*.³ The data are nominal, with the categorization based on the trichotomous allocation of societies by modal religious category—Protestant, Catholic,⁴ or non-Christian.

Figures on industrialization are drawn from United Nations sources (FAO, 1957). This variable is measured by the proportion of economically active males not engaged in agriculture *ca.* 1957, the midpoint of Quinney's suicide and homicide series.

Lethal Aggression Rates and Suicide-Murder Ratios were calculated from Quinney's (1965) data according to equations (1) and (2) above.⁵

of American blacks, however, is consistent with the perspective of the model.

³ We are not comfortable with this source, but it is the best we can do. As far as we have been able to determine, there is *no* reliable cross-national tabulation of religious adherence. The United Nations (1964, 1965) occasionally publishes data from national religious censuses, but coverage is spotty and data collection procedures are not standardized across nations. Even if the UN data were complete and reliable, the tabulation procedures followed do not permit us to disentangle Protestants and Catholics, who are lumped together in a single category. A second possible source of religious data is the *World Handbook of Political and Social Indicators* (Russett *et al.*, 1964), but this source not infrequently assigns a nation more Roman Catholics than Christians. Coxill and Grubb at least give us some sort of estimate, by nation, of the population of each religious body.

⁴ Greece, whose modal religious affiliation is Greek Orthodox, is included with Roman Catholic nations.

⁵ The suicide and homicide data on which these measures are based are crude rates per 100,000 population per year for the period 1953-1960. These rates are not standardized for either age or sex, but there is reason to suspect that standardization might introduce more bias than it would eliminate. As Quinney (1965) notes, UN data on homicides are for victims rather than for those who commit homicide. Thus, we have no way of knowing the age and sex distribution of offenders; and standardization becomes, strictly speaking, meaningless.

Table 1. Religion, Economic Development, the SMR and the LAR in 47 Countries

Religious Tradition: Nation	% of Economically Active Males Not Engaged in Agriculture circa 1957	SMR	LAR
Protestant:			
England & Wales	95	.943	12.3
Scotland	93	.920	8.8
Australia	88	.882	12.7
United States	87	.691	14.9
W. Germany	85	.949	19.8
Switzerland	84	.963	19.1
N. Ireland	84	.881	4.2
New Zealand	81	.932	10.3
Norway	81	.937	7.9
Iceland	80	.920	10.0
South Africa	78	.517	15.3
Denmark	76	.969	22.4
Sweden	76	.959	19.3
Finland	66	.894	23.5
Catholic:			
Belgium	87	.885	15.1
Netherlands	86	.942	6.9
Canada	84	.862	8.7
Uruguay	81	.698	15.9
Austria	78	.952	24.8
France	75	.911	18.1
Trinidad & Tobago	75	.414	9.9
Puerto Rico	63	.660	15.9
Chile	62	.487	7.6
Venezuela	60	.451	13.3
Portugal	58	.882	10.2
Italy	56	.810	7.9
Dominican Republic	55	.338	7.1
Poland	53	.803	7.6
Spain	52	.768	6.9
Panama	51	.356	9.0
Hungary	51	.890	25.5
Ireland	51	.897	2.9
Greece	48	.667	5.4
Colombia	47	.046	41.0
Costa Rica	45	.406	6.4
Paraguay	44	.193	8.3
Mexico	42	.046	34.5
Guatemala	38	.265	11.7
El Salvador	37	.256	44.1
Nicaragua	32	.167	7.2
Philippines	31	.200	4.0
Non-Christian:			
Singapore	94	.822	11.8
Israel	82	.429	17.7
Japan	56	.923	25.9
Ceylon	47	.672	12.2
United Arab Rep.	35	.020	4.9
Thailand	34	.219	13.7

FINDINGS

Table 2 summarizes the data in Table 1. Clearly, Protestant nations tend to be more

Table 2. Means, Standard Deviations, and Pearson Product - Moment Correlations of all Variables, by Religious Tradition.

Religious Tradition: Variable	Mean	Standard Deviation	Correlation Matrix				
			X	Y ₁	Y ₂	Y ₃	Y ₄
Protestant:							
(X) Industrialization	.824	.074	-	-.467	-.192	-.529	.059
(Y ₁) Suicide Rate	12.7	5.80		-	-.148	.941	.375
(Y ₂) Homicide Rate	1.64	1.99			-	.194	-.968
(Y ₃) LAR	14.3	5.85				-	.041
(Y ₄) SMR	.883	.126					-
Catholic:							
(X) Industrialization	.571	.166	-	.504	-.394	-.088	.684
(Y ₁) Suicide Rate	7.01	6.20		-	-.204	.372	.637
(Y ₂) Homicide Rate	6.91	10.39			-	.833	-.668
(Y ₃) LAR	13.9	10.96				-	-.273
(Y ₄) SMR	.568	.309					-
Non-Christian:							
(X) Industrialization	.580	.249	-	.314	-.208	.255	.571
(Y ₁) Suicide Rate	8.77	8.25		-	-.531	.883	.854
(Y ₂) Homicide Rate	5.60	3.88			-	.655	-.632
(Y ₃) LAR	14.4	7.01				-	-.071
(Y ₄) SMR	.516	.355					-

highly industrialized than either Catholic or non-Christian countries. Protestant nations tend to have somewhat higher suicide rates and lower rates of homicide. Though there is little difference between nations adopting the three religious traditions in mean LAR, the SMR's of Protestant nations are higher than those of Catholic and non-Christian nations.

Turning to the correlation matrices in Table 2, we find that suicide and homicide vary inversely with one another in all three subsamples, and that, in Catholic and non-Christian nations, they vary with industrialization in opposite ways, suicide increasing and homicide decreasing with industrialization. However, in the Protestant subsample, the correlation between suicide rates and industrialization shifts from positive to negative. Thus, in Protestant nations, both suicide and homicide decrease with increasing industrialization.

How well do these findings fit our theoretical perspective? Looking first at the total volume of aggression, predicted relationships between the LAR and industrialization in Protestant, Catholic, and non-Christian nations are confirmed. Table 3 shows the slopes⁶ of the LAR on indus-

trialization, for each of the three subsamples. For Protestant nations, the slope is negative and quite steep, ($b = -41.67$, $r = -.53$). In the non-Christian subsample, the slope is low but positive ($b = 7.15$, $r = .26$); and in Catholic nations, the association between the LAR and industrialization is effectively non-existent ($b = -5.83$, $r = -.09$).

Because of the small number of countries involved, only the Protestant correlation is significant. We expected the failure to reach significance with the Catholic subsample, since Catholicism was assumed to be neither strongly congruent nor strongly incongruent with industrialization. The fact that almost no relationship exists between the LAR and industrialization in the Catholic subsample supports the notion that, across the array of nations, Catholicism and industrialization are neutrally related.

The nature of our hypotheses considerably complicates further analysis of the relationship between the LAR and religion and industrialization. Our argument hinges on the presence of statistical interaction between

text indicate the expected change in the dependent variable accompanying a shift from a completely agricultural economy (100 percent of the economically active males engaged in agriculture) to a completely industrial system (none of them engaged in agriculture).

⁶ The slopes (b's) reported in the tables and

Table 3. Slopes of the LAR on Industrialization within Religious Traditions

Religious Tradition	Slope (b)	Correlation (r)
Protestant (N=14)	-41.67*	-.53*
Catholic (N=27)	-5.83	-.09
Non-Christian (N=6)	7.15	.26

*Significant at .05 level.

religion and industrialization. In testing for interaction using analysis of covariance techniques, one compares the sum of squares explained by allowing the subsample slopes to operate with the sum of squares explained by using a single slope (the average within-class slope) in all three subsamples. Interaction is thus measured by the reduction in error (unexplained sum of squares) obtained by using three slopes instead of one. Since our hypothesis calls for a very weak relationship in the Catholic subsample, we have stacked the deck against finding significant interaction. The Catholic subsample is the largest of the three and contains the greater part of the total sum of squares in the dependent variable. If our hypothesis for Catholic countries is correct, as it appears to be, then we will gain little by using the individual within-class slope rather than the average within-class slope. Either way, very little of the sum of squares in the LAR will be explained in the Catholic subsample. We would have to gain a great deal in the other two subsamples for interaction to be significant.

As expected, interaction among the three subsamples fails to reach significance. However, when two-sample tests of the significance of the difference between correlations were computed for each pair of correlations, results were somewhat more satisfactory. For the Protestant-Catholic comparison $p = .085$, using a one-tailed test, and for the Protestant-non-Christian comparison $p = .097$; the Catholic-non-Christian comparison finds $p = .288$. Thus, two of the three comparisons approach acceptable levels of significance.

At the risk of trying to make a silk purse of a sow's ear, let us assume for the moment that the correlations in Table 3 represent true values, with a negative correlation in Protestant nations, a zero correlation in Catholic countries, and a positive correlation in non-Christian countries. The probability that this would occur by chance is one in twenty-seven (three cubed), or less than .037.⁷ Thus, the *pattern* of the findings is consistent with our hypotheses. This pattern suggests that our major problem may be small sample size. Tests of the hypotheses on units smaller than nations would be desirable, but the data are not presently available for such tests.

Our hypotheses for the lethal aggression rate were based on the prediction of statistical interaction between religion and industrialization. By contrast, our predictions for the suicide-murder ratio employ an *additive* model. The model predicts a positive relationship between the SMR and industrialization within each religious category, with an additive effect of religion operating in such a way that, with industrialization held constant, the SMR should be highest in Protestant countries and somewhat lower in Catholic and non-Christian nations.

As Table 4 shows, the relationship of the SMR to industrialization is positive for all groups of nations, by religious tradition. The relationship is quite weak in the Protestant subsample, nearly all of which are highly industrialized ($b = .10$, $r = .06$). In the other

⁷ This assumes that each slope may take positive, negative, or zero values, and that the chance probability of each is identical.

Table 4. Slopes of the SMR on Industrialization within Religious Traditions.

Religious Tradition	Slope (b)	Correlation (r)
Protestant (N=14)	.10	.06
Catholic (N=27)	1.28*	.68*
Non-Christian (N=6)	.81	.57

*Significant at .001 level.

two subsamples, the relationship is much stronger. In Catholic societies, the slope is extremely strong ($b=1.28$, $r=.68$); while the figures for non-Christians are $b=.81$ and $r=.57$.

To evaluate the significance of the relationship between industrialization and the SMR for the full sample, controlling for religion, an analysis of variance test for the significance of the average within-class correlation was performed. The average within-class slope is 1.073, with an associated average within-class correlation of .610. In the analysis of variance test, $F_{2,43}=25.42$ ($p<.001$). With religion controlled, then, the SMR varies directly with industrialization, as expected.

Our hypotheses predict a relationship between religious tradition and the SMR with industrialization controlled such that the SMR should be higher in Protestant than in Catholic and non-Christian countries. In effect, this is the equivalent of the hypothesis that the mean of the SMR, when adjusted through analysis of covariance techniques to remove the effects of industrialization,⁸ should be higher in Protestant nations than in the other two subsamples. Because interaction is not significant, we are justified in using this adjustment procedure. Table 5 shows the adjusted and raw means of the SMR for each religious category. The data indicate that when industrialization is not controlled, there is an appreciable difference between the mean SMR of Protestant countries and those of Catholic and non-Christian countries. While some 88 percent of the lethal aggression in the average Pro-

testant nation is expressed as suicide rather than homicide, the comparable figures for Catholic and non-Christian nations are 57 percent and 51 percent, respectively. Part of this difference is due to the high level of industrialization in Protestant nations, but the pattern does not disappear entirely when industrialization is controlled. After adjusting for industrialization, the Protestant SMR is .69, as compared with .54 for the Catholic subsample and .59 for non-Christian countries. The mean SMR for Protestant countries before adjusting for industrialization is significantly higher than that of Catholic ($p<.001$) and non-Christian ($p<.05$) nations, but the difference between the mean SMR's of Catholic and non-Christian countries is not significant. After adjusting for industrialization, only the Protestant-Catholic difference is significant ($p<.05$).

DISCUSSION

This paper attempts to do two things. First, it makes explicit a formulation implicit in existing theories of suicide and homicide by constructing a model which subsumes as special cases the theories of Straus and Straus (1953), Henry and Short (1954), Gold (1958), and Wood (1961). Second, it tests a series of hypotheses relating religion and industrialization to the amount and direction of aggression in the context of that model.

The results of our research are disappointing, but at the same time promising. Although many of our findings fail to reach statistical significance, the patterning of the results supports our derivations from the model. In terms of the effects of religion, industrialization tends to insulate populations against lethal aggression in the presence of a Protestant religious tradition, but this is not the case in Catholic or non-Christian nations. Our data make it reasonably clear that Protestantism leads to a greater tendency for the "choice between suicide and homicide" to be resolved in favor of the former. It also appears that the direction of aggression shifts toward suicide and away from homicide as industrialization progresses, at least in non-Protestant nations. The extremely small slope in Protestant nations may be due to the relatively homo-

⁸ See Blalock (1960) for this adjustment procedure.

Table 5. Mean Values of the SMR by Religious Tradition

Religious Tradition	Mean of the SMR	
	Unadjusted	Adjusted for Industrialization
Protestant	.883	.692
Catholic	.568	.543
Non-Christian	.516	.589

geneous composition of this subsample, or; possibly, to the effects of factors which come into play only at *very* high levels of industrialization. Marcuse (1964) suggests that in a technological system run rampant new forms of control begin to restrict the individual; Riesman (1961) speaks of a shift at very high levels of industrialization from the "entrepreneurial" ethic of inner-direction toward a "bureaucratic" ethic of "other-direction"; and the homogenization and standardization implied by "mass society" literature points to a lessening of individual control over the determination of one's goals. As Stein (1964) puts it, "in the pendular movement from over-integration to over-individuation, the balance seems to have swung from the latter to the former." Such observations suggest that in very highly developed societies, the trend toward increasing internalization of aggression may be reversed. The relatively low suicide-murder ratio of the United States is consistent with this perspective.

Both our findings and interpretations are weakened by the crudeness of our data. Subsamples are small, and the religious and industrialization categories are crude. Future research might well profit by refining the religion measurement, perhaps by including percent Catholic or percent Protestant as a variable, or by looking at religiously homogeneous versus religiously mixed nations. Eisenstadt (1968), in his critique of Weber, notes that the transformative capacity of Protestantism has been weakest in those areas in which Protestant bodies have been either totalistic state religions or oppressed minorities and stronger when Protestantism dominates "secondary elites" not actually in power but having access to and influence on primary elites.

Alternative measurements of economic development, such as per capita income (Clark, 1957) and electrical productivity (McClelland, 1961) are available, as are a number of indexes of development using more than one indicator (Cf. Marsh, 1967; McGranahan, 1971). Non-agricultural employment is used here for two reasons—availability and continuity. Figures on non-agricultural employment are available for all countries in Quinney's sample except Jordan, and the use of a similar measure of industrialization

provides continuity with his earlier study. Moreover, McGranahan (1971), whose index includes eighteen "core" indicators, notes that the average correlation of non-agricultural employment with all other core indicators is quite high ($r = .809$).

Our findings are, of course, ecological. Within any given nation, various regions or sectors of the economy are differentially located with respect to the impact of industrialization. The impacts of industrialization on the LAR and SMR may vary, not only by the religious composition of nations, but also according to the distribution of religious groups across economic sectors within nations.

Another point of difficulty revolves around the articulation of our research hypotheses with the model itself. Weber's notions concerning the relationship of religion and economy have been seriously questioned (for a scathing attack on Weber, see Samuelsson, 1961; for a more balanced recent treatment see Eisenstadt, 1968). Like Weber, we lumped all non-Christian religions together in a single category and assumed their incompatibility with industrialization. The non-Christian religions, however, are a heterogeneous lot; and some (Cf. Bellah, 1957, 1963) may be congruent with industrialization. Similarly, variations of both Protestantism and Catholicism may be differentially congruent with industrialization (see e.g., Vallier, 1970). Weber (1958) noted that congruence with the value elements of development is greater in Calvinist than in Lutheran and Anglican Protestantism.

While this report raises more questions than it answers, the findings are encouraging both in terms of the general model and our derivations from it. If we are correct, future research will show that suicide and homicide are not causally separate, but closely connected.

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DEVELOPMENTAL CHANGE IN THE SENTIMENT STRUCTURE OF CHILDREN'S GROUPS *

SAMUEL LEINHARDT
Carnegie-Mellon University

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Current theories of cognitive development in children and those of structure in small-scale social systems when jointly considered suggest that the social organization of children's peer groups will demonstrate developmental trends. This inference is tested by measuring transitive organization in 118 positive affect sociograms of children's classroom groups and regressing these measurements on an age variable and other variables associated with the sociometric data. The analyses indicate that a statistically significant positive association exists between transitive organization and age and that this association is independent of variation in choices, made and received per group member, pair proportions, group size or sex composition.

THE study of children's interpersonal relations has been dominated by a theoretical orientation in which personal traits are presumed to be the prime behavior determinants. Hence, developmental studies of the behavior of children in groups typically test hypotheses about children's social presentations or social perceptions rather than inferences about social relations among children. Such designs are clearly evident in the sociometric studies of "stars" and "isolates" and of sociometric status in general (Moreno, 1953), and in the more recent experimental studies of cognitive consistency in children (Feffer, 1970; DeSoto, 1959; DeSoto and Kuethe, 1960; Atwood, 1969). Indeed, what little study has been made of developmental changes in the social organization of children's groups has been confined, largely, to the development of sex roles, as in older children's tendency to prefer members of their own sex (Moreno, 1953; Gronlund, 1959; Campbell, 1964). Thus, while the terms "social structure" and "social system" often appear in discussions of children's groups, little is known of these small-scale social systems nor what effect developmental trends have on how they structure interpersonal relations.

My purpose is to examine the development of social organization in children's groups by using a mathematical model of interpersonal sentiment structure in a procedure which facilitates the observation of change in the structure of sociometric data. The model is a transitive graph (t-graph), a general model of the structure of positive interpersonal sentiment proposed by Holland and Leinhardt (1970, 1971). T-graphs are simply directed graphs of transitive (and reflexive¹) relations. In the social structure modeled by t-graphs both cliquing and hierarchy are expressions of one social organization principle, the transitivity of positive interpersonal sentiment.

The original hypothesis of the transitivity of positive interpersonal sentiment is Heider's (1946, 1957) and his "p-o-x" model of cognitive consistency is an enumeration of transitive triples. However, he was primarily concerned with psychological states and he examined the social structure consequences of his balance theory only briefly. Some of the social organization properties of t-graphs are discussed in Harary, Norman and Cartwright (1965) and Flament (1963).

The t-graph model has several features suitable to a study of children's social relations. First, its general nature requires

* The data analyzed in this report were supplied by Dr. Mary L. Northway under whose guidance they were collected at the Institute for Child Study, University of Toronto. Gaea Leinhardt, James A. Davis and Lester B. Lave provided advice and support. A special debt of gratitude is owed to Paul W. Holland for his critical review of an earlier draft.

¹ The reflexive condition is required to avoid trivial mathematical complications. No substantive weight is afforded to it. Transitivity is the assertion that, if $A \rightarrow B$ and $B \rightarrow C$ for some relation, r , then $A \rightarrow C$.

that a digraph be a t-graph if any of several other models of small-scale social structure are to obtain. Structural balance (Cartwright and Harary, 1956), clustering (Davis, 1967), ranked clusters (Davis and Leinhardt, 1971) and tournaments (Landau, 1951, 1952, 1953), for example, are special cases of t-graphs and obtain only when conditions *in addition* to transitivity are satisfied. Second, a standardized measure for t-graphs exists which permits assessing the statistical significance of tendencies toward transitivity in sociometric data and comparing these tendencies across different groups. Third, data on 752 sociograms presented by Davis (1970) and reanalyzed in Holland and Leinhardt (1971) and in Davis, Holland and Leinhardt (1971) indicate that the t-graph model can predict tendencies in the empirical organization of interpersonal sentiments. Recent results in Holland and Leinhardt (1972) corroborate these findings. Finally, the emphasis which the t-graph model places on transitivity as the prime organization principle in informal social relations connects current theories of children's cognitive development to those of small-scale social system organization.

This connection is crucial here. The analysis of structural development in interpersonal relations focuses on the organization differences which distinguish young children's and older children's sentiment networks. The working assumption is that the relations between individuals depend on the structural tendencies of the group's social system. This emphasis on structural constraints over individual attributes, normally assumes nothing about variations in the personal traits of group members. It assumes that group members can operate within the behavioral constraints imposed by the structural logic of the social system. If transitivity is a constraint, then the individuals must be able to respond to complex prompting situations which involve the behavior of others and behave so as to render the situation transitive. One is usually justified in granting this capacity to adults. With them and older children transitive logic is commonplace. Among the very young, it cannot be so easily assumed since there seems to be a relationship between age and the ability to exhibit transitive judgmental

logic (Piaget, 1966). This relationship suggests that if interpersonal sentiment relations are transitive, their organization in children's groups will exhibit developmental trends.²

A Structural Approach to Sociometric Analysis

Holland and Leinhardt propose a complex technique for testing transitivity in sociometric data. They discuss the t-graph model at length (1971), present formulae for the relevant statistics (1970), and Leinhardt describes a program for carrying out the necessary computations (1971). Consequently, only a brief review is provided here.

Essentially, a group is conceived of as a set of individuals connected by a certain pattern of relations. A sociogram generated by responses to a "liking criterion" is considered to be a graph of a group's positive interpersonal sentiment network. Thus, whatever the graph's structural properties, they are assumed to express the social system's organizational characteristics. If an informal social system's positive choice relation is transitive then, by definition, the social system is a t-graph and whatever organizational properties hold for t-graphs also hold for the group's social structure. The mathematical nature of the t-graph model allows these properties to be made clear and permits straightforward exploration of their implications. The two sociologically relevant properties of t-graphs are (1) equivalence classes and (2) ordering relations.

In sociograms that are t-graphs, mutual relations, situations in which both members of a pair choose one another, define equivalence classes. Transitivity guarantees that if *x* and *y* are linked to one another by a

² For evidence that the developmental and the social structural notions of transitivity are similar see the work of Atwood, 1969, DeSoto, 1959, DeSoto and Kueth, 1960, and Piaget, 1966. Although a controversy exists concerning the specific age at which children demonstrate transitive logic, there is general agreement that *whatever* the manner of testing transitivity, children of some age will fail to demonstrate the trait while older children will succeed. See for example: Glick and Wapner, 1968; Braire, 1964; Smedslund, 1965, 1963; and Wallace, 1967.

mutual relation and x is similarly linked to a third individual, z , then y must also be linked to z by a mutual relation. Similarly, if one of the pair is not linked to a third by a mutual relation neither can the other be. Thus, mutual relations can be interpreted as defining subgroups or cliques of group members who mutually like one another. This interpretation of cliquing accords with most models or theories of subgroup formation (cf., Homans, 1950; Brown 1965) and sociometric studies of classroom social behavior (Gildewell, *et al.*, 1966).

Unreciprocated liking or asymmetric pairs, on the other hand, define ordering relations such that cliques connected by asymmetric pairs are ordered by them into a consistent hierarchical system. Again, transitivity guarantees that asymmetric relations which pass from one clique's members to another's will all go in the same direction and will pass from every member of one clique to every member of the other. Cliques need not be connected by asymmetric relations, but if they are, the connecting relations must conform with the requirements of transitivity. Asymmetry plays a purely mathematical role as an ordering relation in the model. However, its role in status hierarchies is supported by theoretical writing on social power, stratification, dominance and sociometry. The model leads in a ranking situation to differential popularity and a tournament hierarchy, both common definitions of informal social status.

The t -graph model is operationalized in terms of triads, unordered triples of directed relations. There are sixteen essentially different (nonisomorphic) triad classes; and Holland and Leinhardt (1971) show that graphs which lack seven triads containing intransitive triples are t -graphs, a condition which obtains whatever other triads occur in the graphs.

Although the t -graph is not an extraordinarily restrictive model (it does not constrain the size, number and arrangement of its subcomponents) it nonetheless imposes exact constraints on a graph. A group's informal social system, even an adult group's, would not be apt to conform precisely to the structure of a deterministically stated t -

graph model. Several factors would detract from perfection. First, a group may be in the process of short-term development. Change may occur in relatively stabilized social structures with members altering alliances, friendships and feelings of hostility from time to time. Second, roles such as liaison positions (Harary, 1965) while inherently intransitive may be so advantageous that they occur despite this incongruity. Third, a very transitive group and one much less transitive may differ qualitatively in that the former may be under less pressure to become transitive than the latter. Even without these perturbing effects on transitivity, still another argument against a deterministic model is the sheer magnitude of its demands on group members. Since the number of a graph's triads rises nearly with the cube of the number of group members (a twenty person group possesses 1140 triads), it is intuitively unrealistic to expect empirical sociograms of more than a few individuals to satisfy a t -graph's conditions.

Thus, the study of empirical sentiment networks must be couched in terms which permit group structural tendencies to be measured and compared. For such comparisons to be fruitful, one should be able to make them despite the differences between other variables associated with the sociometric situations, such as group size. Holland and Leinhardt (1970) have addressed this problem and present equations for calculating expectations and variances for a transitive tendency measure based on the infrequency of intransitive triads. They develop a standardized measure which permits determining whether an empirical sociogram contains fewer intransitive triads than a graph of equal size with the same pair frequencies but which has had these pairs placed on the points "at random." They call their measure τ , where

$$(1) \quad \tau = \frac{T - \mu}{\sigma}.$$

In equation (1), the numerator on the right is the difference between the total number of observed intransitive triads in a sociogram, T , and their expected number in a random graph, μ . The denominator is the standard deviation. This index is a minimum

when the choice relation is transitive and has a mean of zero and variance one under the null hypothesis that the sociogram is random. Because it is in standardized form, approximate significance levels for an observed value of τ may be obtained by referring it to a table of the percentage points of the normal distribution.⁸ The appropriate test is one-sided and rejection of randomness in favor of the transitivity model is indicated by sufficiently large negative values of τ .

In this investigation, a more sensitive measure of transitivity, τ^* , is applied (Holland and Leinhardt, 1972). The index τ gives equal weight to triads containing one, two or three intransitivities. The hypothesis it tests, thus, is that intransitive triads will be rare. The measure τ^* is generated by weighting each intransitive triad by the number of intransitivities it contains. Rather than count only the intransitive triads, this procedure counts all the intransitivities in an observed graph. The distinction made by counting intransitivities rather than intransitive triads amounts to assuming that the more intransitivities a triad contains the more unlikely it is to appear. Since for any one individual a triad can be intransitive in only one way, a triad containing more than one intransitivity has more individuals in intransitive situations. By counting all intransitivities, more information contained in the sociogram is used and sensitivity to the transitivity hypothesis is increased. However, the two measures, τ and τ^* can be expected to behave similarly. The alterations which must be made on the probabilities presented in Holland and Leinhardt (1970) are minor as indicated below.

Applying this procedure of counting all intransitivities, the measure becomes:

$$(2) \quad \tau^* = \frac{T^* - \mu^*}{\sigma}$$

where T^* is now the total number of observed intransitivities, μ^* is the expected number of intransitivities in a random

graph, and σ^* is the standard deviation.⁴ The comments made above in reference to the normality of the distribution of τ apply to τ^* .

DATA

The data consist of 135 sociomatrices of children's classroom groups collected at the University of Toronto's Institute for Child Study. These sociomatrices cover a fourteen year span, 1953-1967, during which sociometric tests were administered in each class under a procedure which allowed up to three choices per child on three liking or positive sentiment criteria. The questions were as follows: 1) Who would you like to play with inside (the building)? 2) Who would you like to play with outside? and 3) Who would you like to sit next to during music class? Nine grades are represented, from junior nursery (minimum age of 2:9, i.e., two years nine months; maximum 3:8) through sixth grade with yearly intervals. While the data span a fourteen year period, sociomatrices for all nine grades exist only after 1957.

$$^4 T^* = \sum I_i T_i \quad \text{Where:}$$

i is an index from 1 to 7 for the intransitive triads;

T_i is the number of observed triads of type i ; and

I_i is the number of intransitivities in a type i triad

$$\mu^* = \sum_i I_i E(T_i) \quad \text{where: } E(T_i) \text{ is the number of triads of type } i \text{ expected in a random graph.}$$

$$\sigma^* = \sqrt{\sum_i I_i^2 \text{var}(T_i) + 2 \sum_{i < j} I_i I_j \text{Cov}(T_i T_j)}$$

where:

$\text{Var}(T_i)$ is the variance of triad type i ;

$\text{Cov}(T_i, T_j)$ is the covariance of triad type i and type j ;

$I_i I_j$ is the product of the intransitivities of triads type i and type j . The number of intransitivities by triad type and their index values are:

Triad Type†	030C	201	120C	021C	111U	111D	210
Intransitivities	3	2	2	1	1	1	1
Index	1	2	3	4	5	6	7

† Following Holland and Leinhardt, 1970.

⁸ Holland and Leinhardt (1970) report the results of a simulation study designed to test the reasonableness of assuming that τ is normally distributed. This study involved analyzing 2700 computer generated random groups and indicates that the assumption of normality as warranted.

The children who attended the Institute's school during this period were drawn predominantly from families associated with the University of Toronto and lived, for the most part, in its immediate neighborhood. Thus, the data are a sample of children drawn from a white, middle class Canadian population, and several of the children reappear as they move sequentially through the grades. Although some children are present in many sociograms, whole classes are never consistent; and our analysis is not meant as a longitudinal study. Nor does it depend on the existence of a panel. The approach is cross-sectional with grade level the independent variable, but the technique yields the same general results as a longitudinal study.

Of the 135 sociomatrices, 118 were used in the analysis. The seventeen sociomatrices excluded contained classrooms where grades had been amalgamated; the number of individuals in the group fell below six (a feature which would yield a highly discontinuous sampling distribution for τ^* and invalidate the normal distribution approximation), or the responses were obscured and not machine codable. Attrition in the later years occurred from a tendency of parents to send older children to public schools. To avoid having too few sociograms from different grades in the upper grade levels, the fifth and sixth grades were

pooled, and amalgamated classrooms in these categories were permitted. Table 1 presents a summary description of the sociometric data. Sample sizes for each of the nine grades appear in its second row.

Attrition appears in the third row, "average group size." Group size remained constant (about twenty) through kindergarten and then fell (to about twelve) in the third grade, remaining constant thereafter.

Young children, who could not respond to sociometric questionnaires, were surveyed personally by teachers; while older children filled in distributed forms. A maximum of three choices were permitted but not required for each question. Although nine choices were possible, often a child chose the same individual more than once. Since fine distinctions between sentiment were irrelevant, no distinctions between criteria were made. All multiple choices by one child for another were recorded as a single choice. Row 4 of Table 1 gives the average number of choices of different individuals per individual by grade. The average size of the groups tended to decrease with grade ($\rho = -.61$). The average number of choices made rose from a low of 1.62 in junior nursery to about 4 in kindergarten remaining between 4 and 5 thereafter ($\rho = .46$). The average number of choices to different individuals by each child in the entire sample was 3.98.

Table 1. Characteristics of 118 Sociograms for Which Transitive Organization was Measured.

Grade Level ¹	J	S	K	1	2	3	4	5-6 ²	Σ
Official Age ³	3.2	4.2	5.2	6.2	7.2	8.2	9.2	10.7	
No. of Sociomatrices	10	9	23	23	20	14	7	12	118
No. of Boys-only Subgroups	8	9	23	19	15	8	3	9	94
Average Group Size ⁴	19.6 (18-20)	20.0 (20-20)	21.8 (9-25)	18.2 (12-20)	17.2 (11-21)	12.1 (7-18)	12.9 (10-18)	13.7 (7-19)	17.4 (7-25)
Average Choices Made per child	1.62 (.7-2.6)	2.94 (2.3-3.6)	4.07 (3.0-5.0)	4.61 (3.0-5.4)	4.63 (3.9-5.6)	4.34 (3.6-5.6)	4.48 (3.6-5.4)	4.14 (1.4-5.2)	3.98 (2.50)

¹J=junior nursery; S=senior nursery; K=kindergarten; 1=first grade, etc.

²Grades five and six combined.

³Average age of children who could be admitted to a grade.

⁴Range of group values appear in parentheses.

Table 2. Average τ^* ¹

	J	S	K	1	2	3	4	5-6	Total
Full	-.36 (1.52)	-2.19 (2.18)	-4.00 (1.78)	-4.18 (1.84)	-4.44 (2.46)	-4.12 (2.48)	-5.80 (1.41)	-6.98 (2.47)	-4.09 (2.53)
Boys	-.28 (1.33)	-1.20 (1.25)	-2.13 (1.64)	-2.35 (1.13)	-2.37 (.98)	-2.49 (1.21)	-2.22 ² (.39)	-3.20 (1.24)	-2.10 (1.47)

¹Standard deviations appear in parentheses below sample means.

²Based on a sample size of 3. See Table 1 for sample size distribution in boys subgroups.

RESULTS

A computer program (Leinhardt, 1971) was used to calculate the values of τ^* for each sociomatrix. These values were grouped by grade level and a least squares regression program was used to examine the effect of grade and other variables on τ^* . A separate analysis was carried out on partitions of the sociomatrices which isolated the boy-to-boy choices. This procedure was performed to control for the development of sex cleavage. (A similar procedure was carried out by Katz and Proctor, 1959). Unfortunately, the sizes of the groups of girls became unduly small much earlier than those of boys, preventing analogous treatment.

Table 2 presents the sample means and standard deviations of the grouped τ^* values for the full groups (boys and girls) and for the boys' subgroups. Results in both cases support a hypothesis of structural development. At junior nursery level, age about 3:2, the grade average τ^* of -.36 indicates that these sociograms are not significantly different from random graphs of comparable size and pair proportions. However, by senior nursery level, the average sociogram is showing significant (τ^* less than -1.65, the one-sided 5% point of the standard normal distribution) tendencies toward transitive organization. This trend in average τ^* continues almost unremittingly (each higher grade except the third has a more negative τ^* than the grade earlier). At the fifth-sixth grade the typical sociogram's structure is far more transitive than a hypothesis of randomness would lead one to expect.

The trend of the sample means in the boys' subgroups makes a similar impression.

As might be expected from their smaller size (random variation will be a function of the square of the group size), the averages for the boys subgroups have consistently less significant τ^* values than the full groups. But, in addition to the effect of size, these values have been computed after removal of any transitive structure involving girls. For example, the sex cleavage which this procedure controls is a transitive structure—two independent components—and its contribution to the overall structure of the larger group will be discounted by partitioning. Thus, they should be viewed in a perspective which emphasizes trend over magnitude.

It is reassuring to note that the boys' subgroup results continue to support our general developmental contention. Junior nursery sociograms are on the average random, as in the full groups, and around senior nursery or kindergarten the typical sociogram of the boys' choices is significantly non-random in the infrequency of intransitivities. From this point through the fifth-sixth grade the average τ^* s for the boys' subgroups, therefore, indicate that the full groups' observed tendency to become increasingly more transitive as grade increases is not solely due to the fact that the children become sexually segregated during the period covered by the data.

Further evidence for a developmental trend appears in the number of groups in each grade which have τ^* values significant at the 5% level. Table 3 reports the proportion of the groups in each grade with τ^* values less than -1.65 for the full groups and the boys' subgroups. In both cases the likelihood that a group possesses a significant

Table 3. Percent of Groups with τ^* Exceeding the 5% Point.

	J	S	K	1	2	3	4	5-6
Full	20%	67%	83%	87%	90%	86%	100%	100%
Boys	10%	22%	65%	68%	87%	75%	100%	100%

τ^* increases with grade. While the full groups exhibit significant transitive structure sooner than do the boys' partitions, by the fourth grade all of the full groups and all of the boys' partitions exhibit statistically significant tendencies toward transitive organization.

No matter how well these conclusions support the developmental hypothesis, the within group variation demonstrated by the size of the standard deviations for the grouped data suggest that one should be wary of informal conclusions. To examine more closely the relationship between structure and age, several regression studies were carried out. In a preliminary analysis a simple model in which τ^* was linearly dependent on grade was estimated for both the full groups and the boys' partitions. Table 4 presents the results of these analyses.

For the full groups 31% of the variance in τ^* is "explained" by the regression which predicts a decrease in τ^* of $-.69$ for each increment in grade (indicating an increase in transitivity). In this analysis the t-statistic for testing the null hypothesis that the line has zero slope is -7.36 . With 117 degrees of freedom, this is a highly significant value and indicates that rejection of the null hypothesis is appropriate. Analysis of the boys' partitions corroborates these findings. While only 15% of the variance in τ^* is "explained" by the regression, the t-statistic indicates that the slope, $-.28$, is still significantly different from zero.

There is of course, a great deal of variation in τ^* which is "unexplained" by these regressions. The within group variation in τ^* is such as to indicate that this result is not likely to change if the age-grade variable is expressed in some nonlinear form. However, other factors not incorporated into this elementary model affect τ^* . With sociograms taken at an arbitrary point in the school year, any process of reorganization or ar-

rival or departure of new children would add to the variation within grades. Then too, natural differences exist between children of similar ages. In adult groups, while there is a documented tendency toward transitive organization (Holland and Leinhardt, 1972), variation in the degree to which a t-graph structure is reached is great. Perhaps most important, however, is the contribution to variation made by the sociometric method itself (Holland and Leinhardt, 1969, 1972). Since the sociometric data were collected by a fixed choice procedure, it is highly likely that structural measurement error, the presence of a relation in the sociogram not actually present in the social system (and its converse), contributed greatly to the within group variation of τ^* . While it was not possible to examine these variables' effect on τ^* , it was possible to estimate regression equations in which the residual effects of several variables associated with sociometric data were controlled.

In their description of the structural measure τ , Holland and Leinhardt (1970)

Table 4. Simple Regression of τ^* on Grade¹

τ^*	=	b_0	+	b_1 [AGL]	R^2
Full Groups		-3.099		-.685 (-7.36)	.313
Boys Subgroups		-1.805		-.280 (-4.13)	.146

¹Values for the t-statistic are shown in parentheses under the coefficients.

Adjusted Grade Level [AGL]=

- 2 for junior nursery
- 1 for senior nursery
- 0 for kindergarten
- 1 for first grade
- 2 for second grade
- 3 for third grade
- 4 for fourth grade
- 5.5 for fifth or sixth grade.

There were 118 observations for the full groups, 95 for the boys subgroups:

	Variable	Mean
Full	τ^*	-4.087
	AGL	1.440
Boys	τ^*	-2.130
	AGL	1.163

acknowledged the preferability of a random model which constrained choices made, choices received and pairs simultaneously. However, they were able to provide equations only for probabilities based on pair proportions and group size. In other words, instead of comparing the structural tendency toward transitivity in a given sociogram with tendencies in random graphs of the same size, pair proportions and distributions of choices received and made, the measure compares empirical graphs with the structures exhibited by the larger set of random graphs of the same size and pair proportions. By introducing variables for group size, pair proportions, mean choices made, sociomatrix row and column variances (the variances of the choices made and received) and the proportion of the group which was male into the regression equation, it was possible to estimate the residual effect of these variables on τ^* and to examine their effect on the observed relationship between transitive structure and age.

An initial set of eight regression equations was estimated in which τ^* was regressed on nine variables associated with the sociometric data. The estimations appear in Table 5. The statistically significant variables in these regressions were brought into the multiple regression equations for further study. This procedure led to the selection of mean choices, column variance and proportion mutual. In addition, the variance of the

choices made was included because prior studies had indicated its relevance (Holland and Leinhardt, 1972). The proportion of pairs which are asymmetric was also included as germane to identifying effects not accounted for in the random model.

Two multiple regression models were estimated. In the first, no time variable was entered. The aim was to isolate the general effect of the sociometric variables. The second model added age and treated it as a linear variable; that is, each additional year of age was assumed, *ceteris paribus*, to have the same effect on group structure. The models appear in Table 6.

Five independent variables are present in model I: (1) mean choices made, (2) sociomatrix row variance, (3) sociomatrix column variance, (4) proportion of mutual pairs, and (5) proportion of asymmetric pairs. These variables reappear in model II, but here a time variable, adjusted grade level, has been added.

Estimates of coefficients for these models appear in Table 7.⁵ In the estimation for the first model, all the sociometric variables have statistically significant coefficients. Since group structure becomes more transitive with decreasing τ^* , the negative coefficient for mean choices indicates that increases in the choices made per individual

⁵ Only the full groups were analyzed with these multiple regression models.

Table 5. Simple Regressions of τ^* on Sociometric Variables.

Independent Variable	Coefficient	Standard Error of Coefficient	R ²	Standard Error of Estimate	Constant	Mean of Ind. Var.	Standard Deviation of Ind. Var.
Group Size	-.010	.05	0.00	2.54	-3.901	17.492	4.47
Mean Choices	-1.189	.19	.25	2.21	.645	3.979	1.05
Row Variance	.222	.26	.01	2.53	-4.503	1.872	.90
Column Variance	-.246	.07	.11	2.40	-2.624	5.938	3.34
Proportion Mutual	-10.907	3.35	.08	2.43	-2.833	.115	.07
Proportion Asymmetric	.530	2.77	0.00	2.54	-4.219	.249	.08
Proportion Null	2.272	1.58	.02	2.52	-5.468	.608	.15
Proportion Boys	-1.011	2.18	0.00	2.54	-3.523	.558	.11

Table 6. Multiple Regression Models

$$I. \tau_i^* = b_0 + b_1C_i + b_2RV_i + b_3CV_i + b_4PM_i + b_5PA_i + e_i$$

$$II. \tau_i^* = b_0 + b_1C_i + b_2RV_i + b_3CV_i + b_4PM_i + b_5PA_i + b_6AGL_i + e_i$$

where:

b_0 is a constant term;

b_1 - b_6 are the regression coefficients to be estimated;

C_i is the mean choices made (total choices/individuals) in the i th sociomatrix;

RV_i is the sociomatrix row variance for the i th sociomatrix (variance of the choices made);

CV_i is the sociomatrix column variance for the i th sociomatrix (variance of the choices received);

PM_i is the proportion of pairs which are mutually positive in the i th sociomatrix;

PA_i is the proportion of pairs which are asymmetric in the i th sociomatrix;

AGL_i is the adjusted grade level (see Table 4) of the i th sociomatrix;

e_i is a random error term associated with sociomatrix i .

are associated with increases in the group structure's measured transitivity.

On the other hand, increased variation between group members in the number of choices they make is associated with decreased measures of transitive organization. In other words, while increases in all group members' choices increase tendencies toward transitivity in the sociometric data, if this increase is unequally distributed among the group, measured transitivity will decrease.⁶

⁶This finding conflicts with results obtained in Holland and Leinhardt (1972) in which increases

in sociometric status, that is, in the distribution of choices received (sociometric column variance), is associated with increased tendencies toward transitivity. The pair proportions have similar conflicting effects on the transitivity measure. Increments in the proportion of mutual pairs are associated with more negative values of τ^* while increments in the proportion of asymmetric pairs are associated with more positive values of τ^* . Regression I explains approximately 48% of the adjusted variation in the τ^* values. The standard error of 1.82 indicates that the equation estimates τ^* better than does the sample mean of τ^* .

Introducing a linear age-grade variable increases the adjusted R^2 by .12 to .60 and decreases the estimate's standard error to 1.61. All the coefficients remain statistically significant; the sign for the proportion mutual coefficient becomes positive, indicating a positive correlation between proportion mutual and grade ($\rho = .80$, most likely an expression of the development of sex cleavage). The coefficient for the age-grade variable is significant and essentially the same as the coefficient for this variable in Table 4's initial regression estimate.

In sum, after controlling for several sociometric variables which have residual effects unaccounted for in the random model used to determine τ^* , the measurements of transitive structure and the age of children seem still to be significantly associated. According to regression II of Table 7, each yearly grade increment is associated, on average, with a

in sociomatrix row variance were associated with increases in measured transitivity.

Table 7. Multiple Regression Results.*

Constant	Mean Choices	Row Variance	Column Variance	Proportion Mutual	Proportion Asymmetric	Adjusted Grade Level	$\bar{R}^2$	Standard Error of Estimate
I -1.232	-1.678 (.27)	.580 (.21)	-.178 (.07)	-7.388 (3.39)	18.632 (2.61)		.48	1.82
II -2.909	-1.232 (.25)	.485 (.19)	-.170 (.06)	9.303 (4.19)	15.177 (2.38)	-.735 (.13)	.60	1.61

*Standard errors of the coefficients appear beneath them in parentheses.

$\bar{R}^2$ is R^2 adjusted for the loss due to degrees of freedom.

shift of three-fourths of a standard deviation of tendency away from randomness towards transitivity in the organization of positive interpersonal sentiment.⁷

SUMMARY

Evidence indicates that, in general, positive interpersonal sentiment is transitive. The more transitive these relations, the more a group's sentiment network resembles a t-graph, a structure with a systematically articulated set of cliques and hierarchies. In this study, the positive interpersonal sentiment networks of 118 sociograms of children's groups were tested for transitive tendencies to determine whether a process of age related development occurred. Comparisons between sociograms in different grades indicated that from Junior Nursery (aged three years) through sixth grade (aged eleven years) there was a continuously increasing disparity between the expected and observed frequency of intransitivities. A series of regression studies gave estimates of this trend's significance, its magnitude, and the intervening roles of several variables associated with the data. These analyses indicated that groups of older children were significantly more transitive in their interpersonal relations than were younger children, even after the effects of group size, pair proportions and the distributions of choices made and received were controlled.

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⁷ A third model was specified and estimated in addition to the two reported. In this model the age-grade variable was introduced in the form of seven dummy zero-one variables. This specification removed any constraint of functional form other than additivity from the relationship between age and structure. Each grade was thus allowed to have its own effect. There were no essential differences between the reported results and the dummy variable regression. The absence of difference suggests that the relationship between age and structure is indeed linear.

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THE POLITICAL ORIENTATION OF THE SOCIALLY MOBILE IN ITALY: A RE-EXAMINATION

MARY R. JACKMAN *

University of Wisconsin

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This paper examines the data analysis and conceptual assumptions of some recent work on the political orientation of the socially mobile in Italy. Whereas the previous research relied on separate cross-tabulation analyses of the upwardly and downwardly mobile respectively, this paper (utilizing the same data) specifies additive and interactive models of origin and destination effects for both directions of mobility and concludes, contrary to previous research, that mobility per se does not appear to affect the political affiliation of the socially mobile in Italy. The paper concludes with a discussion of the lack of correspondence between key theoretical concepts and their empirical indicators in the data and suggests that the previous research can be questioned on conceptual grounds alone.

SOME recent research in this *Review* reported that the upwardly mobile in Italy lie between origin and destination status in their political orientation (Lopreato, 1967), while the downwardly mobile are more "radical" politically than either origin or destination status (Lopreato and Chafetz, 1970). In the separate theoretical propositions invoked to account for their respective results, Lopreato attributes the former to tensions suffered by the upwardly mobile, while Lopreato and Chafetz attribute the latter result to a type of cognitive dissonance process on the part of the downwardly mobile. This paper suggests that the research by Lopreato and Lopreato and Chafetz lacks a unified theoretical or analytical perspective: consequently, the two sets of results and their accompanying explanations are somewhat contradictory. This paper will focus on two main problems involved in their research: (1) the method employed to analyse the data, and (2) the tenuousness of the link between the theoretical concepts in question and the indicators chosen to represent these concepts.

The two theories to which the Lopreato

and Lopreato and Chafetz research is most relevant may be termed the "cognitive dissonance" theory of mobility effects, and the "acculturation" theory of mobility non-effects. The former theory is not formally derived from Festinger's theory (1957) but it follows a similar logic in predicting that an individual's orientation toward the political system in which he lives will be affected by his personal success or failure in that society (Tumin, 1957; Wilensky, 1966). Thus individuals who rise above their fathers' occupational status develop a "cult of gratitude" toward the system which coaxes them into an ultra-conservative political viewpoint (Tumin, 1957), while the downwardly mobile (in poor countries lacking a success ideology) "externalize" their occupational "skidding" by blaming the system (rather than themselves) for their occupational failure: a higher proportion of the latter than of the occupationally stable express their rejection of the system by supporting leftist political parties that advocate change (Wilensky, 1966). While we might question the categorization of Italy as a poor country lacking a success ideology, Lopreato and Chafetz' results (1970) are especially significant because they are interpreted as support for Wilensky's theory.

The "acculturation" theory (Blau, 1956; Duncan, 1966) takes a less complicated approach by predicting that mobile individuals lie between origin and destination status as they are gradually resocialized into their

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new status group. This approach (which is consistent with the 1967 Lopreato results) postulates that the experience of mobility, upward or downward, has no special effect on a person's political orientation other than to move his primary social interaction from one group to another; where the groups differ in attitude, his attitude will gradually change. Such a model predicts that the attitudes of both the mobile and the occupationally stable can be accurately represented by simply adding the effects of father's status and respondent's status, without recourse to special interaction effects associated with the mobility experience itself (for a more detailed discussion of theory in this area see Jackman, 1972).¹

A RE-ANALYSIS OF THE DOXA DATA

The method of analysis employed by Lopreato (1967) and Lopreato and Chafetz (1970) has three major weaknesses.

1. Dichotomization of the independent variables (in both articles) and the dependent variable (in the 1970 article) with a consequent loss of information results in unnecessarily crude measures that are highly sensitive to the arbitrary cut-off point. Recent research indicates that the manual/non-manual break does not always coincide with the break in the relative education, income, or prestige of occupations (Duncan, 1961 and 1966). The conservative/left-wing dichotomy, which makes a Republican as left-wing as a Communist, and a supporter of the center Christian Democratic party as right-wing as a neo-fascist, is seriously lacking in sensitivity.

2. The use of percentage tables where the base N includes individuals with no party affiliation and those who are ignorant of or not interested in politics, leads to confusing results. Because the percentage figure does not incorporate all the cases in the cell, the

relationship between mobility and political affiliation may look quite different, depending on whether one is examining percent left-wing or percent conservative (i.e., the one is not automatically the obverse of the other, since non-partisans are present in the base N). For example, in Table 1 (Lopreato and Chafetz, 1970:441), the skidders have a marginally higher percentage left-wing than the stable manual category, but in the row for percent conservative, they fall between origin and destination status (though somewhat closer to the latter, suggesting that father's status does not have as strong an influence as respondent's status). Strong evidence for a distinct leftist orientation on the part of skidders would require an extra large entry in the percent-left-wing cell for skidders, matched by an extra small percentage conservative.

3. Separate treatment of upward and downward mobility in two articles (and with each categorizing the dependent variable differently)² makes it difficult for either article to support one theory over another. Although the original formulations of the cognitive dissonance approach are as piecemeal in this respect as these authors' empirical discussion of it, the theoretical writings are linked by the common assumption that individuals avoid dissonance and develop sets of beliefs about the system consistent with their personal success or failure in its status hierarchy. A table that presents data for only one direction of mobility cannot rule out competing theories to favor a clear cognitive dissonance interpretation, as this framework requires specific behavior of both the upwardly and the downwardly mobile.

Combining the results from Lopreato (1967) and Lopreato and Chafetz (1970), the skidders appear to have a slightly higher percentage left-wing than the stable manuals but are intermediate between origin and destination status in their percent conservative, while the upwardly mobile are intermediate on both measures. Had these results

¹ Lopreato and Chafetz (1970:446) to the contrary, an interaction cannot produce an additive effect. The "tension" theory of mobility effects (which predicts that both upwardly and downwardly mobile are especially conservative as an expression of the special tensions they are experiencing), is not discussed in this paper, since it is considered irrelevant to an explanation of the results reported in both Lopreato (1967) and Lopreato and Chafetz (1970).

² Lopreato (1967) switches between a trichotomization and a dichotomization of party affiliation, but the table presented in footnote 3 (Lopreato, 1967) differs from Table 1 (Lopreato and Chafetz, 1970) in that the Republicans and Christian Democrats are in a "center" category instead of in the left-wing and right-wing categories respectively.

been presented in the same table, it would have been difficult to entertain a cognitive dissonance interpretation. Instead, they would lead either to (a) a serious questioning of the validity of the one discrepant cell (percent left-wing for skidders) and an attempt to categorize more finely to see if the discrepancy still held or if it joined the other cells in an acculturation mobility non-effect, or (b) a rejection of the cognitive dissonance interpretation for the Italian case in favor of a new theory resting on assumptions that would predict distinct political behavior by the downwardly mobile alone. We suggest that option (a) would be more appropriate, especially since the empirical definitions of "left-wing" and "conservative" are inconsistent in the two articles, with the article on upward mobility employing a slightly finer categorization.

In our reanalysis of the DOXA data,³ we present first a comparison of actual percentages left- and right-wing of the mobile and stable with percentages predicted by a model that assumes purely additive effects of father's and respondent's status (as the acculturation theory hypothesizes).⁴ Following Lopreato and Chafetz (1970), the left-right continuum is dichotomized into (a) left-wing, consisting of supporters of the Republican party, the Social Democratic party, the Socialist Party, the Communist party, and "the Left," and (b) right-wing, consisting of the supporters of the Monarchist parties, the Italian Social Movement, the Liberale party, the Christian Democratic party, and "the Center." Also in line with the 1970 article, the base N includes those "not interested" in politics and those with no party preference, while missing data are excluded. The occupational status scale⁵ is broken into

three categories as follows: Low SES = responses 9 + 12; Medium SES = responses 6 + 7 + 8; High SES = responses 1 + 2 + 3 + 4 + 5 (response numbers refer to those listed in the occupation question reproduced in footnote 5 above).⁶ Following both the 1967 and the 1970 articles, those employed or with origin status in farm occupations, those not currently in the labor force, and missing data were excluded from the analysis which is therefore confined to male members of the currently active industrial labor force.

Table 1(a) compares actual percentages left-wing with those predicted by an additive regression model

$$\hat{L} = a + \sum_{i=1}^8 b_i X_i + \sum_{j=1}^8 c_j Z_j \quad (1)$$

while Table 1(b) compares actual percent-
cupational status was provided by the following questions:

(a) Among the occupations listed here, which corresponds to your present occupation?

(b) Which corresponds to your father's last occupation?

1. politicians, large scale proprietors, entrepreneurs, managers of big firms, high government functionaries.
2. university-educated professionals (doctors, lawyers, engineers, artists, etc.).
3. medium-sized proprietors and merchants, managers of medium-sized firms.
4. other professionals (land surveyors, bookkeepers, etc.).
5. middle grades of officials in State, industry and commerce.
6. small merchants, small proprietors (except farm), artisans with dependent workers.
7. low grades of officials in State, industry and commerce.
8. artisans without dependent workers, skilled workers.
9. semi-skilled.
10. small farm owners, share-croppers, etc.
11. farm hands.
12. unskilled workers, except farm hands.
13. unemployed, on pension, student (for respondent's occupation); don't know, don't remember, no answer (for father's occupation).

³ These are the same data that were used by Lopreato (1967) and Lopreato and Chafetz (1970). They come from a national survey of Italian male family heads interviewed by DOXA in December 1963 and January 1964.

⁴ For previous uses of this method for the analysis of the effects of mobility on fertility, prejudice, associational membership, and attitude toward the political system (all with American data), see Duncan (1966), Hodge and Treiman (1966), Vorwaller (1970), and Jackman (1972), respectively. Means predicted by the additive model of father's and respondent's status effects accurately matched the actual means for both mobile and stable groups in all of the above papers.

⁵ The measure of respondent's and father's oc-

⁶ Initially four SES categories were employed, with "High SES" divided into "Upper Middle Class" (=responses 3+4+5) and "Upper Class" (=responses 1+2). However, far too few individuals either originated in and/or attained status in the "Upper Class" to yield stable entries for those cells; thus the "Upper Class" and "Upper Middle Class" were amalgamated to form "High SES".

Table 1(a). Actual & Predicted Relation of Percent Left-Wing to Father's & Respondent's Socioeconomic Status, Italy, 1963-64.**

Father's SES	SES of Respondent			Total
SES	Low	Medium	High	
Actual Percentages Left-wing				
Low	52.7	52.0	39.0*	51.2
Medium	52.5	43.1	27.2	41.8
High	66.6*	42.0	30.7	35.4
Total	53.3	45.3	30.4	43.5
Predicted Percentages Left-wing				
Low	55.3	49.2	34.0*	
Medium	49.7	43.6	28.5	
High	52.0*	45.9	30.8	
Actual Minus Predicted				
Low	-2.6	+2.8	+5.0*	
Medium	+2.8	-0.5	-1.3	
High	+14.6*	-3.9	-0.1	
Number of Cases				
Low	114	75	18	207
Medium	61	181	59	301
High	9	31	101	141
Total	184	287	178	649

* Number of cases less than twenty.

** Source: DOXA national sample of male household heads.

ages right-wing with those predicted by an additive regression model

$$\hat{R} = a + \sum_{i=2}^3 b_i X_i + \sum_{j=3}^8 c_j Z_j \quad (2)$$

where $\hat{L}$ is the predicted percentage left-wing on a dichotomous variable defined to equal 1 if the respondent's party preference is left-wing, and 0 otherwise; $\hat{R}$ is the predicted percentage right-wing on a dichotomous variable defined to equal 1 if the respondent's party preference is right-wing, and 0 otherwise; a is the constant; $X_2=1$ if the respondent's occupation falls into response categories 6, 7, or 8, and 0 otherwise; $X_3=1$ if the respondent's occupation falls into response categories 1, 2, 3, 4, or 5, and 0 otherwise; and each $Z_j=1$ if the respondent's father was in the similarly defined occupational categories for fathers, and 0 otherwise. One status group each for fathers and respondents (Low SES) was excluded from the equation to make it soluble; slopes for the included categories are then expressed

as deviations from the missing category, i.e., the constant (Suits, 1957).

The cognitive dissonance approach suggests that the model of additive origin and destination status effects alone would overpredict the percent left-wing and underpredict the percent right-wing among the upwardly mobile, while overpredicting the percent right-wing and underpredicting the percent left-wing among the downwardly mobile. However, Tables 1 and 2 reveal no such pattern. While the deviations of the predicted from the actual percentages left-wing in Table 1(a) tend to be fairly sizeable, they follow no consistent pattern; and the two largest deviations (for the extreme upwardly and downwardly mobile) are based on unreliably small cell sizes.⁷ The summarized weighted percentages left-wing in

⁷ Lopreato and Chafetz barely differentiate between reliable and unreliable cell sizes in making inferences from their tables (see especially 1970: Tables 4 and 5), but it is common practice to regard cell N's of less than twenty (arbitrarily chosen as the cut-off point) as containing too much sampling error to yield stable results.

Table 1(b). Actual & Predicted Relation of Percent Right-Wing to Father's & Respondent's Socioeconomic Status, Italy, 1963-64.**

Father's SES	SES of Respondent			
	Low	Medium	High	Total
Actual Percentages Right-wing				
Low	16.7	21.3	27.8*	19.4
Medium	23.0	25.5	40.7	28.0
High	11.1*	25.9	40.6	35.4
Total	18.5	24.3	39.3	26.8
Predicted Percentages Right-wing				
Low	16.3	20.2	34.4*	
Medium	22.0	25.9	40.1	
High	21.6*	25.5	39.7	
Actual Minus Predicted				
Low	+0.4	+1.1	-6.6*	
Medium	+1.0	-0.4	+0.6	
High	-10.5*	+0.4	+0.9	
Number of Cases				
Low	114	75	18	207
Medium	61	181	59	301
High	9	31	101	141
Total	184	287	178	649

* Number of cases less than twenty.

** Source: DOXA national sample of male household heads.

Table 2(a). Summary of Actual & Predicted Relation of Percent Left-wing to Intergenerational Social Mobility, Italy, 1963-64 (weighted percentages derived from Table 1(a)).

Mobility	Actual Percent	Predicted Percent	Actual Minus Predicted	Ratio of Actual to Predicted	Number of Cases
Upward	40.83	39.37	+1.46	1.037	152
Stable	42.70	43.70	-1.00	0.977	396
Downward	50.53	48.74	+1.79	1.037	101

Table 2 (a) indicate that the additive model underpredicts the percentage left-wing among the upwardly mobile almost as much as it does among the downwardly mobile (which contradicts the cognitive dissonance approach), and in both cases the deviations are not large. Furthermore, adding cognitive dissonance interaction terms for the upwardly and downwardly mobile to the simple additive model⁸ increased the proportion of variance explained from 3.30% to 3.43%—a tiny increment, statistically insignificant at the .05 level (see Cohen, 1968, for the test used).⁹ In Table 1(b), the deviations of predicted from actual percentages right-wing are small in four out of four off-diagonal

⁸ The cognitive dissonance interaction terms were added to equation (1) as follows:

$$\hat{L} = a + \sum_{i=1}^n b_i X_i + \sum_{j=1}^m c_j Z_j + dU + eV \quad (3)$$

where the first four terms are defined as in (1), and where $U=1$ if the respondent is upwardly mobile, and 0 otherwise; $V=1$ if the respondent is downwardly mobile, and 0 otherwise; the occupationally stable were excluded in order to make the equation soluble.

⁹ We should note that since the dependent variable is dichotomous, the homoscedasticity assumption of regression analysis is violated, and thus the significance test for the increment to the R^2 is not strictly appropriate (see Goldberger, 1964:248-251).

cells that have reliably high cell N 's. The summarized weighted percentages right-wing in Table 2(b) indicate that the additive model predicts the actual proportions right-wing closely among both the upwardly and downwardly mobile; adding interaction terms for the upwardly and downwardly mobile, as in (3) above, increased the R^2 by a similarly tiny and statistically insignificant increment, from 3.61% to 3.62%.¹⁰

Although Tables 1 and 2 use a uniform measure of the dependent variable for the upwardly and downwardly mobile, and treat socioeconomic status as more than a dichotomous phenomenon, the insensitive categorization of the dependent variable and the inclusion of individuals with no particular political sympathies in the base N yield results clumsy to interpret. One could test the cognitive dissonance and acculturation theories more efficiently by including in the analysis only those individuals with an expressed political preference and assigning

¹⁰ The further addition of an interaction term for the extreme downward mobility cell (i.e., Respondent's Low SES x Father's High SES) to equations (1) and (2) in order to assess the possibility of a mobility effect for the extreme downwardly mobile only, also failed to increase the R^2 by a substantively or statistically significant increment.

Table 2(b). Summary of Actual & Predicted Relation of Percent Right-wing to Intergenerational Social Mobility, Italy, 1963-64 (Weighted percentages derived from Table 1(b)).

Mobility	Actual Percent	Predicted Percent	Actual Minus Predicted	Ratio of Actual to Predicted	Number of Cases
Upward	29.60	29.61	-0.01	0.999	152
Stable	26.82	26.66	+0.16	1.006	396
Downward	22.83	23.04	-0.21	0.991	101

each with a leftism score according to his distance from the far right: mean leftism scores for each cell predicted by the additive acculturation model could then be compared with actual mean leftism scores. Although mean scores remain gross measures, this measure would be superior to the use of percentages on two grounds: first, it provides a more sensitive measure of leftism, and second, the mean score includes all people in the cell.

Leftism was scored as follows, with individuals reporting no political affiliation and missing data excluded from the analysis:

- 0 Monarchist Parties; Italian Social Movement
- 1 *Liberale* Party
- 3 Christian Democratic Party; "the Center"
- 4 Republican Party
- 5 Social Democratic Party
- 7 Socialist Party; "the Left"
- 10 Communist Party

Scores are based on each party's estimated distance from the extreme right.¹¹ These estimated distances are approximate since they are derived from readings from the literature on Italian politics rather than from some direct measure of perceived party distances, but this scoring is considered less arbitrary than assuming equal distances between parties without evidence (Converse, 1966; Tufte, 1969; Barnes, 1966; LaPalombara, 1965).¹²

The first part of Table 3 presents the actual mean leftism scores for mobile and stable groups (SES categories are defined as in Table 1). The main diagonal cells indicate a negative monotonic relationship between SES and leftist political affiliation among the occupationally stable. All but one

¹¹ See Converse (1966) for the difficulties associated with treating the political spectrum as unidimensional (as is done here and in Lopreato's and Chafetz' research).

¹² The assumption of equal distances between parties actually made little empirical difference. Analyses were carried out with both the unequal-distances scale and a scale ranging from 0-6 that assumed equal distances between parties: the two scales correlated .98 and yielded equivalent results, although use of the unequal-distances scale obtained a marginally higher R^2 (12.9% compared with 10.8%).

of the six mobile cells yield scores intermediate between origin and destination status; and the only discrepant cell has an unreliably small N. Thus the actual mean scores follow the pattern predicted by the acculturation theory. A more precise estimate of the acculturation theory's efficiency is provided by comparing the actual mean scores with those predicted by the additive model of origin and destination status effects

$$\hat{P} = a + \sum_{i=1}^3 b_i X_i + \sum_{j=1}^3 c_j Z_j \quad (4)$$

where $\hat{P}$ is the predicted mean leftism score and the other terms are defined as in (1) and (2) above. The second part of Table 3 presents the mean scores predicted by the additive model;¹³ while the third part pre-

¹³ Coefficients yielded by the additive regression model are:

$$\begin{array}{ll} a & 6.77 \\ b_1 & -.679 \\ b_2 & -2.175 \end{array} \quad \begin{array}{ll} c_1 & -.688 \\ c_2 & -.711 \end{array}$$

The model was statistically significant beyond the .0001 level, and the coefficient of determination was .1286. The fact that this coefficient is small should

Table 3. Actual & Predicted Relation of Leftism to Father's & Respondent's Socioeconomic Status, Italy, 1963-64.**

Father's SES	SES of Respondent			
	Low	Medium	High	Total
Actual Mean Leftism Scores				
Low	6.76	6.15	4.42*	6.34
Medium	5.96	5.40	4.05	5.27
High	7.00*	5.24	3.83	4.35
Total	6.49	5.59	3.96	5.41
Predicted Mean Leftism Scores				
Low	6.77	6.09	4.59*	
Medium	6.08	5.40	3.90	
High	6.06*	5.37	3.88	
Actual Minus Predicted				
Low	-.01	+.06	-.17*	
Medium	-.12	.00	+.15	
High	+.94*	-.13	-.05	
Number of Cases				
Low	79	55	12	146
Medium	46	124	40	210
High	7	21	72	100
Total	132	200	124	456

* Number of cases less than twenty.

** Source: DOXA national sample of male household heads.

sents the deviations of predicted from actual mean scores. With the exception of the extreme downward mobility cell which contains only seven cases, predicted scores attain close fit with the actual scores.

Table 4, which gives a summary comparison of the actual and predicted weighted mean scores for mobile and stable groups, indicates that the prediction errors of the additive model are not only in the wrong direction to be of interest to the cognitive dissonance theory, but are also too small to be of theoretical interest. Furthermore, adding interaction terms for the upwardly and downwardly mobile to the simple additive model as in (3) above negligibly increased the amount of variance explained in political affiliation from 12.86% to 13.04%, an increment insignificant at the .05 level (for the test used see Cohen, 1968).

We have demonstrated that a more thorough analysis of the DOXA data fails to support the cognitive dissonance theory of mobility effects advanced by Lopreato and Chafetz (1970). Instead, our analysis, which supports the acculturation theory of additive origin and destination status effects, suggests that mobility *per se* has no effect on political affiliation. More fundamentally, however, we will now question the adequacy of the measures employed by these authors as indicators of the theoretical concepts in question.

THEORETICAL CONCEPTS AND EMPIRICAL INDICATORS

The Independent Variables

There are three major, and interrelated, difficulties with the scale used to measure father's and respondent's occupational status (see footnote 5 above for the full wording of the question): (a) the categories are loosely specified with only sketchy guidelines for the inclusion of a specific occupation in one category rather than another; (b) the ordering of categories is *ad hoc* with no demonstration that occupations falling into one category are of higher status than those falling into another; (c) use of de-

not be regarded as a serious problem, given that the purpose of this exercise is to ascertain the effects of mobility on partisanship *rather than* to account for all of the determinants of partisanship.

scriptive terms such as "middle grade" and "low grade" as guides to respondent self-location yields results very much like a measure of subjective social class.¹⁴

A comparatively difficult cognitive job is thus required of the respondent (or perhaps sometimes of the interviewer?). Rather than simply asking the respondent to report his own and his father's occupation, and then later coding it according to some uniform and theoretically meaningful SES or prestige scale, he is called upon to decide for himself, with only sketchy and rather value-loaded guidelines, his own and his father's position in the occupational hierarchy.¹⁵ Since there is no evidence to suggest that there is a consensus in mass publics as to the criteria for distinguishing objectively between various "grades" of government officials or between "skilled," "semi-skilled," and "unskilled" workers (to name only those categories that pose the most obvious challenge to the respondent's perceptual "accuracy"), the clear value implications of such terms make the question little more than a measure of where the respondent *thinks* he and his father belong on an *ad hoc* status scale.¹⁶

The Dependent Variable

The use of responses to the question "In your opinion, which party in Italy has best at heart the interests of people like you?" as a measure of respondents' orientation toward the political system has two main problems. (a) Research indicates that among

¹⁴ Although such measures are interesting in themselves, they are not pertinent to the "cognitive dissonance" theory of mobility effects (in other than perhaps an intervening sense), which posits that *actual* (structural) mobility, and not perceived (psychological) mobility, affects an individual's orientation toward the system.

¹⁵ It is possible that respondents were asked to name their own and their father's occupations and that these occupations were later coded uniformly into the categories outlined in the question quoted from the codebook, in which case this criticism should be ignored. However, it is apparent from the original questionnaire that respondents classified themselves.

¹⁶ A more trivial point about the measure of father's occupation is that it asks for father's *last* occupation, rather than his occupation when the respondent was growing up which might be a more accurate measure of the respondent's status of origin.

Table 4. Summary of Actual & Predicted Relation of Leftism to Intergenerational Social Mobility, Italy, 1963-64 (Weighted means derived from Table 2).

Mobility	Actual Score	Predicted Score	Actual Minus Predicted	Ratio of Actual to Predicted	Number of Cases
Upward	5.17	5.10	+.07	1.014	107
Stable	5.38	5.40	-.02	0.996	275
Downward	5.85	5.88	-.03	0.994	74

the mass public a respondent's party affiliation is a poor predictor of his political opinions on specific ideological issues. The substantive content to party affiliation varies considerably among followers of the same party and very little between followers of different parties. Moreover, a large proportion of respondents in the mass public appear uneducated in the left-right ideological continuum (McClosky *et al.*, 1960; Converse, 1964). For the Italian case, Barnes (1966) cautions that although, on a formal parliamentary level, Italian parties can be placed on a left-to-right continuum, the role of ideology in the mass support of various parties is difficult to evaluate (see also LaPalombara, 1965:285). For example, support for the dominant Christian Democratic party comes from many disparate elements: the party contains a trade union faction, an office-oriented majority, a reformist faction, a traditionalist conservative faction, and several personal cliques (Barnes, 1966:305). Besides attracting ideologically diverse supporters on the left-right continuum, the parties are also cross-cut on other dimensions such as clericalism vs. laicism, laissez-faire vs. government planning, and regional differences in patterns of party recruitment and support between the industrialized North and the poorer and more rural South (Barnes, 1966; Tarrow, 1967). Lopreato and Chafetz' (1970) data for their Table 3 is evidence that a leftist position on ideological issues correlates poorly with a leftist party affiliation among Italian males.¹⁷ (b)

¹⁷ Lopreato's and Lopreato and Chafetz' treatment of the ideological questions presented in Tables 3 and 4 (1967) and Table 3 (1970) as conceptually distinct from political ideology (which is what party affiliation is supposed to be measuring) is theoretically tautological.

An even more fundamental problem is that, regardless of how well party affiliation measures political ideology, it is not a valid measure of a person's *affective* feelings toward the political system as a whole. Here the looseness of the cognitive dissonance approach becomes critical. A distinction should be drawn between *substantive* political opinion (as measured by a battery of questions on issues and ideology) and *affective* feelings toward the system as a whole (as measured by questions on political efficacy).¹⁸ The original theoretical formulations posit that an individual's occupational success or failure will affect his attitude toward the political system in which he lives: the deduction is then made that an individual's satisfaction or dissatisfaction with the political system as a whole will be indicated by his substantive political ideology or party affiliation, seen in left-right terms. However, from the preceding discussion, there is no reason to expect that an individual's generalized orientation toward the political system will be "logically" translated into a particular coherent political ideology (DiPalma, 1969). Furthermore, there is no reason to regard ideologues of the extreme right as more satisfied with the current political system than are those of the extreme left: both ideologies advocate some kind of radical change. We suggest that satisfaction with the system will be reflected more accurately in an individual's feelings of efficacy toward the institutionalized *processes* of the political system than in his more specifically *issue-oriented* opin-

¹⁸ For a discussion of political efficacy and related variables, see Robinson, *et al.* (1968) and Finifter (1970).

ions, or non-opinions (for a fuller discussion on this topic, see Jackman, 1972).

SUMMARY AND CONCLUSIONS

The piecemeal approach of Lopreato (1967) and Lopreato and Chafetz (1970) to the problem of the orientation of the mobile toward the political system in Italy led them to view their contradictory results as exceptions to the American case and to build complicated theoretical propositions in order to account for these exceptions. Had they subjected the theories in the area to careful consideration *before analysis* rather than resorting to *ex post facto* interpretation of findings, Lopreato and Lopreato and Chafetz might have inferred (a) that their results spoke, in a contradictory manner, to the acculturation theory of additive status effects and the cognitive dissonance theory of mobility effects on attitude toward the political system, and (b) that their measures of the posited theoretical concepts were inappropriate.

A re-analysis of the DOXA data indicates that the additive acculturation model of father's and respondent's status effects accurately predicts the mean political affiliation of both mobile and stable groups in Italy. A cognitive dissonance explanation of the political affiliation of the downwardly mobile (or a tension explanation of the political affiliation of the upwardly mobile) from these data is inappropriate. However, shortcomings of the data prevent these results from providing a definitive test of the cognitive dissonance position. Apart from the suspected error in measurement of father's and respondent's occupational status, the political affiliation measure does not validly indicate the respondent's ideology (which it purports to measure) or his general orientation toward the political system (which it does not purport to measure but which is the essential concept in the cognitive dissonance mobility-effects theory).

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COMMENTS

THE POLITICAL ORIENTATION OF THE SOCIALLY MOBILE IN ITALY: A COMMENT

JOSEPH LOPREATO
University of Texas, Austin

JANET SALTZMAN CHAFETZ
University of Houston

Mrs. Jackman is to be commended for much of what she has done, and we are grateful for the attention she has given to our work. Her paper, however, contains a number of errors. Space limitations dictate that we address ourselves to the most basic.

The fundamental error lies in a certain misunderstanding of our work. Arguing against what she terms the "cognitive dissonance" theory of mobility effects," which allegedly informed our 1970 work, she claims to find support for the "acculturation" theory of mobility non-effects." The latter "theory," which is new to us, "postulates that the experience of mobility, upward or downward, has no special effect on a person's political orientation other than to move his primary social interaction from one group to another; where the groups differ in attitude, his attitude will gradually change." That is, the "model" of her preference "predicts that the attitudes of both the mobile and the occupationally stable can be accurately represented by simply adding the effects of father's and respondent's status, without recourse to special inter-action effects associated with the mobility experience itself."

To predict and to explain are two quite different aspects of scientific inquiry. Our basic aim in both papers (Lopreato, 1967; Lopreato and Chafetz, 1970) was not so much to predict the political orientation of the occupationally mobile as to *explain* possible changes in that orientation which may have been associated with occupational mobility. Thus, the 1967 article honored the table on mobility and political orientation with a mere footnote. If an explanation of changes in a person's political orientation is what is at stake, you can hardly say that "where the groups differ in attitude, his attitude will gradually change." It would be like a physicist arguing that every x number of years the planet Mars reaches its closest point to the earth because interdependencies in our solar system "gradually change."

To say that the upwardly mobile, for instance, change their attitudes in the direction of the class of destination because they "acculturate" is merely to play with words. If "acculturation" means anything at all in the present context, it means that, for brevity's sake, some of the experiences of men in the flesh as they move from one class to another may be elliptically noted under the general rubric of "acculturation." When we inquire about the analytical components of that acculturation, we will find it necessary to generate statements about the mental dispositions (needs, sentiments, interests) of men in the flesh. That Mrs. Jackman departs radically from the sort of thing we tried to do is all too clear when she states that "mobility *per se* does not appear to affect the political affiliation of the socially mobile in Italy." We would not disagree. "Mobility *per se*" does nothing. Nor do we claim that it does.

Let us put it differently: while acculturation indubitably goes on when people move from one social class to another, not everyone acculturates at the same pace or to an equal degree. So, for instance, in the 1970 article we found that the downwardly mobile children of upper-level nonmanuals were more often left-wing than skidders who came from lower-level nonmanual positions. Likewise, the literature shows clearly that although achievers tend to bestride the classes of origin and destination in their political orientation, in some societies they are closer to their destination class, and in others to their class of origin. To be sure, some of these differences result from age differences and sample differences in time elapsed since the occupational movement; but doubtlessly, some are due to differences among societies in the cultural factors that aid or impede class socialization. Both studies were placed in this context, and in it perceived discrimination and perceptions of the opportunity structure seemed important explanatory variables.

Mrs. Jackman does not address this core of our work; instead she speaks to an almost incidental aspect of it. Moreover, our measure of mobility and hers are not comparable: where we use a class dichotomy, she uses a trichotomy. Her subsamples of achievers and skidders are larger than ours. We are not surprised by some of her findings.

Mrs. Jackman argues that we ought not measure mobility across the manual-nonmanual

axis, saying that "recent research indicates that the manual-nonmanual break does not always coincide with the break in the relative education, income, or prestige of occupations." First, such evidence is not recent. In truth, the matter goes at least as far back as Marx who found it necessary even in his stalwart commitment to a class dichotomy to take into account the famous "intermediate strata."

Second, we applied a four-class scheme at a crucial point in both papers. Also, the 1967 paper contains the following statement:

Following convention in this area of sociology, I have conceived of the class structure dichotomously; I have accordingly assumed "the middle class" to be a homogeneous entity. This procedure is, of course, convenient, but it does not do full justice to the reality of the case. What we term "middle class" represents in fact a rather wide spectrum of social differences (Lopreato, 1967: 589).

Third, we deliberately measured mobility across the manual-nonmanual axis because we were addressing ourselves to an existing literature on a comparative key. In both papers we aimed to examine existing literature; draw out the logical structure of existing theory; test an aspect of it with our Italian data; and on this basis return to existing theory and amend it as needed. We relied heavily on the work of Lipset and Zetterberg who had recently brought the present area of analysis to center stage and who in their cross-national work had faced the hard task of achieving some comparability cross-culturally. The class dichotomy understandably aided their comparative analysis, and we, to present our findings comparably, had little choice but to follow established procedure. Aside from that, whether a class dichotomy or a more detailed scheme should be used in research is a highly complex issue, though there is no invariant reason why Mrs. Jackman should not use a trichotomy either as refinement or as predilection.

These same comments apply also to Mrs. Jackman's criticism of our conservative/left-wing dichotomy. The comparative framework of our research pointed to the utility of a dichotomy. We do, however, share our critic's objections to this practice, and regret moreover including *one case* of preference for the Republican Party in the left-wing category of the 1970 article.

Concerning the inclusion of individuals who gave no specific party preference in our base N, this served readers who preferred full information and did not damage our work—whether or not skidders were more often left-wing than stable manuals was not important; they were

significantly more likely to be left-wing than their counterparts elsewhere. And it is this that we tried to explain. Moreover, skidders were more politically interested than both stable manuals and nonmanuals (i.e., less frequently ignorant or uncommitted), a fact that also supported our general argument, and hence was worthy of notice.

The two main points made in Part II of Mrs. Jackman's paper are incorrect. However, the senior author of this Comment is partly to blame for one of these errors, and we are grateful to Mrs. Jackman for helping prevent future misunderstandings.

First, she is mistaken in assuming that our interviewees classified themselves occupationally according to the scale reported in her footnote 5. Lopreato is to blame for this misinterpretation because, although he gave a full set of instructions to the Roper Public Opinion Research Center when he placed his study in the public domain, the interview schedule, which she probably obtained there, does indeed read as if occupational placement resulted from interviewee self-placement. The truth is that while interviewees were indeed asked to classify themselves occupationally by the above scale, interviewers in turn were required to obtain detailed information on the respondent's job duties, employing firm, and sector of the economy. The occupational information was coded after the interviewing. Mrs. Jackman recognizes this possibility in her footnote 15; we regret the misunderstanding.

Concerning Mrs. Jackman's contention that "the ordering of the categories is *ad hoc* with no demonstration that the occupations falling into one category are of a higher status than those falling into another," we can only say that our occupational scale condenses a much more detailed one, and was constructed with the help of Italian researchers and scholars who probably know more than any of us about the relative prestige and economic power of occupations in Italy. *Utinam modestia sociologica virtus!*

Mrs. Jackman's second point in Part II of her paper raises a complex issue. She quotes our question on political orientation ("In your opinion, which party in Italy has best at heart the interests of people like you?"), and asserts that "Research indicates that for the mass public a respondent's party affiliation is a poor predictor of his political opinions on specific ideological issues."

We cannot treat the issue she raises in so short a space but would observe that the validity of her statement depends on the complexity

of the party structure *inter alia*. More basically, why speak of party affiliation? The above research question concerns party preference, not affiliation. Strictly speaking, it does not even concern party preference but the respondent's perception of the interest-group representations of his society's political parties. Our interviewees were literally asked in Italian to state "which party best defends the interests" of people like themselves. This question gave them a chance to express their political *interest*. The question is the closest thing to a Marxian category, which is what we had in mind in the first place.

The question, which in a recent publication (Lopreato and Hazelrigg, 1972) is literally translated, was chosen after long consultation with Italian scholars and researchers to avoid precisely the kinds of problems Mrs. Jackman mentions. It is better than a question on party affiliation because, even assuming our respondents' willingness to give us this information, such a question would have been relevant for a minute portion of our sample only. It is preferable to a question about actual or intended voting behavior because it avoids the problem of the "generic dissent" vote, which has been prevalent in Italy; thus, while there are many committed Communists in Italy, many who vote for the Communist Party do so primarily to criticize a bungling bureaucracy, the church's meddling in secular affairs, a terribly fragmented Christian Democratic Party, and the like. Recently we discussed such problems in more detail (Lopreato and Hazelrigg, 1972: Chapter X). Naturally, we do not claim perfection for our research probe, which incidentally is one of many more specific.

Finally, Mrs. Jackman speaks of our theory (1970) as a "cognitive dissonance theory of mobility effects," despite the fact that in neither paper do we mention this theory. The statements she makes in this connection are puzzling. She says that her "more thorough analysis of the [Lopreato] data fails to support the [Lopreato-Chafetz] cognitive dissonance theory of mobility effects," (despite the fact that applying cognitive dissonance theory would require analyzing a research situation containing at least two "cognitions," while she deals only with one—political orientation). We feel rather like the gentleman in the Neapolitan folktale who, after relating a dream that he could fly, was declared an angel by his peers.

A careful examination of our work will reveal that we draw from at least half a dozen theoretical strains, some of which are farther removed from "cognitive dissonance theory"

than others, and especially from Marx's theory of class consciousness and Pareto's theory of logical conduct. "Interest," as we state several times, is the basic "cognition" guiding our skidders' party preference. Cognitive dissonance theory would apply clearly if their particular party preference resulted from a process resolving two inconsistent (dissonant) interests. But we did not find that to be the case. Nowhere is this more obvious than in our finding that, far from reflecting proletarian or "revolutionary politics," the extreme left-wing orientation of the skidders represented "an individualized, pragmatic, and self-seeking calculation" (Lopreato and Chafetz, 1970:448). As we showed, they did not identify with their class of destination, and, strange as it may seem, they viewed left-wing parties as their best chance for redeeming their old middle-class position.

We do not deny, of course, that our work is related to cognitive dissonance theory, and indeed the theory with which we conclude the 1970 paper shows affinity with it—probably more than was intended. But frankly, had we been inclined to argue dissonance, Pareto's work on the derivations and the residues, and especially Classes I (instinct for combinations) and V (integrity of the individual and his appurtenances) of the residues, would have made more sense to us. It would also have precluded accepting the most basic tenet of cognitive dissonance theory: "It is still overwhelmingly true that related opinions or attitudes are consistent with one another" (Festinger, 1957:1). The proposition is either false or fails to recognize the complexity of mental "consistency."

We disagree with many more points in Mrs. Jackman's paper, but have probably said too much already. We would add in closing that we found much in her paper to be both correct and useful.

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REPLY TO LOPREATO
AND CHAFETZ*

MARY R. JACKMAN
University of Chicago

Lopreato and Chafetz' remarks allow me to be brief.

1. Their distinction between "explanation" and "prediction" becomes a little esoteric. A good theory provides a number of hypotheses which are phrased as predictions about empirical relationships: these predictions constitute hypothetical explanations about how the world works, and insofar as empirical data are consistent with these hypothesized explanations, we consider them more seriously as possible "true" explanations. While it is sometimes possible to make empirical predictions without recourse to a theoretical explicandum (as in economic or demographic forecasting), the type of theoretical prediction discussed in my paper clearly implies an attempt at explanation.

2. The "acculturation" theory ("new" to Lopreato and Chafetz) was proposed by Blau (1956:291) and has subsequently been used by Hodge and Treiman (1966), among others.

3. In response to the argument that there is nothing in their work to suggest an interaction effect, I can only point to the following passages in the 1970 article: page 440 (abstract); page 441 (Table 1, paragraph 4); page 442 (paragraphs 2, 4, and 5); page 443 (paragraphs 5 and 6); page 444 (paragraph 3); page 446 (paragraphs 1, 3, and 4); and page 449 (paragraphs 3 and 6). With reference to their further argument that the difference between my results and theirs stems from the fact that I trichotomized the independent variables while they used a dichotomy, I can report that my original analysis did include a comparison of actual percentages and means with those predicted by an additive model for a two-by-two table, using their classification. Results obtained were not substantially different from those obtained with the trichotomized independent variables, but this part of the analysis was not reported because of the insensitivity of such gross categorizations and because use of a four-cell table would mean that the addition of just one interaction term to the additive model in this case would make the number of parameters equal to the number of cells and would thus make the so-called predicted means or percentages exactly equivalent to the actual ones: further, the addition of two interaction terms as in equation (3) of

my paper (which would be necessary to represent adequately the "cognitive dissonance" approach) would yield a model with five parameters but only four cells, for which there would be no solution.

4. In response to their claim that my paper addresses itself to "an almost incidental part" of their two articles, I remind them of the titles of those articles: "Upward Social Mobility and Political Orientation" (1967), and "The Political Orientation of Skidders: a Middle-Range Theory" (1970). While it is true that both papers contain much additional analysis and discussion to which I did not address myself, it would have been superfluous to do so since that subsequent work rested heavily on the validity of the initial "findings" that I did discuss (for further discussion see Goldberger, 1968:112-118).

5. While Lopreato and Chafetz do regret their inconsistent classification of one Republican from one article to another, they do not mention the inconsistent classification of the Christian Democrats, who comprise approximately fifteen percent of the sample analyzed in Tables 1 and 2 of my paper (the Christian Democrats were placed in the "center" category in the 1967 article and in the "right-wing" category in the 1970 article).

6. Regarding the occupational status scale, it is unfortunate that the detailed information on each individual's occupation was not recorded in the data set. While it is now questionable whether the scale can accurately be described as essentially similar to a measure of subjective social class, it is not clear what theoretical or empirical rationale did inform its construction. If the respondent's self-location on the occupational question quoted from the codebook (which appears *verbatim* [in Italian] in the original questionnaire used by DOXA) was not used in estimating respondent's occupational ranking, then why was it asked and why was it alone recorded in the codebook? We still have no information on how many criteria were used to construct the scale, on how (or why) the occupations were ranked on each criterion separately, or on how and on what grounds the various criteria were weighted when summed to form the scale reported in the data. Lopreato and Chafetz claim the help of Italian researchers and scholars in the construction of their scale but they cite no specific literature on occupational status in Italy to justify their occupational ranking. This method of scale construction is no less arbitrary for the Italian case than it would be for the American case were one to rely on the intuitive judgment of a few similarly placed native American scholars

* I would like to thank Warren Hagstrom and Robert Jackman for their comments on my first draft.

without reference to the relevant literature. Had respondent's detailed occupation been recorded in the data, future researchers would be free to construct an acceptably rigorous scale, rather than having to depend on a scale whose intuitive *ad hoc* qualities they may find unsatisfactory.

7. Lopreato and Chafetz misunderstand my use of the term "party affiliation": in line with its general usage, I used it to imply *psychological* affiliation, or party preference, and not card-carrying membership. Their further argument that the question "In your opinion, which party in Italy has best at heart the interests of people like you?" is tapping perceived Marxian interests and not party preference is a trivial one: had respondents been asked to name the parties that best represented the interests of various specifically named economic groups, their argument might be more valid, but since respondents were asked to respond in terms of their *own* perceived, *unspecified* interests, there is no assurance that they are using "Marxian" economic self-interest as a framework in answering the question. The essential point is that recent research by Converse (1964) and McClosky (1960), among others, indicates that partisanship, however conceived, is a poor reflection of ideology.

8. Lopreato and Chafetz are correct in their supposition that cognitive dissonance theory involves the resolution of two inconsistent cognitions: the cognitive dissonance theory of mobility effects reasons that an individual, having perceived his personal success or failure in society relative to his father (cognition 1), will develop a generalized attitude toward the political system consistent with his personal experience (cognition 2). The individual's perception of social mobility is an assumed intervening variable between objective social mobility and attitude toward the political system.

9. Finally, Lopreato and Chafetz accuse me of erroneously alleging that the cognitive dissonance theory of mobility effects "informed" their (1970) work. They misunderstand me completely: my essential criticism was rather, that while they did draw on Wilensky's theory (1966) to help account for their findings (1970: 442), that neither a cognitive dissonance nor indeed any other particular organizing framework informed their analysis of social mobility and political orientation in Italy.

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SOME COMMENTS ON "THE CHANGING SOUTH: NATIONAL INCORPORATION OF A REGION"

DAVID F. SLY AND ROBERT H. WELLER
Florida State University

McKinney and Bourque (1971:399-412) expound on the often documented thesis that the South has been experiencing dramatic social change and rapidly integrating with the rest of the country. Although it is difficult to disagree that the South is changing, we wish to point out theoretical inadequacies in their approach, call attention to errors in tables, and raise several methodological issues.

McKinney and Bourque attempt to place the entire question of regional integration within systems theory. However, in so doing they commit the fallacy of assuming that similarity in socio-economic characteristics implies similarity in values, attitudes and norms. Hence, although the changes that they argue have taken place are almost exclusively cultural and behavioral, the indices they use and the descriptions provided are

exclusively structural: urbanization, industrialization, occupational redistribution, income, and education. However, behavioral and cultural aspects of social behavior can and do vary independently of the institutional and structural aspects of society (Duncan and Schnore, 1959; and van den Berghe, 1967). Thus it does not necessarily follow from their analysis that "the South is becoming increasingly integrated into the larger United States' social system" (p. 409). This lack of logical necessity seems particularly manifest in McKinney and Bourque's assertion (p. 400) that a social system is "sustained by established norms, beliefs, values and sentiments." To arrive at norms, beliefs, values and sentiments from socioeconomic and demographic variables unduly stretches the laws of inference.

Observe their discussion (pp. 407-408) of the role of education as "a major factor in the generalizing of the nation . . ." They say "physics is physics and mathematics is mathematics," but we know that the fertility values, attitudes and norms of a Catholic woman educated in a Catholic college differ greatly from those of a Catholic woman educated in a non-Catholic college (Westoff and Potvin, 1967). We also know that race and fertility are differently associated in the South than in the non-South (Sly, 1970). McKinney and Bourque may also believe that "history is history," but how many non-Southern children learn about "The War Between the States?" Or, "Is the governor of Mississippi identical in values, attitudes and norms to the governor of New York?" We think not. Yet McKinney and Bourque write (p. 408) that ". . . there is good reason to believe that to the extent the daily occupational and educational environment of the Southerner becomes similar to that of the non-Southerner, the attitudes and values of the two will also become indistinguishable."

McKinney and Bourque argue further that, as a system develops, the various subsystems should grow to resemble the larger system. However, the larger system is by definition relatively self-supporting. Hence, as a particular region comes to resemble its larger system it increases its capacity to be self-supporting. Hence, the South's increasing structural similarity to the non-South

does not necessarily mean its increasing integration into the larger nation. It could also mean the development of structural pluralism. This is an empirical question which requires data additional to that analyzed by McKinney and Bourque.

Thus, we question the appropriateness of their data to their theoretical model. In addition we wish to 1) point out errors that appear in their tables, 2) question the comparability of their data, and 3) question methodologically their manner of computing "partial" indices of dissimilarity.

Let us begin with their Table 3. For reasons outlined below, we believe this table should not have included 1968 data. Once included, the data should have been presented correctly. For instance, the non-South data for 1968 total 71.52 percent. Given the authors' percent distribution, the ID could be either 14.31 or 28.62 depending on

$$\text{whether } ID = \frac{1940-1960}{2} \text{ or } ID = \Sigma(+\Delta's).$$

In either case, the reported ID must have been derived from another percent distribution, presumably one totaling 100.0. Also, the 1960 ID should be 11.1 instead of 9.4.

The first column in Table 4 totals 110.0 percent, because "clerical and kindred" should be 5.7 instead of 15.7. Based on correct figures, the ID for the 1940, 1960 South comparison is 29.0 rather than 29.9. Also, the citation given for Table 4 caused considerable difficulty. Its source title (Table 127) is "Occupation Group of Employed Females, by Regions, Divisions, and States: 1960." Those wishing to replicate the table would find Table 89 of the United States Summary volume and Table 59 of the respective state volumes more helpful than McKinney's and Bourque's citation. Moreover, according to Table 5, per capita personal income is, and always has been, higher in the South than in the non-South.

The 1968 data in Table 3 cannot be compared with the census data for 1940, 1950, and 1960. Apparently, it is derived from two sources. One source (U. S. Bureau of the Census, 1969b, Table 316, p. 216) reports only employees in nonagricultural establishments. The other (U. S. Bureau of the Census, 1969a, Tables 1A, 1B and 2A, pp.

4-79—page 3, cited by McKinney and Bourque, which contains an attractive map) omits the category "Public Administration." McKinney and Bourque's Table 3 contains both agricultural industries and public administration; therefore, their 1968 figures derive from a combination of 1969a and 1969b. Unfortunately, they fail to describe their exact procedures. Yet, consider the following problems in combining these sources. The data in 1969b (*Statistical Abstract*) cover all full and part-time employees who worked during, or received pay for, any part of the pay period reported. If the authors used Table 316 as they claim, the pay period is the year of 1968. However, the figures in source 1969a refer only to the number of employees in the mid-March pay period.

This causes part of the discrepancy between the 1969a and 1969b figures. For instance, source 1969b for Alabama lists 54,000 persons in "transportation and public utilities"; source 1969a lists only 48,058 for "transportation and other public utilities." A glance at the U. S. totals shows that these sources of data are not comparable. Source 1969b (Table 316), shows 68,146,000 persons employed in nonagricultural U. S. industries in 1968. Yet Table 1A, source 1969a lists 54,446,130 employees in *all* industries. Clearly something is amiss. Even the March, 1968 figures in source 1969b (66,713,000) exceed the 1969a source total which, because it includes agricultural and nonagricultural industries, should refer to a more inclusive universe.

Hence, we question the use of the 1968 data. McKinney and Bourque might have presented the figures despite their shortcomings, but they should have included a description of their derivation methods and a warning against an uncritical acceptance of them.

The fifth column of Table 3 is inaccurately labeled, "% change '68/'40." To take this heading at face value would mean that agriculture had declined by 29.9% between 1940 and 1968 in the South. Yet the figures show that this column is the ratio of the 1968 to the 1940 proportions of the labor force in agriculture. Thus, the 1968 data signify a 70.1% decline in the relative contribution of agriculture in the South for

this interval and not the 29.9% decline that McKinney and Bourque's label indicates.

Next we would change the treatment in this table of "partial" dissimilarity indices. As heretofore, the authors do not state an exact meaning for "partial" ID's. Their discussion in note 2 indicates that they consider partial ID's analogous to partial correlation coefficients. If so, partial ID's could be used to control for various factors, e.g., agricultural activity.

McKinney and Bourque apparently computed their ID's by: finding the proportion difference for each labor force in agriculture, multiplying it by .5, and subtracting this product from the overall ID. This procedure is methodologically deficient. The ID assumes that the distribution categories are equal, each percentage distribution sums to 100, and the sum of the negative differences equals the positive differences. Their first step violates the last two assumptions and their last step violates the first.

We need a method of computing partial ID's which would be uninfluenced by differences in the relative presence of another variable, e.g., agriculture.

One method that allows scores ranging from 0 for complete similarity to 100 for complete dissimilarity is as follows: (1) calculate a distributing factor, 100 divided by the percent of the total distribution in non-agricultural activities; (2) multiply the original proportion in each nonagricultural industry category by the distributing factor. This will inflate the original figures in non-agricultural industries to 100%; and (3) compute an ID with the transformed data. This method yields a partial ID ranging from 0 to 100, uninfluenced by variations in the relative presence of agriculture.

We have derived partial ID's between the South and the non-South of 8.5 in 1940, 8.8 in 1950 and 7.9 in 1960 in this manner. The figures suggest that the South and non-South's increasing similarity is due to the withdrawal of Southern employees from agriculture. With that change controlled, the South's labor force is no more like the non-South's for 1960 than 1940. In fact, we were struck by the minimal original difference, with the exception of agriculture, between South and non-South industrial composition.

When these procedures are used to calculate partial ID's for Table 4, the 1940-1960 South comparison index is 12.2, the non-South 10.2. Minus the influence of changes in farm related occupations, the occupational composition of the non-South has changed as much since 1940 as the South's. The South-non-South partial ID's for 1940, 1950, and 1960 are 10.4, 6.6, and 6.5 respectively. These figures support the conclusions drawn from within-region comparisons over time. They also validate McKinney and Bourque's statement on p. 404 that: "Partial indices of dissimilarity in Tables 3 and 4 would indicate that almost all of the difference in occupational distribution between the two areas is due to differences in proportion of workers in agriculture."

Our final comments concern the type of data presented in Tables 5 and 6. McKinney and Bourque's treatment of income would inform more had they supplemented the average income table for South and non-South with one that contained the distribution throughout the various income categories. Though we lack comparable data for 1940, Table 67 of the 1960 U. S. State Census reports contains income percentage distribution for fourteen categories of persons by color, sex and rural-urban residence for 1949 and 1959. Table 47 of these reports contains the number of school years completed by color, sex, and rural-urban residence for all persons twenty-five years old and over for 1940, 1950, and 1960. Had these distributions been presented with the average figures, we could compare more completely McKinney's and Bourque's two areas and make similar analyses of the occupational and industrial composition.

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FURTHER COMMENTS ON "THE CHANGING SOUTH": A RESPONSE TO SLY AND WELLER

JOHN C. MCKINNEY
Duke University

LINDA BROOKOVER BOURQUE
California State College, Los Angeles

The array of critical remarks, commentaries, and suggestions directed toward our paper by Sly and Weller encompasses quite a variety of assumptions and interpretations on their part and requires some corrections of errors on our part. It seems appropriate to categorize their comments into two broad areas: (1) theoretical considerations; and (2) methodological and technical considerations. In view of space limitations we will organize and present our response within these categories.

(1) THEORETICAL CONSIDERATIONS. When one brushes aside such irrelevancies as the question, "Is the Governor of Mississippi identical in values, attitudes and norms to the Governor of New York?" he gets down to what appears to be the fundamental question raised by Sly and Weller: the appropriateness of our data for the theoretical model used in the analysis. There is, of course, no easy answer to such a question. We can assert, however, that the "model" derives from a long-standing sociological tradition (structural-functionalism), the "data" are standard (census materials and the extensive literatures of such specialties as occupational, industrial, and educational sociology), the use of "indicators" is conventional, and the "interpretation" flows rather naturally from the preceding elements. Sly and Weller indicate a remarkable unawareness of the massive pertinent literature of this tradition of analysis. Moreover, they base their case

on a, "reified" separation of behavioral and cultural aspects of social behavior from institutional and structural aspects. This is an arbitrary and artificial separation which is pragmatically useful for certain kinds of empirical endeavor and analytic purposes. However, it does *not* represent the perspective and theoretical stance of our paper, which is based on quite a different set of premises and assumptions. Under the circumstances, an explication of our perspective seems warranted. We will proceed by specifying what we mean by social systems, social structure, and indicators.

Social systems. Within the context of action theory, the notion of the *social system* is a powerful ordering construct in sociology (Parsons, 1951). A familiar distinction must be made between the social system as an intellectual construct, model, or type of system, and social systems as "going concerns", as phenomena. All systems of real life,—e.g., a particular society, community, corporation, or family,—are *open systems*. They are interacting with their environment, which includes other social systems, in many and varied ways. Empirically, we recognize that these interactive foci or relational clusters which we view as social systems are always involved with, and interlinked with, other social systems. Their autonomy and isolation are always relative rather than absolute, and *de facto* they are always, to varying degrees and in widely varied ways, a part of a more extensive network of social relationships within which they are nested.

However, when analyzing social systems, it is necessary to assume an intellectual construct of social system. This construct, when applied to particular real life systems, must assume that the operation of the system being analyzed is affected only by specifiable conditions previously established in the environment and by relations among the system's elements obtaining at the time of the analysis. The construct social system must, on the one hand, be broad enough to encompass all relational clusters conceivable as social systems despite variations in type, and, on the other hand, be always so delimited that it never completely or perfectly represents the complexities, actualities, and uniqueness of any social system as an active

going concern. There is nothing unusual about this; it is in the nature of all constructs and analysis. Our ability to use the construct of social system, particularly in comparative work, is contingent on our ability to distinguish similarities and differences between, and within, definable systems. This requires specific application of the intellectual construct to empirical phenomena. In our paper we specified the conterminous United States as a social system and the "Confederate" South as a sub-system for purposes of comparison and analysis.

Social structure. Inherent in every social system is a structure. This structure evolves from daily interactions and is solidified in the form of institutions. The individual moves through the structure (and hence the social system) in a series of roles which he learns through observing, interacting with, and reflecting on the behavior of others. He learns these roles during the process of socialization.

In this view the individual becomes an object to himself and thus a self, only by taking the attitudes of other individuals toward himself, by assuming the roles of others, and regarding himself from their perspective. He becomes aware of himself as others are. This is accomplished through the mechanism of communication which is essential to the emergence and maintenance of social organization. The individual does not at first experience himself directly as subject, but only indirectly from the particular standpoints of individual members of the social system(s) to which he belongs. He develops a sense of self within the context of social relations and the experiences they involve.

Communication is paramount in this process in that it enables the individual to become an object to himself within a social environment. The reflexive form in language, in which the self is recognized as both subject and object, reflects this process. In addition to allowing the objectification of the self, communication implies interaction. Communication is not something that can go on by itself but must always involve a communicator, a receiver, and a message. It is a medium for such basic cooperative activities as exchange and assistance.

For most purposes, at least of micro-sociology, the most significant unit of the social system is the role, of which role expectations are the primary ingredient. Historically, role has been defined in various ways; one definition delineates it as "that sector of an actor's orientation which constitutes and defines his participation in an interactive process" (Parsons, 1951:5-6). Social roles have also been defined as "typifications of the attitudes and patterns of action ascribed to a category of social actors on the basis of a given trait which they have—or are believed to have—in common" (Offenbacher, 1967:5). Role is a category of constructs or typifications by which men orient themselves in their environment. Each individual is involved as a participant in a plurality of patterned interactive relationships or roles. Each role participation implies within itself communication, interaction, and continual redefinitions of self.

Roles, of course, vary in their degree of institutionalization and the degree to which they are common to members of the society at large. Parsons conceives of institutions as constituting the main link between the social system and the actor, in that they are at the same time related to the functional needs of actors and to those of the system (Parsons, 1949:34-36). The link evolves from the normative-voluntaristic aspect of the structure of action. The roles that individuals play in a social system are defined in terms of goals and standards; stated differently, in terms of typical objects of action and typical modes of conducting the action. From the point of view of the actor, then, his roles are defined by the normative expectations of the members of the group as they are formulated by the cultural tradition.

Thus, in a social structure one has a system of patterned expectations defining the *proper* behavior of actors in specified roles: typical behavior to be enacted under typical circumstances as typically perceived within a social system. This system is positively enforced both by the individual's own motives for conformity and by the sanctions of others. These typical and well-established patterns of expectations in the perspective of a social system are our institutions. These

institutions constitute the structurally stable element of social systems, and their prime function is to preserve role definitions of the constituent individual. Viewed functionally, institutionalized roles constitute the mechanisms by which varied human tendencies become integrated into a system capable of dealing with the problems of society and its members. Moreover, this social structure has been *constructed* by actors in interaction over time. This construction activity is a natural aspect of the social process and in part is a process of typification.

Typification as a central feature of cognition is the development of a selective and persistent attitude of an actor toward his environment which evolves primarily during the socialization process. Typification is a process by which the field of action is organized so that interaction can proceed on an orderly basis. Typification, perceiving the world and structuring it by means of categorical types, appears to be an essential and intrinsic aspect of an actor's basic orientation to his situation. (McKinney, 1969:5). However, it is important to note that these typifications are never "frozen." They undergo continuous reconstruction over time in the social transactions involved in the conduct of action. Clearly, this is a very different view of social structure than that implied by Sly and Weller. From this perspective it is virtually impossible to separate cultural and behavioral factors from structural factors. The existence of one always assumes or implies the existence of the others whether explicitly stated or not. The indicators we chose to use in our paper, while structural in nature, imply the existence of behavioral and cultural variables.

Indicators. At this point it is important to note the commonplace distinction between variates characteristic of individuals and those characteristic of collectives. It is obvious that collectives as well as individuals can be the elements of empirical propositions. Pragmatically, we mean by a collective any combination of individuals which forms a meaningful unit in the frame of a specific inquiry. This can be a small group assembled for the purpose of an experiment, a family, a corporation, a bureau, a region, a

nation, etc. The distinction Lazarsfeld makes concerning collective variates is useful here (Lazarsfeld, 1969). He distinguishes *aggregative* from *global* variates and indicates that both are forms of the *collective* variate. A collective variate is aggregative if it is based on information about each member of the collective under study. In contrast, the variate is global if it has emanated from the collective without our knowing or caring which or how individual members have contributed to it. For instance, when one characterizes states by strict or lenient divorce laws, or nations by varied voting procedures, one is using global variates derived from information unavailable by investigating characteristics of each member of the population. In contrast, the proportion of the population which is divorced is an aggregative variate of a collective, as is the proportion of votes in a country. This distinction between aggregative and global variates is important because sociologists generally have global variates in mind when they refer to *structural characteristics*. On the other hand, since both aggregative and global variates are *collective* it is legitimate to treat both as structural characteristics.

An important key here is that our disclosure of social structure does not assume that society is an aggregate phenomenon, that individuals are interchangeable units, and that individuals can like or love each other equally, can hold esteem or express respect for one another equally, or associate in various task performances at random. On the contrary, it assumes that the same individuals can prefer to associate with very different individuals for different purposes in different action contexts. In describing social structure as consisting in typical behavior to be enacted under typical circumstances as typically perceived within a social system, we have implicitly acknowledged the properties of individual variability, contextual variance, and role differentiation.

In effect, what we have done is used a set of collective variates, imputed them to social systems as characteristics, and treated them as *social indicators* of those systems. In an article published since ours, Land has defined social indicators as follows:

I propose that the term *social indicators* refer to social statistics that (1) are components in a social system model (including sociopsychological, economic, demographic, and ecological) or of some particular segment or process thereof, (2) can be collected and analyzed at various times and accumulated into a time-series, and (3) can be aggregated or disaggregated to levels appropriate to the specifications of the model. Social system model means conceptions of social processes, whether formulated verbally, logically, mathematically, or in computer simulation form. The important point is that the criterion for classifying a social statistic as a social indicator is its *informative value* which derives from its empirically verified nexus in a conceptualization of a social process. (Land, 1971:323).

In our article, we have selected five indicators: (1) urbanization, (2) industrialization, (3) occupational redistribution, (4) income, and (5) education, representing certain structural characteristics of a social system. We would maintain that these are not only conventional collective variates (witness the extensive "modernization" and "change" literature using them), they are *indicators* of systems in process and have, with respect to Land's stipulation, *informative value*. Given our above assumptions with respect to social systems and social structure their use as indicators not only seems warranted but mandatory.

(2) **METHODOLOGICAL AND TECHNICAL CONSIDERATIONS.** With respect to the inclusion of certain tabular errors, Sly and Weller are absolutely correct. There are errors in both Tables 3 and 4. Table 3's data for 1940, 1950, and 1960 came from the 1960 *Census of Population* by States, Social and Economic Characteristics, Table 62. All of the 1968 data, except that for "Agriculture" and "Forestry and Fishing" were obtained from *Statistical Abstracts* 1969, Table 316, p. 216. "Forestry and Fishing" data are from the 1968 *County Business Patterns*, United States Department of Commerce (Mid-March Statistics), Table 1a, p. 3 in the U.S. summary and individual state volumes. "Agricultural" data are from the "Farm Labor," United States Department of Agriculture, Statistical Reporting Service, LA1 (1-69). The latter citation was inadvertently dropped.

We added the 1968 data shortly before the final draft of the paper was completed. Our intent was to obtain data as recent as possible even though, in the process, a certain degree of comparability was sacrificed. We are unable to reconstruct the exact process by which the incorrect percentages for the Non-South in 1968 were obtained. We thought they were probably computed using an incorrect estimate of the total United States population as the base. Correcting the percentages changes the values of a number of other computations in the table (see the corrected version of Table 3). Finally in redoing the table, we discovered that lines (b) and (c) in the original table were reversed. This and certain other miscalculations are corrected in the revised version.

An uncorrected version of Table 4 was mistakenly included with the final draft of the paper. The correct final version of Table 4 is included here. Please note that in addition to the change in the citation and the correction of the value for "Clerical and Kindred" in 1940, the Index of Dissimilarity (line (b)) had never been divided by two in the original published version of Table 4. This latter error may, in part, explain Sly and Weller's confusion about the use of the index of dissimilarity.

Certainly the most significant methodological observation in Sly and Weller's "Comment" is their discussion of our use of partial indices of dissimilarity. An index of dissimilarity is a descriptive statistic. The advantage of the "partial" index of dissimilarity is that it tells you how much of the total "redistribution" is within the collapsed categories as opposed to how much is between them. In this sense, the partialing procedure is rather more similar to an analysis of variance than to a partial correlation. In this instance, our intention was not primarily to "control" for agricultural activity, but rather simply to "describe" or indicate the way in which discrepancies between agricultural and non-agricultural activities explain the discrepancies in industrial and occupational distributions between the South and Non-South.

It is not clear to us that Sly and Weller's technique is any more appropriate than (or

indeed as appropriate as) our technique—either for "controlling" for the presence of agriculture or for simply "describing" what happens when agricultural activities are deleted from an occupational or industrial distribution. Sly and Weller's technique essentially deletes agriculture from the occupational distribution categories. It assumes that persons who move out of agricultural activities will divide themselves proportionally among the other categories according to the occupational or industrial distribution or weighting which presently exists for each category. As Sly and Weller point out, and as we point out on pages 403 and 404 of the original article, the deletion of agricultural activities by whatever method essentially causes the distribution of the two areas to appear much more similar. In no sense does *either* method give us the ability to predict the real world effects of a trend toward equalization of agriculture in the two regions.

When we originally developed the article, we included a table in which we did exactly what Sly and Weller suggest. However, space limitations and some concern with the implications of such a table caused us to delete it. The assumption attached to Sly and Weller's technique—that persons who leave agricultural activities will distribute themselves proportionally among other occupations—seems fallacious. Our data would indicate that this does not occur. Evidence from Table 3 suggests that persons most often move into manufacturing when they leave agriculture. Consequently we deleted the table.

The remaining comments on the data appear to us to be minor or a matter of personal preference. The headings on Table 5 were reversed during the publication process. The choice of data included in Tables 5 and 6 strikes us as a matter of personal preference. In addition, there is the problem of space available and efficiency. It is, after all, impossible to include all data that might conceivably be relevant.

In Sum. In general, it is our impression that neither the corrections in Tables 3 and 4, or the choice of partialing procedures in any way changes the thrust of the original article. We are pleased to have the opportunity to make these essential corrections. We place little value on the general

Table 3. (Corrected Version) Percentage Distribution of the Population in Industrial Categories for the South and Non-South, 1940-1968 (Population in Millions).

Industry	Year				% Change '68/'40	Index of Dissimi- larity '40-'68*
	'40	'50	'60	'68		
South:						
Agriculture	34.8%	21.7%	10.2%	10.3%	29.6	32.0
Forestry & Fishing	.5	.4	.3	.05	10.0	
Mining	2.0	2.0	1.6	1.4	70.0	
Contract Construction	4.4	6.8	7.1	5.6	127.3	
Manufacturing	16.0	18.7	21.8	24.7	154.4	
Transp., Commun., Pub. Util.	5.3	6.8	6.5	5.8	109.4	
Wholesale & Retail Trade	13.2	17.3	19.0	19.0	143.9	
Finance, Insurance, Real Est.	1.9	2.4	3.4	4.1	215.8	
Service & Miscellaneous	18.3	18.3	21.8	12.5	68.3	
Public Administration	2.3	4.0	4.9	16.6	721.7	
Industry, not reported	1.3	1.6	3.4		261.5**	
Population	11	14	16	18		
Non-South:						
Agriculture	13.4%	9.2%	5.4%	5.3%	39.6	16.3
Forestry & Fishing	.2	.2	.1	.02	10.0	
Mining	2.1	1.5	.8	.7	33.3	
Contract Construction	4.7	5.9	5.5	4.1	87.2	
Manufacturing	26.3	28.4	28.8	27.8	105.7	
Transp., Commun., Publ. Util.	7.5	8.2	7.0	6.0	80.0	
Wholesale & Retail Trade	17.9	19.1	18.0	19.5	108.9	
Finance, Insurance, Real Est.	3.7	3.7	4.4	4.8	129.7	
Service & Miscellaneous	19.4	17.7	20.8	15.0	77.3	
Public Administration	3.4	4.6	5.0	16.8	494.1	
Industry, not reported	1.7	1.5	4.2		247.1**	
Population	34	43	49	55		

(a) Index of Dissimilarity between South and Non-South	21.9 ('40)	14.8 ('50)	9.4 ('60)	7.1 ('68)
(b) Index of Dissimilarity for Agriculture	10.7 ('40)	6.3 ('50)	2.4 ('60)	2.5 ('68)
(c) Difference between (a) & (b)	11.0 ('40)	8.5 ('50)	7.0 ('60)	4.6 ('68)

*In order to increase the comparability of 1940 and 1968 data, the "Industry, not reported" category was deleted for 1940 and the percentage distribution recomputed prior to computing the Index of Dissimilarity.

**Since the category "Industry, not reported" does not exist for 1968, percentages were computed comparing 1960 and 1940.

- Sources: 1) U.S. Department of Commerce Bureau of the Census, United States Census of Population 1960, General Social and Economic Characteristics, PC(1)-2C, 5C, 11C, 12C, 19C, 20C, 26C, 35C, 42C, 44C, 45C, and 48C, Table 62, Industry Group of Employed Persons, for the State: 1940-1960.
- 2) U.S. Department of Commerce Bureau of the Census, 1960 Census of Population, Volume 1, Characteristics of the Population 1960, Part 1, U.S. Summary, "C - General Social and Economic Characteristics," Table 92, Industry Group of Employed Persons, for the U.S.: 1940-1960, p. 1-223.
- 3) U.S. Department of Commerce Bureau of the Census, Statistical Abstract of the United States 1969, "Labor Force, Employment, and Earnings," Table 316, Employees in Non-agricultural Establishments, Annual Averages - States: 1960, 1965, and 1968, p. 216.
- 4) U.S. Department of Commerce Bureau of the Census, United States Census, County Business Patterns, CBP - (68)-1, 2, 5, 11, 12, 19, 20, 26, 35, 42, 44, 45, and 48 (Mid-March Statistics), Table 1a, State: 1968 and 1967.
- 5) United States Department of Agriculture, Statistical Reporting Service, LAL, (1-69), Farm Labor, "Workers on Farms: Average January through December 1968," p. 5.

Table 4. (Corrected Version) Occupational Distribution of the Population in the South and Non-South, 1940-1960 (Population in Millions).

Occupation	South				Non-South			
	1940 (%)	1950 (%)	1960 (%)	ID '40- '60	1940 (%)	1950 (%)	1960 (%)	ID '40- '60
Profess., Tech., & Kindred	5.7	7.0	9.5		8.7	9.3	11.7	
Farmers & Farm Managers	20.4	13.5	5.5		8.3	5.8	3.4	
Managers, Officials & Propri. except Farm	6.4	7.9	8.5		8.6	9.2	8.3	
Clerical & Kindred	5.7	8.8	11.7		11.1	13.5	15.3	
Sales Workers	5.1	6.3	6.9		7.4	7.1	7.3	
Craftsmen, Foremen & Kindred	7.8	11.6	12.5	29.0	12.8	14.6	13.8	13.1
Operatives & Kindred	14.1	17.9	18.5		19.3	20.5	18.4	
Private House, Workers	7.0	4.2	4.7		3.8	1.9	2.0	
Service, exc. Private House	5.4	6.6	8.0		7.7	7.9	8.5	
Farm Laborers & Foremen	13.9	7.8	4.0		4.6	3.1	1.7	
Laborers, exc. Farm & Mine	7.7	7.0	5.8		6.7	5.8	4.5	
Occupation not Reported	.8	1.4	4.4		1.0	1.3	5.1	
Population	11	14	16		34	43	49	
(a) Index of Dissimilarity between South & Non-South		25.6 (1940)		16.0 (1950)			8.7 (1960)	
(b) Index of Dissimilarity between Farm Related Occupations & Non-Farm Related Occupations		10.7 (1940)		6.2 (1950)			2.2 (1960)	
(c) Difference between (a) & (b)		14.9 (1940)		9.8 (1950)			6.5 (1960)	

Sources: 1) U.S. Department of Commerce Bureau of the Census, 1960 Census of Population, Volume 1, Characteristics of the Population, Part 1, United States Summary, "C - General Social and Economic Characteristics," Table 89, Occupation Group of Employed Persons by Sex, for the United States: 1940 to 1960, p. 1-219.
 2) U.S. Department of Commerce Bureau of the Census, United States Census of Population 1950, General Social and Economic Characteristics, PC (1)-2C, 5C, 11C, 12C, 19C, 20C, 26C, 35C, 42C, 48C, Table 57, Occupation Groups of Employed Persons, By Sex, For the State, 1940-1960.

theoretical and methodological criticisms presented by Sly and Weller since it seems to us that they are largely irrelevant to the analysis and represent a set of assumptions and premises quite foreign to those actually used.

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SOME COMMENTS ON BIOLOGICAL MODELS OF SOCIAL CHANGE

RICHARD A. BERK

Northwestern University

One of the problems with the discipline of sociology is its vulnerability to fads. Frequently these fads are generated from notions whose appropriate use would bring benefit to many social scientists. And, ironically, it is often those fads generated from the best seminal ideas that create the most misleading of "pop" sociologies. Witness the history of computer technology, math models, and radical sociology.

"Biological Models of Social Change" is just the kind of paper that can feed into this tendency to adopt uncritically the newest and flashiest approaches to difficult problems. To begin with, the paper has several characteristics that make it extremely attractive, even seductive. First, it makes many interesting points that probably could be further operationalized and tested. Second, the systems approach gives the paper a kind of parsimony too often absent in social change literature. Third, the obvious utility of systems approaches in other disciplines argues for its potential power in sociology. Fourth, the application of systems analysis and biological models necessarily gives the paper a "hard science" flavor attractive to many researchers.

But besides being a positive statement of possible uses for systems theory and biological models, the paper fails to acknowledge the existence of many questions that logically precede application of the approach proposed. Though Nisbet's arguments may have been refuted, more fundamental problems are largely ignored. The paper's coupling of the strong positive statement about the potential uses of systems theory and biological models with its failure to specify underlying assumptions and issues lulls one's critical sense. Even a brief summary of salient unresolved questions in systems would have encouraged healthy skepticism.

Such a summary could have begun with a formal definition of a system. For example:

A system can be defined as a complex of interacting elements. Interaction means that elements, p , stand in relations, R , so that behavior of an element p in R is different from its behavior in relation, R' . If the behaviors in R' are not different, there is no interaction, and the elements behave independently with respect to the relations R and R' . (Bertalanffy, 1968:56)

Clearly the definition demands that elements interact before they can be labeled a system, and this raises at least three crucial untreated

questions. First, it must be shown that a set of elements comprises an entity, because an entity must exist before one can consider whether the set is a system. Entity definition involves operationalization of boundaries as the paper notes. More fundamentally, it involves explicit choices of theoretical criteria specifying entity members, since operational processes imply theoretical positions. The suggestion that "identification of group members themselves" may be a useful way to establish group boundaries, builds in unstated theoretical assumptions about the nature of a group. More generally, one must wrestle with abstract notions of what an entity is. For example, Campbell discusses several criteria for defining an entity and ways to apply such definitions across all social sciences, at various levels of analysis. (Campbell, 1958) In short, before one can decide if societal elements comprise a system, one must establish theoretical criteria which clearly arrange the elements into an entity.

Second, it must be shown that at some level, elements in an analysis of social change fulfill a definition of a system. In other words, systems concepts should only be applied to systems. For some kinds of societal events, this can probably be done, though Spilerman argues that civil disorders, for example, may be understood in part through random processes. (Spilerman, 1971:427-442) Clearly, the jury is still out on the applicability of systems analyses to societal processes; and though the article correctly notes such problems, it attributes them mainly to vagueness in systems concepts rather than more fundamentally, to whether societal elements meet the criteria of a system.

Third, and most important because of the choice to concentrate "not on general system characteristics but on distinct processes which have been identified in biological systems," is the extent to which relations between elements in biological systems are similar to relations between elements in societal systems (assuming for the moment there are societal systems). If relations between elements in the two kinds of systems differ greatly, then sociological applicability of biological models is seriously challenged. For example, are the integrative mechanisms of a social class enough like those of a biological organism to lift such a comparison beyond a potentially misleading analogy? (Some researchers would feel uncomfortable with less than a homologous comparison.)

The above comments are not meant to imply that sociological researchers should abstain from trying the approaches proposed until the underlying issues are resolved. Indeed, an evaluation of their utility will only come

from attempted applications. However, social science authors are obligated to alert readers to underlying assumptions and issues. This rarely calls for a detailed account of the debates, but simply a few words of warning. For "Biological Models of Social Change" such additions would have been useful.

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REPLY TO BERK

I have little quarrel with the logical and theoretical analysis of systems theory which Dr. Berk presents. It supplements my own analysis. Where we differ, however, is in evaluating research strategy.

What is logically prior need not be studied first. It is precisely because most scientists have in studying systems gone first into the underlying conditions, that most research based on their approach has been sterile. For the concept of system to be useful, it should be important in understanding processes, comparing different kinds of changes, analyzing all the consequences of a system. That is, if assuming the presence of a system leads to fruitful insight, this approach is indicated. In deductive logic, to make this assumption is called the fallacy of "asserting the consequent." It is, however, the method of inductive logic and scientific procedure. In scientific work, we can not achieve the tautological certainty of deductive logic, but must work with probabilities which increase with the increase in established consequences.

The inordinate and exclusive concern with the logical presupposition of systems has prevented scientists from following up the system models for their interesting implications and has led to vague speculations about possible consequences. These practices make systems analysis a fad in sociology instead of a full theory and procedure. It is just this discrepancy between the promise of a theory and its lack of accomplishments which led me to write my article.

KURT W. BACK
Duke University

Items (Continued)

■ **Alan Booth** is Associate Professor of Sociology at the University of Nebraska. He is completing a study of the relationship between social integration and exposure to mass media, and recently has undertaken a comprehensive examination of the biological and social consequences of human crowding. **Hugh P. Whitt** is Assistant Professor of Sociology at the University of Nebraska (Lincoln). He is currently working on studies of aggression in families, violence in history, and the effects of migration on religion and values. **Charles C. Gordon** is Assistant Professor of Sociology at Carleton University (Ottawa). His research includes studies in formal organizations, environmental systems, and the interface between sociology and architecture. **John R. Hoffee** is Associate Professor of Sociology and Department Chairman at the University of Winnipeg. The author of a recent monograph, *Poverty in Canada*, and co-editor with John Harp of *Man on Relief*, he has recently returned from India, where he conducted research on caste and class.

■ **Samuel Leinhardt** is Assistant Professor at Carnegie-Mellon University. He is continuing research in the development of mathematical models of small-scale social systems. These efforts include the development of a dynamic model of structural change and evolution and attempts to specify the roles of non-sentimental variables in the determination of affect networks. He is also doing research in the sociology of health and education. The latter includes studies of ambulatory care delivery

systems, the effect of low level radiation on human health, and the development of mathematical models of accountability in public education systems.

■ **Mary R. Jackman** is a doctoral candidate in sociology at the University of Wisconsin (Madison). Her research interests are in the social-psychological consequences of social stratification; her current research deals with alternative models of racial and ethnic prejudice. Mrs. Jackman's paper is commented on by **Joseph Lopreato**, Professor of Sociology and Department Chairman at the University of Texas (Austin) and **Janet Saltzman Chavetz**, University of Houston.

■ **David F. Sly**, Assistant Professor, and **Robert H. Weller**, Associate Professor of Sociology, are both associated with the Population Studies Program at Florida State University. Their comment on "The Changing South: National Incorporation of a Region" is responded to by co-authors **John C. McKinney**, Vice Provost and Dean of the Graduate School at Duke University, and **Linda Brookover Bourque**, Assistant Professor of Sociology at California State College, Los Angeles. The issue concludes with a similar exchange between **Richard A. Berk**, Assistant Professor of Sociology and Urban Affairs at Northwestern University, and **Kurt W. Back**, Professor of Sociology and Acting Department Chairman at Duke University, concerning the latter's "Biological Models of Social Change."

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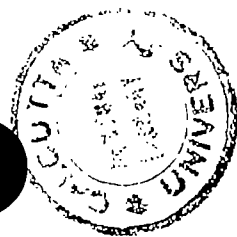
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Format of References in Appendix

List all items alphabetically by author and, within author, by year of publication, in an appendix, titled "REFERENCES." Use no italics and no abbreviations. For typing format, see the following examples:

Davis, K.

1963a "The theory of change and response in modern demographic history." *Population Index* 29(October):345–366.

1963b "Social demography." Pp. 204–221 in Bernard Berelson (ed.), *The Behavioral Sciences Today*. New York: Basic Books.

Goode, W. J.

1967 "The protection of the inept." *American Sociological Review* 32(February): 5–19.

Moore, Wilbert E., and Arnold S. Feldman.

1960 *Labor Commitment and Social Change in Developing Areas*. New York: Social Science Research Council.

Sanford, Nevitt (ed.)

1962 *The American College*. New York: Wiley.

STATUS CHARACTERISTICS AND SOCIAL INTERACTION*

JOSEPH BERGER, BERNARD P. COHEN AND MORRIS ZELDITCH, JR.

Stanford University

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This paper discusses the small groups literature on status organizing processes in decision-making groups whose members differ in external status. This literature demonstrates that status characteristics, such as age, sex, and race determine the distribution of participation, influence, and prestige among members of such groups. This effect is independent of any prior cultural belief in the relevance of the status characteristic to the task. To explain this result, we assume that status determines evaluations of, and performance-expectations for group members and hence the distribution of participation, influence, and prestige. We stipulate conditions sufficient to produce this effect. Further, to explain the fact that the effect is independent of prior cultural belief, we assume that a status characteristic becomes relevant in all situations except when it is culturally known to be irrelevant. Direct experiment supports each assumption in this explanation independently of the others. Subsequent work devoted to refining and extending the theory finds among other things that, given two equally relevant status characteristics, individuals combine all inconsistent status information rather than reduce its inconsistency. If this result survives further experiment it extends the theory on a straightforward basis to multi-characteristic status situations.

1. Background and statement of the problem

THIS paper studies how status characteristics organize social interaction. The problem concerning us is an old one in sociology. By 1908 Simmel was saying that, "The first condition of having to deal with somebody at all is to know with *whom* one has to deal" (Simmel, 1908; quoted from Wolff, 1950:307, italics in the original). While one might know this from direct experience of an individual, Simmel observed one might also know it from the individual's status category (Simmel, 1908; in Wolff, 1959:344-345). Twenty years after Simmel's *Soziologie*, Park already assumed a

conception of interaction in which one individual classified another by age, sex, race, and social type, and behaved towards him on the basis of these stereotypes (Park, 1928).

Many studies in the "small groups" literature deal with this problem: that is, they observe some interaction measure, such as participation or influence, in groups whose members differ in status categories. For example, Caudill finds that positions in the occupational hierarchy of a psychiatric hospital determine participation rates in ward rounds. The ward administrator participates more than the chief resident, the chief resident more than other residents, the most passive resident more than the most aggressive nurse (Caudill, 1958, ch. 10). Or Torrance finds that positions in a B-26 air crew influence group decisions. Pilots influence decisions more than navigators, navigators more than gunners; and this is true even when the pilot's opinion is objectively incorrect; and when the group task is unrelated to B-26 air crew activities (Torrance,

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1954). In juries, sex and occupation determine participation, election to foreman, and evaluation of juror competence (Strodtbeck, James and Hawkins, 1958; Strodtbeck and Mann, 1956). In biracial work groups whites initiate more interactions than blacks, talk more to other whites than blacks, and even blacks talk more to whites than other blacks (Katz, Goldston and Benjamin, 1958; Katz and Benjamin, 1960). Similar findings are reported in over a dozen investigations between 1950 and 1965 (in particular, see Croog, 1956; Heiss, 1962; Hoffman, Festinger and Lawrence, 1954; Hurwitz, Zander and Hymovich, 1960; Leik, 1963; Mishler and Tropp, 1956; Zander and Cohen, 1955; Ziller and Exline, 1958).

First, we will examine what all this research adds up to; that is, describe the phenomenon in general. Second, we will explain the results of this research; that is, show them as consequences of some more general theory. Part of the paper, therefore, constructs the required theory. Third, with the theory formulated, we review research designed to test, refine, and extend it.

2. Generalization of research results.

It is difficult to say what all this research adds up to for two reasons. First, the relevant variables are nowhere precisely conceived. The investigations all involve status differences, but none defines status difference precisely. They all involve some form of participation, prestige, or influence; but none defines precisely how these are related. Second, the concrete differences in these studies make it hard to determine their systematic import. Some treat formal positions in a hierarchy (chief of a service, resident, nurse; pilot, navigator, gunner); others treat personal reputations (prestige as experts on mental hygiene); still others treat community or social status (occupation, age, sex, race). The dependent variable might be participation rates, influence over decisions, perceptions of abilities, or performance. The task and interaction conditions differ most of all. They range from estimating the number of dots on a card to discussing damage awards in a jury trial; from conferences among professionals to coalition games; from familiar tasks, intimately related to group

purposes (mental hygiene specialists holding a mental hygiene conference) to tasks utterly foreign to their purpose (air crews constructing a story from a projective stimulus).

Nevertheless, these investigations have a number of common elements. First, they all deal with *some* form of status difference. Furthermore, their status categories (1) always appear to imply different evaluations of individuals, and (2) always provide the basis for inferring differences in an individual's capacities or characteristics. Status assumptions appear to be of two kinds: those dealing with specific abilities relevant in the interaction situation and those dealing with generally useful capacities. For example, they might relate to the ability to solve a math problem or to general intelligence. (A characteristic the states of which are differentially evaluated is a *status characteristic*; a status characteristic from which one infers general assumptions about individuals is a *diffuse status characteristic*.)

Second, the observable interaction in these studies can be conceptualized as follows: (1) Individuals either give or do not give *action opportunities* to others, as when one individual asks another for his opinion. (2) Given an action opportunity, individuals either do or do not contribute a *performance output* to the interaction of the group. (3) Given a performance output, others evaluate it, positively or negatively; in evaluating it they either communicate a *reward action* to another or they do not. Finally, (4) in exchanging views about the task, sometimes one individual is *influenced* by another, that is, changes his mind after differing in opinion with another, or he does not. All four aspects are highly intercorrelated: Bales, for example, found that those who initiate activity most often receive activity most often and tend to be ranked highest by group members on the criteria of who had the best ideas, who guided the group discussion, and who demonstrated leadership (Bales *et al.*, 1951; Bales and Slater, 1955). Strodtbeck found performance outputs highly correlated with influence (Strodtbeck, 1951). Therefore, though the investigations differ greatly in their dependent variables we assume that all reflect different behavioral consequences of one underlying structure and are related functions of that structure. (Collectively,

these four kinds of interaction can be called the *observable power and prestige order of the group*.¹

Third, though the studies' tasks, settings, and interaction conditions differ concretely, in all cases group members share a common task. Usually the group must make a decision; its members believe that a right and a wrong, or a good and a bad decision exists; and their purpose is to make the right, or good decision. Typically, they believe it is legitimate for one to base his opinion on another's if one believes the other is right. If a task has a right or good answer defined as "success" and a wrong or bad answer defined as "failure," it is a *valued* task; a task in which it is necessary or legitimate to use whatever opinion one believes is right is a *collective* task. A group oriented to a collective, valued task is a *task-focused* or *task-oriented* group.

Some of the ways in which these investigations differ are informative: An investigation like Caudill's study of hospital ward rounds involves a status structure embedded in the hospital's formal organization that is immediately and obviously defined as relevant to what takes place in ward rounds. But Torrance's study of air crews shows that the formal status structure operates not only in respect to its established purposes (such as solving an air crew survival problem), but in respect to a totally non-relevant task (constructing projective stories). Furthermore, Hurwitz, Zander and Hymovitch (1960) show that informal status structures are as powerful as formally instituted ones in determining power and prestige orders. Thus, the effect of the status characteristic does not depend simply on formal sanctions and supports, nor on the access to resources that official position provides. Nor need the status structure evolve out of the interaction of group members, for Strodtbeck shows the same effects though his juries are *ad hoc*. (Indeed, some evidence shows that categories like age, sex, occupation, education, and race work best when the group has *not*

had a long history of interaction as a group.) Thus, all these investigations reveal a status structure, sometimes formal, sometimes informal; sometimes having prior associations with the group task, sometimes not.

The principal findings of this research may be reduced to the following empirical generalization:

When a task-oriented group is differentiated with respect to some external status characteristic, this status difference determines the observable power and prestige within the group whether or not the external status characteristic is related to the group task.

3. *The problem of explanation and constructing a theory.*²

Our problem now is how to derive the above generalization from some more general theoretical formulation. To be useful, that is to increase understanding of this phenomenon and provide a basis for refining and extending knowledge of it, this theory should satisfy at least the following four requirements: (1) It should stipulate the sufficient conditions for a status organizing process to occur. (2) It should specify what about status characteristics determines behavior. (3) It should specify what behaviors are determined by status characteristics. (4) It should describe the mechanisms by which status characteristics determine behavior.

We will formulate our theory from the point of view of an actor, *p*, oriented to at least two social objects, himself, *p'*, and another, *o*. *P* faces a task, *T*, with at least two outcomes, *T_a* and *T_b*, *differentially evaluated*: One outcome constitutes "success," the other, "failure." The individuals for whom *T* is a task are task-focused; that is, motivated to achieve the successful outcome. They are also *collectively-oriented*, that is, they believe it both legitimate and necessary to take others' behavior into account. It is assumed that possessing the state *x* of some performance characteristic *C** increases the individual's likelihood of achieving "success"

¹ In some cases, the dependent variable is not one of these behaviors, but is some behavior which can be shown to depend on them. For example, in Zander and Cohen (1955) the dependent variable is relative satisfaction with the group experience, apparently due to action opportunities and reward actions received by the higher status individuals.

² The theory presented here is part of a more comprehensive research program on expectation-states processes. This program consists of a number of theoretical formulations concerned with how expectation-states are formed and how they determine behavior in different types of social situations. (See, Berger *et al.* (ed.) forthcoming.)

at the task; while, possessing the complementary state of C^* increases the individual's likelihood of "failure" at the task. In other words, C^* is *instrumental* to T . (To simplify analysis, we treat all characteristics as dichotomies. Hence the characteristic C^* has only the states C_a^* and C_b^* . If they are differentially evaluated, we say that one is positively and one negatively evaluated.) Who possesses the state x of C^* may not be known to p . Call a task situation having these properties S^* . We assume that in S^* the social objects p' and o are described by states of a diffuse status characteristic, D , and only by D . A characteristic is a diffuse status characteristic for p if it has three properties: First, p believes that it is better to possess one state of D than another; that is, the states of D are differentially valued. Second, p associates with the states of D one or more specific expectations for behavior. Thus, he may expect that college professors speak a pure form of English, or that Jews are studious. The set of all such specific expectations associated with the state D_x we will denote γ_x . The states in γ_x , like the states of D , are valued, either positively or negatively. Third, p associates with each state x of D a *general expectation state*, GES_x , for example, that people who possess the state D_x are smart, or moral. Whereas, a specific expectation applies to a defined situation or class of situations, for a general expectation no specified situation or class of situation is defined. The belief that officers are gentlemen is an expectation of this kind. GES_x is an evaluated state, and has the same value as the state D_x with which it is associated.

Definition 1. (Diffuse status characteristic) A characteristic D is a *diffuse status characteristic* if and only if

1. The states of D are differentially evaluated
2. to each state x of D there corresponds a distinct set γ_x of states of specific, evaluated characteristics associated with D_x
3. to each state x of D there corresponds a distinct general expectation state, GES_x , having the same evaluation as the state D_x .

The task situation S^* , then, is one having a valued task T ; a characteristic C^* instrumental to T ; individuals task-focused and collectively-oriented; and individuals possessing the states of one, and only one, diffuse status characteristic D .

Status characteristics do not operate in every situation: We need, therefore, to formulate conditions under which individuals base their behavior toward one another on them. When they do we say the status characteristic is *activated*:

Definition 2. (Activation) D is *activated* in S^* if and only if p attributes in S^* the states GES_x and/or the sets of states γ_x to p' and o which are consistent with their states of D .

(By consistent we mean that the states have the same evaluation. Thus the positively evaluated general expectation state is consistent with the positively evaluated state of D .) In saying behavior in a specific situation is based on status-associated conceptions, we mean that p , in that situation, attributes to self and other the same evaluations and expectations attributed to the status classes of which they are members. If p is female and believes that males know more than females about anything legal, then we say that for p sex is activated in a jury if she believes that specific male jurors know more simply because they are male.

There have been almost no studies of activation. We know almost nothing about the conditions under which a status characteristic is activated.³ There are certainly situations in which some status characteristics that might be used to organize interaction are not: Why are they not? Because we do not know, the present formulation gives only one set of sufficient, but not exhaustive or necessary conditions. There may be more general activation conditions than those we assume. However, a status characteristic is at least brought into play if, first, p is in situation S^* and, second

³ Leik's research is exceptionally interesting in this regard. It suggests that the extent to which a status characteristic is brought into play in a given situation is affected (negatively) by the extent of prior acquaintance of the group members (Leik, 1963).

STATUS AND SOCIAL INTERACTION

(remembering that there is just one D in S*) if D is a social basis of discrimination between p' and o; if, that is, p' and o possess different states of D.

Assumption 1. (Activation) Given S*, if D in S* is a social basis of discrimination between p' and o then D is activated in S*.

(Note that we do not use "discrimination" pejoratively here, nor do we imply that p discriminates for or against someone in his actions. Whether he does or not, we intend to derive from other features of the situation; it does not follow tautologically from assumption 1.) Assumption 1, in conjunction with our definition of S*, says that a status characteristic is brought into play, at least, in task situations in which no prior expectations for the task or affectual relations among group members have been established, and in which a status characteristic discriminates some group members from others. In effect, this assumption raises such theoretical and experimental problems as whether and to what extent D is activated, for example, in purely social-emotional situations; whether and to what extent D is activated in equal status situations; whether all or only some status characteristics are activated in multi-characteristic status situations. (The latter two conditions are considered below in section 5.)

If a status characteristic is activated in situation S*, two situations may exist. First, the performance characteristic C* instrumental to the task in S* may have some prior association with D. For example, a male and a female may be required to perform a mechanical task which both may assume the male more able to do because that is the conventional view. Second, the performance characteristic instrumental to the task in S* may have no prior association with D. For example, a pilot and a gunner may be required to construct a fantasy from an ambiguous projective stimulus (Torrance, 1954). The ability to do so is probably not conventionally associated with air crew status.

In the first case, activating the status characteristic is sufficient to determine p's

assumptions about the specific abilities of p' and o: P associates the states of C* with those of D; and attributes them to p' and o in this situation.

The second case is less clear. For if the characteristic instrumental in S* has no prior association with its visible status characteristics, how will status differences imply anything for behavior? How is the status characteristic relevant in the particular situation? (By *relevance* we mean that if o possesses a given state of D, then p expects that o will possess a given state of C*.)

We make the following strong assumption about relevance: that in this situation the members of the group act as if the burden of proof is on showing that the status characteristic is *not* relevant; in other words, that it becomes relevant unless p knows from prior belief that it is not a basis for making inferences about the characteristic C*. Prior belief can, of course, establish the independence of two characteristics: p might believe that athletic ability and academic status are independent and will not be willing, nor believe himself able, to infer from the fact that p' is a professor and o a student either that p' is better than o or o better than p' as a basketball player. Two characteristics socially defined as independent we term *dissociated*. We assume that the status characteristic must be dissociated from the task characteristic required in S* to be irrelevant to it. Faced with a new task characteristic in a new situation, one about which p has formed no prior beliefs, nothing bars p from seeing the status characteristic as relevant. If nothing bars him, it will in our view become relevant and be used as a social basis for defining the new situation. (This we call the *expansive* property of D, and the process by which it defines new situations the *burden of proof* process.)⁴

We note the fact that if a status characteristic is activated in situation S*, its components will consist of an evaluated state x of D; a set of evaluated specific character-

⁴ The social conditions under which the burden of proof process does *not* operate, or its effect is modified, require further research. For example, to what extent is the effect of an initially non-relevant but activated status characteristic modified when task expectations already exist in a social situation?

istics γ_x ; and an evaluated general expectation state x of GES. Given S^* and D 's activation, p possesses or has had attributed to him one set of these components, say D_x , γ_x , and GES_x ; o possesses or has had attributed to him a second set of these components, say $D_{\bar{x}}$, $\gamma_{\bar{x}}$, and $GES_{\bar{x}}$. Furthermore, the elements of the two sets have different evaluations.

With respect to these components, we assume that:

Assumption 2. (Burden of proof) If D is activated in S^* and has not been previously dissociated from C^* , and if there is no other social basis of discrimination between p' and o , then at least one consistent component of D will become relevant to C^* in S^* .

This general assumption permits a number of different mechanisms by which D becomes relevant to C^* : GES, the evaluations attached to the states of D , or even the states in γ might become the basis for forming expectations in S^* . The important point is that if they have not been previously associated with the characteristic instrumental in S^* , but also not previously been socially defined as independent of it, then one or more components of the activated status characteristic become relevant to C^* .

That these components are relevant in S^* means that p expects p' and o to possess given states of C^* . If p assigns states of C^* to p' and o , then p has formed *performance expectations* for p' and o , i.e. assumptions about the quality of future performance outputs. Performance expectations have a one-to-one relation to beliefs about task ability. Those high in ability will be expected to perform well, those low, to perform poorly. Previous work shows that it is reasonable to explain the clustered inequalities that emerge in decision-making groups by underlying differences in performance-expectations (see Berger and Snell, 1961; Berger and Conner, 1969). Explaining such inequalities can be viewed as explaining how performance-expectations are formed, how they differ, how they change, or how they are maintained. For groups initially equal in status, how they form and are maintained can be explained in part by certain features of p 's social situation, for example, that a decision is required,

that disagreements emerge in the group, that resolving these disagreements tends to create some broader and more enduring expectations about who, henceforth, is likely to be right or wrong about such matters. In the case of groups initially different in status, however, a basis already exists for forming such expectations: the status characteristic, D . P has already made some assumptions based on D about what p' and o are like; from these he draws further inferences about what p' and o are like in this particular situation, even if the situation is new and D incorporates no prior assumptions about it.

What states of C^* does p attribute to p' and o ? Assumption 2 claims that elements consistent with D become relevant, i.e. p expects p' and o to possess given states of C^* . The most reasonable assumption to make is that the state of C^* that p attributes to p' and o is the state that is consistent with the state he expects them to possess. The components of the activated D (D_x itself, GES_x , and the states in γ_x) are either positively or negatively evaluated. The states of C^* instrumental to T are also evaluated, because the outcomes to which they are instrumental are themselves positively or negatively evaluated. We assume p will make the relations among these evaluations as consistent as he can:

Assumption 3. (Assignment) If any components of an activated D are relevant to C^* , p will assign states of C^* to self and other in a consistent manner.

With states of C^* assigned to p' and o , the behavior of p is determined. The kind of behavior that concerns us we called in section 2 the *observable power and prestige order*. It will be recalled that this order is composed of four kinds of behavior: (1) action opportunities, (2) performance outputs, (3) reward actions, (4) influence. (Greater detail on the interaction process involving such behavior is given in Berger and Conner, 1969.) We say that position A is higher than position B in the observable power and prestige order if an actor occupying position A is more likely to receive action opportunities, more likely to initiate performance outputs (with or without being given action opportunities), more likely to have his per-

formance outputs positively evaluated, and less likely to be influenced when disagreeing with another. Further, the greater the distance between A and B, the greater the difference in likelihood of initiating and receiving these behaviors.

We now relate self and other performance expectation states to p's power and prestige position in the group by assuming that the higher an individual's expectations for self relative to other (the greater p's expectation advantage), the higher his power and prestige position.

Assumption 4. (Basic Expectation Assumption) If p assigns states of C* to himself and o consistent with the states of an activated D, then p's position relative to o in the observable power and prestige order will be a direct function of p's expectation advantage over o.

We can dichotomize the performance-expectation state of p relative to o as follows: High-high, high-low, low-high, or low-low. P's expectation advantage relative to o for these four possible states is positive for high-low, zero for high-high and low-low, and negative for low-high. Thus, if p holds a low-high expectation state while o holds a high-low expectation state in S*, assumption 4 claims that they interact as follows: P is more likely to give o action opportunities than o will give p; p is less likely to initiate performance outputs than o (with or without being given an action opportunity); p is more likely to communicate positive reward actions to o than o is to p; and in disagreements, p is more likely to yield to o's influence than o is to p. (For more detailed arguments for this assumption, see Berger and Conner, 1969.)

From the concepts and assumptions of the theory, we obtain the following result:

Basic Result. (Order-equivalence of Status Definitions) Given S*, and a D culturally neither associated with nor dissociated from C*, p's position relative to o in the observed power and prestige order will be a direct function of their relative states of D, provided any of the following is true in S*:

- (2) if D is the only social basis of discrimination and has been activated;
- (3) if D is a social basis of discrimination, has been activated, and any of its components are relevant to C*;
- (4) if D is a social basis of discrimination, has been activated, and has had relevant states of C* assigned to it consistently;

and for a diffuse status characteristic, $\hat{D}$, whose states have been culturally associated consistently with states of C*,

- (5) if $\hat{D}$ is a social basis of discrimination and has been activated.

Thus, the ordering effect of a status characteristic is independent of the amount of status definition originally occurring in S*. That is, the mean probability of an action opportunity, performance output, positive reward action, or exercise of and resistance to influence is greater for the higher as compared to lower status individuals in each of these situations. Line (1) is a situation that is initially only minimally defined; that is, initially states of D and C* and expectations of p's behavior are unrelated. Specifically, group members know only that p' is a D_a and o is a D_b and that this status difference has not been culturally related to their immediate task. Line (4), however, is maximally defined at the outset; that is, task-expectations are initially assigned to p' and o consistent with their states of D. Here, p' is known or believed initially to be a D_a and o to be a D_b; this specific p' is superior to this specific o on a set of characteristics C₁, C₂, . . . C_n (some members of the set of characteristics associated with D); and this specific p' is expected to be and is believed to be superior to this specific o with respect to C*, the characteristic instrumental to their immediate task. Line (5) also describes a situation maximally defined initially, but in this case the maximal definition results from the fact that task-expectation differences have been previously culturally associated with status differences. That is, members of the group know that p' is a D_a; that o is a D_b; that this particular p' is superior to this particular o on a set of characteristics C₁, C₂, . . . C_n (some members of the set of characteristics associated with D); and believe

- (1) if D is the only social basis of discrimination;

that C^* , the characteristic instrumental to their task, is included in this set. The basic result claims the equivalence of these status situations: The distribution of action opportunities, performance outputs, reward actions, and influence is in all ordered in terms of relative states of D . This result does not claim that the magnitude of differences between high and low positions in the power-prestige order is necessarily the same in all cases; increasing amounts of status definition may strengthen the effect. But the order of the differences in the mean probabilities of various actions forming the observed power and prestige order between high and low states of D is preserved. Furthermore, this effect does not depend on whether or not states of C^* and D were previously associated. In line (5) our basic result covers a case in which $\hat{D}$ and C^* had been associated but in lines (1)–(4) no prior association of D and C^* is assumed. Again, our basic result claims these situations are equivalent: The distribution of action opportunities, performance outputs, reward actions, and influence will be ordered in both cases in terms of relative states of D .

The order equivalence result makes it evident that the theory explains the results of previous experiments all of which are covered by it. One might validly object that the theory's conditions are simpler than those of previous experiments, and require much interpretation to match theory to experiment. It is for this reason that we subjected the theory to more direct tests which are described in section 4. But otherwise, the results of these experiments follow from our theory's concepts and assumptions in a fairly straightforward manner.

Furthermore, the theory satisfies the requirements we laid down at the outset: It describes precisely what it means for status to organize interaction. It identifies the aspects of behavior organized by status characteristics; it identifies those properties of a status characteristic which organize the behavior; it describes the mechanisms through which the process takes place; it stipulates conditions sufficient for the status organizing process to occur.

4. *Direct experimental tests of the theory.*

While we argue that our theory enables us to explain much that is known about the status organizing process in task-oriented groups, this argument involves considerable interpretation of what is taking place in the wide variety of situations in which the process has been investigated. These situations are typically complex, involving more features than are necessary according to our theory to activate the process. This makes such experiments easily subject to alternative explanations. Furthermore, they are not systematically informative for the whole range of situations in which, according to our theory, status organizing processes will take place. We must therefore obtain more direct tests of our theory in situations involving no more and no less than the conditions it stipulates.

In this section we describe two such tests. Both take place in a standardized experimental situation whose techniques and procedures are designed to operationalize the theory's conditions and the components of an observable power and prestige order.⁵

This situation has a manipulation phase and a decision-making phase:

In the manipulation phase, subjects are placed in an expectation-state, either by testing their ability directly or by giving them status information. The actual status of the subjects is always the same. They are kept apart during the experiment, for complete control over their status information. If both have the state D_x , each is told that one is D_x but the other has some other state of D . For example, if both are Air Force staff sergeants, each is told that one is a staff sergeant and the other a captain; or that one is a staff sergeant and the other an airman third class. Each subject assumes that his counterpart has the other state of D .

In the decision phase, pairs of subjects repeat n identical decision-making trials, each of which requires a binary choice. The choice

⁵ In this section we describe only experiments in which status information is manipulated. The experimental situation, however, was developed originally by Berger to study both the emergence and the effects of performance-expectations. See Berger and Conner, 1969, or Berger, Conner and McKeown, 1969.

is made in three stages, the first of which is the subject's initial choice between alternatives made independently, without knowing his partner's choice. Subjects then communicate these choices to their partner, after which each makes a final choice.

Subjects are instructed to make what they feel is the correct preliminary choice and, after taking the other subject's choice into consideration, to make what they feel is the correct final choice. They are told repeatedly that whether initial choices coincide with final choices is unimportant, that using others' advice is both legitimate and necessary, and that the correct final choice is of prime importance.⁶

The task consists of a sequence of almost identical large rectangles made up of smaller black and white rectangles. The subject must decide whether each rectangle contains more white or black area.⁷ The task is ambiguous: The probability of a white-response for each stimulus is close to .50, and the decision on any given trial is independent of decisions on preceding trials. However, the subject is told that there is always a correct response, and success is defined by a set of standards giving scores (number of correct responses) typical of subjects like himself. The experimenter describes the task ability as "contrast sensitivity" or "spatial" judgment ability, and tells the subject that it is not related to artistic or mathematical ability. In other words, he attempts to preclude the subject's having prior expectations about the ability.

The experimenter totally manipulates communication between subjects. He does so by an Interaction Control Machine (ICOM) consisting of subject-consoles, a host experimenter panel, and a master control unit. Each subject, partitioned off from other subjects, sits before a subject-console having various buttons and lights. When he

makes a decision he pushes the buttons; he receives information from the lights. All circuits pass through the master control unit, so the experimenter can manipulate communication from subject to subject.

The structure of the experimental trial, together with the control exercised through ICOM, makes the action opportunities and performance outputs of all subjects equal; ICOM controls all reward actions, that is, all communicated evaluations, and therefore all subject agreements or disagreements. A precise measure of the subject's power and prestige position is obtained by studying the probability that one subject influences or is influenced by another. If the subject changes his final choice, he is said to make an *O-response*; if not, he is said to make an *S- or stay*, response. The probability of an *S-response* measures the exercise of influence in the situation.

A post-session interview follows the manipulation and decision-making phases. This interview is used to eliminate subjects who are definitely suspicious, and claim to act on their suspicions; subjects who differentiate themselves from their partner on some basis other than the status characteristics—who, for example, manage to see the other subject when the other is from a visible minority group; and subjects who are unable to understand the instructions or to hear or understand the status manipulations of the experiment.

The first experiment testing the status characteristics theory was carried out by Moore (1968), using the standardized experimental setting described above. Moore found that subjects who believed themselves better educated than their partner had a higher probability of an *S-response*; i.e., were less readily influenced, than those who believed themselves less well educated. Furthermore, and perhaps more important, Moore found no significant difference between subjects who already associated the task ability with the status characteristic and those who did not.

For this experiment, Moore used eighty-five junior college students. To create a high-low condition Moore informed forty-five of them that one of the two subjects in the experiment was from the junior college

⁶ Various methods have been used to operationalize collective orientation. For example, sometimes the instructions emphasize the legitimacy of taking advice, sometimes subjects are scored as a team, and sometimes the two have been combined. For team scoring procedures, see Berger and Conner (1969).

⁷ See Moore, 1965. Other tasks have been used (see, for example, Berger and Conner, 1969) as well as variants of this one (see, for example, Berger and Fisek, 1970).

from which both in fact came, while the other was from a nearby high school. Both subjects believed themselves to have higher status and their partner to have lower status. To create a low-high condition, Moore informed the remaining forty that one of the two subjects was from the junior college from which both came, but the other from a nearby four-year private university, Stanford. In addition, twenty-two of the forty-five high-low subjects were informed that those from the junior college consistently did better at the task they were to perform than subjects from the nearby high school. Similarly, twenty of the forty low-high subjects were informed that those from Stanford consistently did better at such tasks. These two instructions experimentally associated the status and performance characteristics. Moore found that if a performance-characteristic is previously associated with a status characteristic, and the latter discriminates between p' and o , the probability that S yields to his partner's influence is an inverse function of this status. If the performance-characteristic is not previously associated with a status characteristic, but not dissociated from it, the probability that a subject yields to his partner's influence is again an inverse function of his status, and to about the same degree. Thus, this experiment confirms two of our theory's predictions.

The burden of proof assumption is perhaps the strongest of the theory's assumptions and it would be desirable if possible to isolate it and test it directly. One can do so if (1) activation can be induced experimentally and (2) independent evidence confirms the assignment and basic expectation assumptions. For if we failed to find differences in the probability of an S -response between high-low and low-high subjects, and had assured ourselves that activation had taken place, then we could not attribute our failure to the activation assumption; nor, if we had independent evidence favoring the assignment and basic expectation assumptions, could we attribute our failure to the theory's remaining assumptions. Clearly the burden of proof assumption would require reformulating.

We can extend the same reasoning to each assumption of the theory in turn: For exam-

ple, if we experimentally induce both activation and relevance, any failure to find differences in the probability of an S -response between high-low and low-high subjects could be attributed only to a failure of either the assignment or basic expectation assumption.

This sort of reasoning led to the following experiment: One hundred and eighty Air Force staff sergeants were informed either that one of the two subjects in the experiment was a staff sergeant and the other an airman third class; or else one was a staff sergeant and the other a captain. (In all cases the partner was said to be from a unit sufficiently distant from the subject's own unit to eliminate direct command relations as a factor in the experiment.) Subjects were about average on army general classification scores; but fifty-eight were told that their partner's score was higher or lower, whichever was consistent with their relative rank. This treatment was meant to induce activation experimentally. Another fifty-seven were told not only their partner's score, relative to their own, but also that individuals with higher army general classification scores performed better in contrast sensitivity tests. This treatment was meant to induce relevance experimentally. The remaining sixty-five subjects were told nothing except their partner's putative rank. We made no effort to directly induce differences in performance-expectations. Previous experiments made us confident of the basic expectation assumption (see particularly Berger and Conner, 1969).

If all four assumptions of the theory held, then the probability of an S -response should be greater for subjects who believe their partner is an airman third class than for those who believe he is a captain, whatever additional information the subject is given about himself and his partner. Table 1 shows that high-low subjects consistently have higher probabilities of an S -response than low-high subjects. Nor does additional information materially increase the treatment's effect for low-high subjects—i.e., subjects who believed their partner was a captain. More complete status definition has some effect on high-low subjects—i.e., those who believed their partner was an airman third class. Possibly this reflects the subjects' greater doubt, expressed in post-session

Table 1. Proportion, Mean Numbers of S-responses, and Standard Deviation

Condition*	Number of Subjects	S-responses		
		Proportion	Mean	Standard Deviation
Assignment: High-Low	28	.88	33.5	4.7
Assignment: Low-High	29	.74	28.2	5.0
Burden of Proof: High-Low	28	.82	31.1	3.3
Burden of Proof: Low-High	30	.74	27.7	5.9
Activation: High-Low	31	.81	30.7	3.6
Activation: Low-High	34	.75	28.7	6.8

*Conditions are labeled according to the assumption directly tested by the condition and putative air force ranks of p and o -- sergeant-airman (high-low) and sergeant-captain (low-high).

interviews, that sergeants differed much from younger enlisted men in ability, the additional information, presumably, removing such doubts.⁸ The basic results, nevertheless, independently confirm each assumption of the theory.

5. Refining and extending the theory

Our further research has had two objectives: First, to refine the theory by increasing its precision; second, to extend it by increasing its generality. Both go beyond what is conventionally meant by testing the theory. To test a theory is to confirm or disconfirm some hypothesis that in a strict sense derives from it. The experiments described in section 4 are in this sense properly called tests of the theory. But for the most part, the research that now concerns us does not test hypotheses that derive rigorously from the theory. Rather, the theory provides the basis for this research in the sense that it poses the research's problems and provides the concepts and theoretical arguments used to treat them. Thus, the theory guides and organizes this research and its results show as modifications in the way the theory is formulated.

We will describe six further experiments which fall into three basic groups: the first group equates, as well as differentiates, the statuses of individuals; the second makes specific (as opposed to diffuse) status characteristics that are initially irrelevant, relevant to a group's task; the third provides

subjects with information about multiple statuses, some of which is inconsistent.

(1) *Equating p' and o.* The theory as originally formulated was meant to explain the consequences of status differences on interaction. In formulating its scope, we ruled out groups all of whose individuals were alike, as for example, Bales' groups. No assumptions were made about whether individuals would form expectations for equal ability if they felt equal in status to others in the group; but we tacitly supposed that, in any case, a status characteristic that did not discriminate between p' and o (such as sex in an all-male group) would not dampen the effect of one that did (such as educational differences in an all-male group). Furthermore, in formulating the burden of proof process, we made the effect of the process depend on the absence of any information discriminating between p' and o other than D itself—for other information might be inconsistent with D or might already have defined the situation independently of D, both possibilities being beyond the scope of the theory. But in formulating this condition, we again assumed that information equating p' and o would not inhibit D's effect on the group's observed power and prestige order.

These tacit assumptions about the effect of equal status are called into question by the results of an experiment by Seashore. Seashore intended originally to study incongruent status situations. To this end, she had white female junior college students work on the contrast sensitivity task with what were said to be black female Stanford students.

⁸ For additional results and further analysis, see Cohen *et al.*, forthcoming.

To isolate the effect of incongruent statuses, she used three control conditions: (1) Subjects were informed that O was a white female Stanford student of the subject's age; (2) O was a white female junior college student of the subject's age; or (3) O was a black female junior college student of the subject's age. The relevant status information was communicated by allowing the subject to see, in filling out a form, O's form showing O's name, age, sex, race, and school. Subjects were equated on all status characteristics not intended to produce differences in the subject's behavior in the situation; for assumption 2 could be interpreted as claiming that the effect of the status characteristic would be inhibited only if other status information discriminated between p' and o .

Seashore found no differences between treatments (Seashore, 1968). While the Seashore experiment made a number of departures in procedure, any of which might have accounted for the difference between hers and previous results like those described in Section 4, Cohen, Kiker, and Kruse (1969) reasoned that Seashore had activated statuses that equated subjects, and hence reduced the differentiating status characteristic's effect on the probability of an S-response. They therefore replicated Seashore's experiment, using as conditions: (1) White female junior college students who were informed that O was black, but given no other information about O; (2) or who were informed that O was black and of the subject's same age; (3) or who were informed that O was a Stanford student, but given no other information about O; or (4) who were informed that O was a Stanford student of the subject's age. This experiment confirmed the hypothesis that equating subjects reduces a differentiating characteristic's effect on the probability of an S-response.

Hence, for the burden of proof process to be maximally effective, not only must no other characteristic be inconsistent with or prior to D that also discriminates between p' and o , but no other characteristic must equate p' and o . Observe that this does not imply that the activation assumption is similarly restricted in scope. For Seashore experimentally induced activation of equal status information, and it does not follow

that if subjects are left to choose for themselves from multiple status information that equal statuses will have the same effect. This remains to be seen.

To extend the scope of the status characteristic formulation, Berger and Fisek (1969) constructed a generalization of this theory. It sought to describe the working of specific as well as diffuse, and multiple as well as single status characteristics in task situations. The experiments described in (2) and (3) below evolved from an attempt to refine and answer the questions posed by this generalization.

(2) *Effect of other kinds of status elements.* A specific status characteristic is one whose states are differentially evaluated and from which one might infer other attributes of p' and o . It differs from a diffuse status characteristic in not implying any general expectation state. The theory as originally formulated deals only with diffuse status characteristics, tacitly treating the general expectation state as one of the principal mechanisms making diffuse status characteristics relevant in situations previously undefined by p . While no evidence disproves this, there is evidence to show that, under certain circumstances, specific as well as diffuse status characteristics can be made relevant in situations in which they have not been previously relevant.

Berger *et al.* (1970) have shown that one can make a specific status characteristic relevant to a task, even if it has no prior association with it, if it is made the basis for allocating rewards to p' and o . This happens even when allocating rewards on the basis of the nonrelevant characteristic is made to look arbitrary. The investigation required a two-experiment design: In the first experiment, subjects were tested for their *meaning insight* ability, an intuitive ability enabling some individuals to know which of two non-English words has the same meaning as a comparison English word. (The experimenter of course artificially creates the ability; for details, see Berger and Conner, 1969.) In the second experiment, the same subjects were told that subjects with high contrast sensitivity ability were customarily given 25¢ per trial and subjects with low contrast sensitivity 10¢, for their respective contributions. Pressed for time, the experimenters

could not test subjects for contrast sensitivity; but wishing to keep the study's conditions comparable to their other studies', they would pay the subjects according to the meaning insight scores the subjects had received from the previous experiment. Thus, there is a nonrelevant characteristic, C_1 ; an instrumental characteristic, C_2 ; and a reward supposedly associated with the instrumental ability. When instead the reward is given on the basis of the nonrelevant characteristic, that characteristic determines the probability of an S-response.

Freese (1969) used a similar two-experiment design to show that given three specific characteristics, C_1 , C_2 , and C_3 , if subjects believe that C_1 and C_2 are positively correlated, they make inferences about the task characteristic C_3 from C_1 . But if they believe C_1 and C_2 are inversely correlated they do not. Freese first tested subjects on their meaning insight ability, then informed them either that one with high meaning insight ability had high *social prediction* ability or that one with high meaning insight ability had low social prediction ability. (Social prediction ability is the ability to predict behavior in complex social situations.) Subjects were then asked to participate in a second experiment using the contrast sensitivity task. Meaning insight ability predicted the probability of an S-response for subjects in the contrast sensitivity situation if it was positively correlated with social prediction ability, but not if negatively correlated.

These two experiments show that, in some circumstances, specific characteristics are capable of defining previously undefined social situations; and they to some degree clarify what about the diffuse status characteristic makes it so "expansive."

(3) *Multiple-characteristic status situations.* When two or more status characteristics are activated they may be inconsistent; most theories of status characteristics have been written about such situations (see, for example, Hughes, 1945 or Lenski, 1956). Such situations are typically assumed to be tense and awkward, but how they are resolved is not so clear. Does the individual define the situation with respect to one characteristic, neglecting, suppressing, or denying the other's significance? A balance theory in which inconsistency is reduced,

might suggest this. Or does the individual "average" all available information, forming expectations for self and other somewhere between those formed if all characteristics are high or low?

Two experiments by Berger and Fisek (1970) and Berger, Fisek, and Crosbie (1970) show that subjects combine rather than balance status information. These experiments artificially construct two equally-weighted specific status characteristics, each of which is made equally relevant to the contrast sensitivity task. When the characteristics are made inconsistent, subjects have a probability of an S-response lower than subjects who are consistently high, but higher than subjects who are consistently low (Berger and Fisek, 1970). Furthermore, a subject high on one characteristic and low on another has a lower probability of an S-response when his partner is high on both than when his partner is low on one but high on the other (and similarly for the obverse) (Berger, Fisek and Crosbie, 1970). This further supports the finding that a combined hierarchy is formed from inconsistent status information.

These two experiments provide information important to extending the generality of the original theory; for, if the result survives further tests, the theory can be extended to situations defined by any number of characteristics.

6. Summary and conclusion

The research described in this paper can be summarized as follows:

(1) First, we constructed an abstract empirical generalization from an analysis of investigations of the distribution of participation, prestige, and influence in decision-making groups that are initially different in status categories: When task-oriented groups are differentiated by some external status characteristic, differences between individuals in status determine the observable power and prestige order of the group, whether the status characteristic is previously associated with the task or not.

(2) Second, we formulated a theory that explains this generalization. Different evaluations, different specific expectations, and different general expectations are associated

with the states of status characteristics. These three properties are called into play when two or more individuals are committed to some outcome, must take each other into account in achieving this outcome, and have no other or prior basis for inferring who can better achieve this outcome. They are called into play even if no prior association exists between status characteristics and instrumental-task characteristics, so long as nothing prevents connecting the two. Becoming in this way relevant to the immediate task situation, expectations for performance in the particular situation are formed consistent with the status characteristic. Once formed, such performance-expectations are known to determine the distribution of opportunities to perform, the rate of performance outputs, the likelihood that a performance output is positively rewarded, and the exercise of influence.

(3) Third, we tested directly the assumptions made by this explanation. The argument's structure enabled us to test each independently. These tests confirmed the basic assumptions of the status characteristics formulation.

(4) Fourth, we refined and extended the theory. Further experiments show that: (a) Under certain circumstances, other status elements can be used to organize the distribution of power and prestige in the group. (b) Given information about two relevant characteristics, subjects combine it even if it is inconsistent; creating a power and prestige hierarchy that places inconsistent individuals between those consistently high and those consistently low. (c) Information that equates the status of subjects is combined with other information in the same way; so that in certain circumstances, if subjects are equal in status, the effect of a status characteristic that discriminates between them is reduced. Our current work involves reformulating the theory to account for these results.

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PEOPLE PROCESSING ORGANIZATIONS: AN EXCHANGE APPROACH¹

YEHESKEL HASENFELD

The University of Michigan and The Hebrew University of Jerusalem

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This paper sets forth a framework for analyzing the role of organization-environment relations in defining the classification-disposition functions of people-processing organizations. People processing organizations are defined as attempting to achieve changes in their clients not by altering basic personal attributes, but by conferring on them a public status and relocating them in a new set of social circumstances. The paper's basic proposition is that people-processing organizations employ classification-disposition systems that reflect, in part, organizational adaptations to the constraints of their exchange relations with various market units receiving the processed clients. Using a power-dependence paradigm, variables which increase or decrease the organization's dependence on its market units are identified. The consequences of these exchange relations on the organization's classification-disposition system are then explored.

THE study of formal organizations whose function is to change the lives of people occupies a prominent position in organizational literature. At the theoretical level, studies have concentrated on conceptualizing such organizations (Goffman, 1961; Vinter, 1963; Lefton and Rosengren, 1966; Wheeler, 1966). At the empirical level, studies on organizations such as schools (Cicourel and Kitsuse, 1963), hospitals (Roth, 1963; Strauss *et al.*, 1964), and correctional institutions (Street, Vinter and Perrow, 1966) have increasingly focused on the effects of organizational characteristics on the fate of the people they serve.

Scant theoretical attention has been paid, however, to the emerging importance of those organizations whose explicit function is not to change the behavior of people directly but to process them and confer public statuses on them. Examples of such organizations are a diagnostic clinic, a university admissions office, an employment placement office, a credit bureau, or a juvenile court. We shall denote these as "people-processing" in contrast to "people-changing" organizations. These organizations shape a person's life by controlling his access to a wide range of social settings through the public status they confer; and they may define and confirm the individual's

social position when his current status is questioned.

Moreover, processing functions (i.e., classifying, conferring public statuses, and disposing of clients), are an integral part of the technology of people-changing organizations (e.g., Freidson, 1965; Scheff, 1966: 169-187; Scott, 1969). For example, the rationale for the treatment of the psychiatric or physically disabled patient is based on the "ill" status conferred on him. As people-changing organizations increase in size and complexity, these processing functions are likely to be delegated to distinct sub-units or organizations. Yet, the organization's requirements in defining the structure and content of their people-processing functions has not been systematically formulated and explored.

This paper analyzes people-processing organizations by focusing on the exchange relations they develop with their environment, and by tracing the effects of such relations on the structure and content of their people-processing functions. Environment here means the organizational network within which the organization seeks to attain its goals. (Dill, 1958; Evan, 1966). These organization-environment relations are emphasized for two reasons. First, the core technology of such organizations consists of a set of boundary roles which define the input of clients to the organization and mediate their placement in various external units. Second, the effects of these relations

¹ I am particularly indebted to A. J. Reiss, Jr., R. C. Sarri and R. D. Vinter for their encouragement and their comments and suggestions on earlier versions of this paper.

on the development of the organization's technology and its clients' careers is a relatively neglected area of study.²

This study identifies systematically key parameters which influence the exchange relations people-processing organizations develop with their environment. In turn, it derives a series of propositions about the effects of such exchange relations on the processing of clients which it illustrates with findings from studies on such organizations.

THE CHARACTERISTICS OF PEOPLE-PROCESSING ORGANIZATIONS

People-processing organizations can be defined by the nature of their "product" and of their processing technology. Their major "product" is people with changed statuses and locations in various community systems. Vinter and Sarri (1966) analyze the processing technology into four major tasks:

- (1) The client's attributes and situation must be assessed to determine whether he is legitimately subject to official action;
- (2) The client's attributes and situation must be explored to determine appropriate action alternatives;
- (3) Choices must be made from among these alternatives; and
- (4) Once an alternative has been chosen, the person's relocation in a new social set must be managed.

Thus, the core technology of people-processing organizations is the classification and disposition of clients.

Again, this organization does not try directly to alter the behavior of people who enter its jurisdiction, as is the case with people-changing organizations. Rather, it produces changes by identifying and defining the person's attributes, social situation, and public identity, which in turn typically results in both societal and self-reactions. It is through these anticipated reactions of significant others that the organization tries to change its clients' social position and future behavior.

Thus, for example, when a diagnostic center "diagnoses" the client's problems, it is

presumed that the therapists who receive the diagnosis will determine the nature of the change efforts to be used. Or when an employment placement agency determines that a client qualifies for a certain job or training program, the client's very definition gains him access to the job or program, and thus alters his situation, prospects, and skills.

Furthermore, unlike people-changing organizations, people-processing organizations are organized so that the anticipated reactions to the statuses they confer occur mainly outside their boundaries. That is, those expected to respond to the classified person by working with him or trying to change him are located in an organization external to the definers'. Similarly, the technological activities of such organizations occur primarily at their boundaries. That is, staff activity consists of transactions with clients and their dossiers at input, negotiations with them on a classification-disposition status at output, and exchanges with potential external recipients of the clients. In contrast, the technology of people-changing organizations is more likely to be insulated from the environment and the organization's boundary transactions, particularly as the intensity of the change efforts increases (Wheeler, 1966: 79-80). Finally, since staff-client encounters in people-processing organizations are not organized to change the client's behavior directly, their relative duration tends to be shorter than in people-changing organizations. Table 1 summarizes some distinct differences between people-processing and people-changing organizations.

Given these distinguishing characteristics, people-processing organizations must develop direct and systematic links with external recipient units or markets. We shall denote such formally organized external units "market units." They are thus an integral part of the larger organizational network to which the processing organization is linked. In fact, the network order is based partly on the predictability of the responses of the network's market units to the output of the processing organization.

Furthermore, given the unique product of such organizations, their claims of service effectiveness depend, in part, on the response of the receiving units to the clients' statuses.

² Some of the studies which address themselves explicitly to this issue are Reiss and Bordua (1967); Ohlin (1960); Carlson (1964); Levine and White (1961).

Table 1. Differences Between People-Processing & People-Changing Organizations

Variable	People-Processing	People-Changing
Type of product	Altered status	Behavioral change
Technology	Classification-disposition	Socialization-resocialization
Locus of technology	Organizational boundary	Intra-organizational
Relative duration of staff-client encounter	Short term	Long term

An employment placement agency's effectiveness is measured largely by its ability to classify and refer applicants to potential employers whose responses will agree with the conferred status (Blau, 1963:37-44). In short, the market's reactions to the products of people-processing organizations are crucial to their survival and effectiveness.

Exchange Relations and Classification-Disposition Systems

It is proposed that the classification-disposition systems of processing organizations will reflect the constraints of their exchange relations with market units. We assume that under the norms of rationality (Thompson, 1967), such organizations will try to develop systems sensitive to the needs of their market units in order to optimize the units' receptivity to their output.

We can analyze the way processing organizations respond, adapt and manage their relations with market units with a power-dependence paradigm. As Thompson (1967: 30) defines it:

An organization is dependent on some element of its task environment (1) in proportion to the organization's need for resources or performance which the element can provide and (2) in inverse proportion to the ability of other elements to provide the same resource or performance.

In general, the more a processing organization depends on a given market unit for accepting its product, the more its classification-disposition system will respond to the constraints and contingencies of that market unit.

Given this paradigm, the analysis of the exchange relations between the processing

organization and each of its market units must focus on:

- (a) the conditions which lead to dependence on a market unit, and
- (b) the countervailing forces the organization can mobilize to enhance its independence.

The following conditions tend to increase a processing organization's dependence on a given market unit:

- (a) Enforced dependency on the market unit by externally controlling agents such as legal statutes, parent organization, and other regulatory groups.
- (b) High discretion exercised by the market unit over its own intake.
- (c) Greater need for the client services provided by the market unit.
- (d) Greater prestige of the market unit as a validator of the processing organization's goals and performance.
- (e) Scarcity of knowledge about potential and alternative market units.

The following conditions tend to increase a processing organization's independence of a given market unit:

- (a) Freedom permitted the organization by its externally controlling agents to develop exchange relations.
- (b) Possession of strategic resources needed by potential market units.
- (c) Availability of alternative market units offering similar client services.
- (d) Development of a self-validating organizational ideology.
- (e) An effective intelligence system about alternative market units.

The conditions that increase or decrease the organization's dependency on its market units are summarized in Table 2 and will be discussed in some detail below. Clearly, in the organization's exchanges with its

market units all the above conditions may operate simultaneously. For discussion and illustration, however, they will be treated separately.

DEPENDENCY VS. FREEDOM IMPOSED BY EXTERNAL POLICIES

In analyzing processing organizations' market relations, one must fully recognize the constraints, requirements, and privileges of policy-regulating external units. The funding agency, the parent organization, the legislative unit and the like, may exert direct and indirect influence on two levels: (a) relations with market units; (b) policies vis-a-vis clients.

Policy regulations may restrict the type or range of market units that the organization can use, and may further stipulate the conditions of such transactions. To comply with these policies the organization will have to incorporate such stipulations in the classification-disposition process. For example, government sponsored manpower agencies are often restricted in the type of employers or training programs they can use for their clients (Levitan and Mangum, 1969). These constraints, in turn, limit the type and range of occupational categories staff can use to classify and refer their clients.

Moreover, external policies may even specify the attributes that potential clients must have to qualify for certain market units. Such policies curtail a staff's discretion and may rigidly restrict the number of classification-disposition categories they can confer on clients. Such is the case for public welfare departments which are con-

trolled by extensive restrictive policies of this kind (Keith-Lucas, 1957).

External policy units may also require specific information about the clients' attributes and situations, and the need to collect such information thus invariably has consequences for the classification-disposition system. Official classifications, at least, will tend to be constrained to reflect these information requirements.

On the other hand, external policies may give much latitude to the processing organization's relations with external units, enhancing its independence. With such freedom it can select the market units most compatible ideologically and can control the type of clients it serves. Scott (1969) in his study of blindness agencies notes, for example, that the near absence of external policy regulations enabled these agencies to select both programs and clients most compatible with their treatment ideologies.

INTAKE DISCRETION VS. POSSESSING STRATEGIC RESOURCES

Market units can be distinguished by the amount of intake discretion they use in accepting or refusing a processing organization's products. The greater the discretion the greater the processing organization's dependency on these units, a situation reflected, for example, in the relationship between the employment placement office and potential employers. Consequently, the organization's classification-disposition system is more likely to adhere to these units' intake criteria. Nevertheless, when the organization possesses resources essential to these units, it can potentially offset the

Table 2. Conditions Which Increase Dependence v. Countervailing Powers

Condition of Dependence	Countervailing Power
Dependence imposed by external policies	Freedom permitted by external policies
Intake discretion of market unit	Possession of strategic resources
Need for client services of market unit	Availability of alternative client services
Market unit as validator of organizational goals	Development of self-validating organizational ideologies
Intelligence deficiencies about alternative market units	Intelligence effectiveness about alternative market units

effects of such dependency. When it is a major supplier of clients, controls economic resources, or largely validates a market units' claims for competence, it is in a better bargaining position with them. For example, while the private physician depends on the psychiatric hospital to accept his referred patients, he may also validate the hospital's claim for competence, and thus be only partly constrained by its intake discretion. Mishler and Waxler (1963) found that the psychiatric hospitals they studied were far more likely to accept patients referred by private physicians, though the hospitals added rules to their classification systems on age, previous hospitalization and presence of a relative.

On the other hand, market units having little intake discretion tend to require a minimal set of criteria of their clients for entry (e.g., the public school, the mission shelter, the city's general hospital, etc.). When a significant proportion of the market is composed of units with little intake discretion, one would expect the classification-disposition system used by the processing organization also to have a few relatively global typification rules which are insensitive to the client's attributes and situation. Since the relative lack of intake discretion decreases its dependency on the market unit, the processing organization is less likely to expend resources and energies to develop an elaborate classification system vis-a-vis the unit. In Scheff's (1966:128-155) study of psychiatric screening of patients to a state mental hospital, the screening psychiatrists used very few and gross classification-disposition procedures partly because the hospital used minimal intake discretion. Sudnow (1965) describes a similar pattern in the procedures of the public defender's office.

Under these conditions, however, the market units may try to develop various tacit agreements with the processing organization to control their intake informally. When these occur, the formal classification-disposition system takes on classification criteria which may make it irrelevant in the actual routing of classified persons. Thus, for example, correctional institutions receiving young offenders from a state diagnostic center may tacitly agree with the center

to consider additional classification-disposition criteria such as space availability, age, and race, which neutralize such other criteria as offender's psychological and social adjustment problems (Dickson, 1967).

NEED FOR MARKET UNIT SERVICES VS. AVAILABLE ALTERNATIVES

The need for a given market unit largely depends on the changes in social position or future behavior the processing organization seeks for its potential clients, and the nature of the demands of potential clients. The more functionally specific its processing tasks the more dependent the organization on other units' specialized services. Yet, the availability of other market units providing comparable services increases a staff's discretion. The organization is in a stronger bargaining position with these units and hence may develop its classification-disposition system somewhat independently of them.

Hence when the organization needs the services of a given market unit having few counterparts, we would expect its system to correspond to constraints imposed by the market unit. On the other hand, having alternative services available potentially frees the organization from such restrictions.

Lacking such units, the organization is under pressure to control either its intake, or its output of clients, or both. Hence, its system may accept only clients for whom disposition resources are available. Similarly, if the organization cannot control its client intake, it is likely to confer a temporary or "holding" status on clients for whom few alternative market units are available. Such a status links the person to the organization, yet defines him as unready for disposition. A study of employment placement agencies indicated that clients for whom employment opportunities were not available were more likely to be classified as requiring counseling than clients similar to them for whom employment opportunities were available (Hasenfeld, 1970).

As we shall note later, the effectiveness of an organization's intelligence system also crucially influences the effective range of available alternatives.

MARKET UNITS AS VALIDATORS OF
ORGANIZATIONAL GOALS VS. SELF
VALIDATING ORGANIZATIONAL
IDEOLOGIES

Potentially, the organization's products are critical to validating its goals, and organizations go to great lengths to use a variety of product measures as indices of their effectiveness and efficiency. For people-processing organizations, the product is placement of a client in one of several market units. Market units differ in their importance in validating an organization's claims of success. This difference is based on: (a) the type of effectiveness and efficiency measures the organization must use with its regulatory groups, and (b) the prestige, importance and amount of crucial resources the market unit controls in the organizational network.

Hence, the greater the organization's dependence on a market unit for validating its goals, the more will it try to devise a classification-disposition system that will assure placing its clients positively in such a unit. For example, state rehabilitation agencies tend to be evaluated by the number of successful job placements; so they are likely to limit the entry of seriously debilitated clients into their programs (Levine and White, 1961:590-592). Similarly, Reiss and Bordua (1967:32-40) suggest that since police departments depend on prosecutors and courts to validate their claim of competence, the criteria they use for disposition of suspects are partly defined through negotiations with these offices.

The proposition, however, is based on the assumption that an organization's critical product for validating its goals is a "placed" client. An organization's usefulness may rest, however, on other products or side payments (Cyert and March, 1963:29) just as critical to its survival as placing clients, if not more so. It may be a source of employment for members of important political groups, or a training ground for jobs considered important by external validating groups. Ohlin's (1960) analysis of the role of interest groups in influencing correctional objectives points up the importance of side payments. These side payments may neutralize the influence of recipient market units. In fact, they may make the processing organization uncon-

cerned with the market unit's response to its clients. Under these circumstances one may find that its classification-disposition system is insensitive to market conditions and that major disjunctures occur between the two.

Similarly, organizations may develop self-validating ideologies as buffers against negative feedback from their market units. Self-validating ideologies are internally generated belief systems about an organization's effectiveness that negate assessment criteria used by external groups, or those based on evaluation of outcomes. Thus, Eaton (1962) found, for example, that treatment-oriented social work agencies were hostile toward evaluative research and preferred symbolic research based, in part, on staff beliefs about treatment outcomes.

Such ideologies minimize the organization's dependence on its market units to validate its goals. A potentially unanticipated consequence of such ideologies is a staff's biased perception of the type and range of market units that could effectively serve its clients. Such perception, in turn, may foreclose potential transactions with a greater variety of market units. Consequently, its classification-disposition system might respond less to market contingencies than to the self-validating belief system.

INTELLIGENCE EFFECTIVENESS VS.
INTELLIGENCE DEFICIENCY

Finally, we propose that organizational intelligence about the environment of potential market units is crucial in determining the range and scope of the exchange relations that people-processing organizations can develop. Such an intelligence system's importance stems from the fundamental fact that environmental stimuli elicit organizational responses only when transformed into bits of information (Dill, 1958). As Wilensky (1967) suggests, the organization's intelligence system profoundly effects the ways an organization interprets its environment and responds to it.

For people-processing organizations the functions of an effective intelligence system would be to:

- (a) determine the various intake criteria used by potential market units;

- (b) search and identify new market possibilities;
- (c) monitor changes in market conditions regarding intake selection; and
- (d) provide feedback to the processing organization about its success or failure in disposing of its clients.

Moreover, an intelligence system's effectiveness will be determined not only by the amount and accuracy of its information but also by the extent it disseminates this information among staff who are processing clients.

The greater the effectiveness of the intelligence system the wider the range of market units likely to be considered and the less the organization will depend on a few market units. Hence, its classification-disposition system will become more elaborate, and have varied and specified typification rules. In contrast, an organization experiencing intelligence deficiencies must rely on the few market units that use only minimal intake discretion, or those whose intake criteria are well known in the community. Hasenfeld (1971) noted that the lack of adequate and valid information about community resources by the staff of community action centers serving the poor led, in part, to using market units already well known to the poor themselves. Under these conditions the classification-disposition system will become simplified, and be characterized by a few global typification rules.

CONCLUSIONS

We have tried to define and develop a perspective on people-processing organizations that emphasizes the effect of the external environment on the classification and disposition of clients. We have suggested that these core activities occur at such organizations' boundaries and are expressed in the form of continuous transactions with clients and other organizations. We suggest, therefore, that an exchange or transaction view of formal organizations provides a potent model for analyzing such organizations and their core activities, particularly in explaining the various patterns of people-processing such organizations develop. This paper identifies some key inter-organiza-

tional features which determine the power-dependence relations between the processing organization and its environment, and traces their significance for such an organization's system for classifying and disposing of clients.

Our study is inevitably limited in its scope, and does not address itself to several key variables that clearly influence clients' careers in processing organizations. Among these are the role of the clients and their resources in negotiating with the organization; the characteristics of the organizational processing technologies; and the social position, training and orientation of personnel in such organizations. Nevertheless, the study systematically derives a series of testable propositions about the effects of the organization's power-dependence relations with its market units on client management patterns in people-processing organizations.

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HETEROGAMY, INTER-CLASS MOBILITY AND SOCIO-POLITICAL ATTITUDES IN ITALY*

LAWRENCE E. HAZELRIGG

Indiana University

JOSEPH LOPREATO

University of Texas, Austin

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The literature on social stratification refers on occasion to inter-class marriage as the "acid test" of class mobility. This paper examines that proposition using Italian national-sample survey data on four socio-political perspectives: the respondent's perception of 1) the different distribution of rewards in society, 2) the degree to which class boundaries restrict social intercourse, 3) the character of inter-class relations, and 4) the interest representations of political parties. Evidence supports the proposition. In addition, rates of heterogamy are reported, and the relationship of heterogamy to career entries is examined.

OCCASIONALLY the literature on social stratification refers to inter-class marriage as the acid test of class mobility. Schumpeter (1951:141) observes that the "most important symptom of [class boundaries to social behavior] is the ease or difficulty with which members of different classes contract legally and socially recognized marriages." Similarly, in his study of the Vendéan counter-revolution Tilly (1964: 88ff) argues that data on marriage patterns illuminate the degree to which class boundaries separate groups "truly distinct" in social honor and social access:

... 1) because marriage itself is ordinarily a powerful sort of alliance between families as well as individuals, 2) because the frequency of marriage suggests which groups were able to treat each other as approximate equals, 3) because even where rank is not involved, the frequency of marriage between two groups generally corresponds to the warmth and frequency of other, less binding, social relations.

Various other scholars (e.g., T. Geiger, 1951; H. K. Geiger, 1968; xviii; Foster, 1968; Bloch, 1970:192) have made comparable observations.

If patterns of marital preference should help identify "those groups which were able to treat each other as approximate equals," then an upward marriage (from the groom's perspective) contracted along with a rise in occupation from the parental class should

constitute good evidence of the newcomer's social acceptance by the old-fashioned members of the destination class.¹ This paper examines such a possibility by analyzing survey data from Italian society. Class heterogamy² has been documented and discussed, though usually briefly, for a wide array of periods, places, and classes: Ciceronian Rome (Cowell, 1956:340); medieval London (Thrupp, 1962:265-269), fifteenth-

¹ There are various expressions of this argument in the "folk wisdom" and other belief structures of a people. A well-known instance from U. S. history is cited by Richards (1970:44-45) in his study of the antiabolition mobs of the Jacksonian era: "Onto the Negro the anti-abolitionists projected their own view of the American way. Marrying 'well,' they assumed, was the only real sign that an 'inferior' had arrived. And since all 'inferior' nationalities and classes measured their standing on the basis of their access to a 'superior's' women, it followed that the most 'inferior' of all would do likewise. Also see Weber's (1946:405ff) brief discussion of heterogamy in relation to caste in India and (1968:II, 932), where he refers to endogamy as one of the "restrictions on social intercourse" characteristic of social honor; and Merton (1941), especially on structural factors.

² In a sense, this term is not altogether felicitous for class analysis. It emphasizes the family as the unit of analysis, which is by and large in keeping with established practice; but it could also be argued that a white-collar man who marries a woman of white-collar origin is her social equal, regardless of his fathers' occupation. The unit of analysis here shifts to the individual; and its validity, it would seem, increases in direct proportion to the "openness" of the stratification structure and to the rate of a society's geographic mobility. It often happens that by the time a successful individual of humble origin gets married, he has been financially independent of and physically and socially distant from his parental home for a number of years.

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century Spain (Mariéjol, 1961:42), eighteenth-century English towns (Foster, 1968), nineteenth-century India (Weber, 1946: 405ff), and mandarin China (Hsu, 1948), medieval English landed aristocracy (Holmes, 1957), English aristocracy of the Tudor and Stuart periods (Stone, 1960-61), eighteenth century English aristocracy (Habbakuk, 1950), and twentieth-century Irish (Arensberg and Kimball, 1940) and Greek (Friedl, 1962) peasants. Sjöberg (1960:145-156), while commenting on the strain toward class homogamy exhibited in preindustrial societies, also notes that heterogamy was certainly known to such societies and often occurred in conjunction with occupational mobility. Miller (1954) has reported in detail on institutionalized hypergamy³ in contemporary India, among the Nayar of Kerala state for whom it was useful communally in partly freeing political recruiting from caste constraints.⁴

³ The reference for this term and its complement, hypogamy, is the female; thus, hypergamy indicates a downward marriage, and hypogamy an upward marriage, for the male.

⁴ There is some evidence that cultural prescriptions of endogamy are associated with the level of material-production technology and economic structure. Data recently assembled by Goody (1969, 1971) show that, whereas endogamous rules have been characteristic of at least many European and Asian cultures, with few exceptions (these resulting, Goody says, from Mediterranean or northern influences) such prescriptions have not been found in sub-Saharan African cultures. In seeking to explain this finding, Goody notes a correlation between the rules of the marriage market (endogamy-heterogamy) and mechanisms of property transmission: in Eurasia endogamy is associated with the dowry; in Africa, out-marriage with bridewealth. The significance of this correlation, Goody argues, lies in the circumstance that in the Eurasian cultures, where endogamy prevails, differences of property and status are sufficiently large (because of an agrarian economy's surplus-product capacity) that mechanisms to preserve those differences are profitable to the privileged groups; in-marriage and the dowry are such mechanisms. By contrast, traditional African economics (horticultural, herding, hunting and gathering) rarely produce a surplus product, and thus property and status differences are negligible. Since in "the absence of the plough and other capital investment in land one man's holding" is much like another's (Goody, 1971:596), there is little or no advantage to property controls on the selection of mates. Indeed, for the group's survival there is more advantage in having mechanisms that level

One of the more extensive studies of heterogamy in relation to class mobility for early industrial society is given by E. Barber (1955:99ff) for eighteenth-century France (cf. also Bloch, 1970 for his discussion of heterogamy and class in earlier periods of French history). She views inter-marriage as "the clearest and final test" of class equality among families. Though the general rule of marriage in eighteenth-century France was homogamy, inter-class unions occurred often enough (especially between the new bourgeoisie and the old nobility) to escape seeming odd to many Frenchmen. To be sure, "... the frequent designation of these marriages as *mésalliances* indicates that they had only limited approval." But, Barber contends, "they are nevertheless proof positive [the acid test] of a considerable amount of social mobility."

Hypergamy, apparently the more prevalent form of intermarriage, was much encouraged by wealthy upstarts' practice of bestowing dowries on their daughters. Barber quotes the Duchesse de Chaulnes' declaration that "to make an advantageous *mésalliance* is 'to fertilize one's soil,' and God knows the soil needs fertilizing." Often, too, however, daughters of the nobility contracted marriages with bourgeois men—sometimes for love, more often for the groom's probable future wealth and standing in society. Investments in education and expected inheritance of *roturier* enterprise and wealth made select bourgeois sons-in-law a serious alternative for financially embarrassed but daughter-rich noble families. For the groom's part, upward marriage put the seal of success on an occupational mobility achieved by intellectual attainments, a military career, or the venality of office⁵ (E. Barber, 1955:105ff, 177ff, 130ff).

Speaking of more recent times, Lipset and Zetterberg (1966:563) have similarly proposed the comparison of husband and wife social origins ("economic class position") as a means of defining important aspects of

differences in property, i.e., out-marriage and bridewealth.

⁵ "The venality of offices contributed heavily, in the France of old, to the upward movement of the inferior classes, and to the renewal of the ruling class" (Pagès, 1932:492, quoted in E. Barber, 1955:107). Cf. also Bloch (1970: Chapter 4, esp. pp. 125-26).

interclass mobility. Specifically, they argue that while a man may change his economic status by changing his occupation, until he achieves intimate relationships (as made possible by heterogamy, for instance) with established members in the destination class, he is apt to suffer segregation via status distinctions of "newcomer" versus "old-timer." More an intruder than recruit, especially to old-timers who see closure as conferring group identity, the newcomer is in but not of the destination class.

Taken with caution, the notion of heterogamy as the final step in inter-class mobility is a useful index of the social-psychological barriers to mobility. Some sociologists (e.g., Dahendorf, 1959:57-61, 218-223) in studying the different barriers to mobility, emphasize such structural constraints as different educational opportunities or other selectivity mechanisms of class recruitment. But there is also the important cultural dimension of "participant acceptance," invariably if often implicitly called upon in discussions of the effects on behavior and attitude of vertical social mobility. Political socialization theory argues, for instance, with some cause (cf. Lipset and Bendix, 1959: 64-72; Hyman, 1959: Chapter 5; Lopreato, 1967a; Lopreato and Chafetz, 1970; J. A. Barber, 1970: Chapter 6; Thompson, 1971), that mobile individuals, especially the upwardly mobile, tend to adopt the political perspectives of their destination class. But such tendencies are doubtless qualified by the degree to which class old-timers accept newcomers. Certainly in terms of the social-psychological identity of a class membership, as distinct from merely being "objectively" in a certain class, the legitimation variable has important resocialization consequences. One ascends socially not merely by making more money and acquiring a superior group of occupational peers but also by being able to feel *dans son monde* among those peers. Newcomers who are accepted socially into the identity of membership should more likely define themselves as members and thereby display *as their own* the perspectives of their new class. Accordingly, to the extent that heterogamy indicates social acceptance—and in general it shows greater acceptance than does the achiever who marries a girl from his own origin class—it should dis-

tinguish different levels of resocialization. With some exaggeration of distinction perhaps, the difference may be on the order of resocialization into "class-as-reference-group" versus "class-as-membership-group" (cf. Dahendorf, 1959:189-193; Bott, 1957; Runciman, 1966:11ff).

This paper will examine such presumed relationships between heterogamy and interclass mobility. Our general question is twofold: first, *to what degree do occupationally mobile men, defined with reference to social inheritance, tend to marry toward their destination class*; and second, *do mobile persons who are correspondingly heterogamous resocialize more often than homogamous mobile persons into the socio-political perspectives of the destination class?*⁶

⁶ There is little evidence that bears systematically on the issue of heterogamy as an index of interclass mobility for industrial society (cf. Lipset and Rogoff, 1954:567; Svalastoga, 1958:302-304, cited in Goode, 1966:594; Coleman and Neugarten, 1971: 247). However, a body of related data on patterns of heterogamy per se has accumulated over the years; and it would be useful to attend briefly to this evidence, since on the way to our primary objectives we shall have occasion to estimate the amount and direction of inter-class marriage in Italy.

Nearly two decades ago, Burgess and Locke (1953:369) could report the existence of more than a hundred studies on variation in assortative mating by socio-economic status levels. The usual conclusions drawn in these and subsequent studies—the majority of which have been confined to the United States—are that (1) choices in marriage seem not to be a major source of upward or downward mobility in any society, though all societies exhibit some degree of upward movement of this type (Goode, 1966:594); and (2) when heterogamy does occur, "men tend to marry down and women tend to marry up" (Burchinal, 1964:654; see also Zelditch, 1964:688; Goode, 1966:594). Excess of hypergamy is typically accounted for by the dominance of the male sex (Merton, 1941; Goode, 1966). Since the male's ranking sets the rank of the immediate family as a whole, he can more afford to marry down. As the saying goes in some European countries, "It is the King who makes the Queen."

Berent (1954), in one of the few non-U.S. studies of heterogamy conducted within the context of an occupational mobility investigation, reported a "slight tendency" for men to marry down, with endogamy greatest among the top occupational levels and lowest in the skilled manual and "routine nonmanual" strata. (Unfortunately, Berent did not provide data on the relationship between heterogamy and occupational mobility.) The conclusion that heterogamy generally takes

DATA AND METHODS

Data for this study are taken from a national survey of social stratification in Italy, conducted in 1963-64 with a total sample of 1,569 male heads of households.⁷ The working sample is here restricted, first to the 1,307 married respondents who gave information on their fathers' and wives' fathers' main occupations, and second, the 1,226 respondents who met the foregoing requirements and gave information on their own first jobs.⁸

Inter-class marriage is defined by comparing the husband's and wife's social origins (fathers' occupations). The category of occupational mobility studied is origin-to-career-entry: we compare the father's main occupation and son's first occupation. Our measure of intergenerational mobility is discrete, unlike the one customarily used which mixes intergenerational and intragenerational movements.⁹ Ideally the origin referent should be specified as "father's occupation at time of son's entry in the labor force." This datum was unavailable but since individuals are seldom mobile in Italy after the first few years of career activity—especially across the nonmanual-manual-agrarian axes, which we use to estimate class position (cf. Lopreato and Hazelrigg, 1972: chapter XIII)—the bias of using "father's main occupation" is probably negligible.

Finally, we come to the important question of the temporal relation of the marriage to the respondent's career history: did it occur before or after career-entry? Only if

the form of downward marriage for the male is not without challenge. Laumann's (1966:74-75) study casts doubt on the presumed excess of hypergamy, and Rubin's (1968:758) recent analysis of Blau and Duncan's (1967) national OCG data revealed "no overall hypergamous or hypogamous tendency." Indeed, this latter study suggests the quite reasonable conclusion that assortative mating is variably patterned on the occupational scale, with heterogamy likelier between some occupational groups than others.

⁷ This refers to DOXA (Milan, Italy) survey No. S6341.

⁸ Note that this reduction of the original, representative sample by 17 per cent in the first instance and 25 per cent in the second makes the use of standard estimations of sampling error rather dubious.

⁹ For a discussion of this methodological issue, see Lopreato and Hazelrigg (1972: Chapter XIII).

it occurred after, could one clearly argue that an upward marriage capped a man's upward occupational mobility; at least the alternative sequence would introduce confounding elements. Since we lack data on age at marriage, we cannot give the exact age relationships between marriage and first job. But the vast majority of respondents were undoubtedly married at or after career entrance. Italian men eagerly seek a *sistemazione* (steady gainful employment) before contemplating marriage. Moreover, permanent entry into the labor force occurs early in Italy, compared to the United States, and age at marriage is considerably higher.¹⁰ A young man without steady employment finds it harder in Italy than in the United States to support a wife.

FINDINGS: HETEROGAMY AND MOBILITY

Heterogamy. Table 1 presents data from the preliminary phase of analysis, namely, rates of heterogamy with respect to the husband's social origin. The table is constructed in the form of a standard outflow matrix. The number in the center of each cell is the absolute frequency; in the upper right corner, the outflow (row) percentage; and in italics in the lower left corner, the standard mobility ratio (observed/expected frequencies).¹¹ Note that because the sample consists only of males married at the time of interview, the matrix does not represent a "closed system" of mate selection—i.e., it ignores the alternative of *not* marrying—

¹⁰ According to Italian vital statistics information for 1960, two-thirds of all bridegrooms were under thirty, as were three-fourths of all brides (Central Institute of Statistics, 1963: Table 13). Comparable proportions for the United States for 1955-1958 were slightly over three-fourths and four-fifths, respectively; for first marriages only, which are more nearly comparable to the Italian case, they were 89 and 94 per cent, respectively (U. S. National Center for Health Statistics, Tables A and 1).

¹¹ The full-equality assumption underlying the ratio is in this case merely a standardized benchmark for comparing the cells. Whether the assumption is realistic for appraising the equity of marital opportunity in Italy is a different matter. Note that because the reported rates do not apply to the total "at risk" population, they fail to discriminate heterogamous tendencies resulting from such market restraints as class differentials in the marriage rate; the rates are contingent on the marriage event itself.

Table 1. Heterogamy Estimates: Comparison of Respondent's and Wife's Social Origin Levels.

Respondent's Social Origin	Wife's Social Origin						Total
	(I)	(II)	(III)	(IV)	(V)	(VI)	
Professional, High Manage- rial & Execu- tive, Entre- preneurial (I)	25.0 9 <u>15.0</u>	41.7 15 <u>2.3</u>	5.6 2 <u>0.7</u>	11.1 4 <u>0.7</u>	16.7 6 <u>0.5</u>	0.0 0 ---	100.1 36
Middle & Low Managerial & Officials, Merchants, Clerks (II)	5.0 13 <u>2.8</u>	48.8 127 <u>2.7</u>	8.5 22 <u>1.1</u>	17.3 45 <u>1.1</u>	17.7 46 <u>0.6</u>	2.7 7 <u>0.2</u>	100.0 260
Skilled Work- ers, Artisans without dependent workers (III)	0.5 1 <u>0.3</u>	21.3 40 <u>1.2</u>	24.5 46 <u>3.1</u>	18.6 35 <u>2.2</u>	28.2 53 <u>0.9</u>	6.9 13 <u>0.6</u>	100.0 188
Semiskilled Workers, Unskilled Workers (IV)	0.5 1 <u>0.3</u>	16.7 36 <u>0.9</u>	11.6 25 <u>1.5</u>	37.2 80 <u>2.4</u>	20.5 44 <u>0.6</u>	13.5 29 <u>1.2</u>	100.0 215
Small Farm Owners, Sharecrop- pers, Ten- ant Farmers(V)	0.4 2 <u>0.3</u>	10.6 48 <u>0.6</u>	4.6 21 <u>0.6</u>	10.8 49 <u>0.7</u>	66.8 302 <u>2.1</u>	6.6 30 <u>0.6</u>	99.8 452
Agricul- tural Laborers (VI)	0.6 1 <u>0.4</u>	6.4 10 <u>0.4</u>	3.2 5 <u>0.4</u>	14.7 23 <u>1.0</u>	18.6 29 <u>0.6</u>	56.4 88 <u>5.1</u>	99.9 156
Total N	27	276	121	236	480	167	1307

Note: Number in the center of each cell is the cell N; number in the upper right hand corner is the outflow percentage (i.e., from respondent's social origin to wife's social origin); underlined number in lower left hand corner is the standard ratio of observed-to-expected frequencies.

and therefore the observed hypergamy and hypogamy rates are not constrained by zero-sum conditions.

Overall, 50 per cent of the unions were homogamous, 27 per cent were hypergamous, and 23 per cent were hypogamous. The rank correlation coefficient (Kendall's Tau_c) was .40. Though such comparisons are crude and tenuous, these data differ somewhat from evidence for England and Wales (Berent, 1954: Table 1) and the United States (Rubin, 1968: Table 1):

	Hyper- gamy	Homo- gamy	Hypo- gamy	Tau.
<i>England and Wales</i>	30%	45%	25%	.26
<i>United States</i>	30%	38%	32%	.29
<i>Italy</i>	27%	50%	23%	.40

Of course, the job classifications used in these studies vary, a fact which could account for some of the above differences; but it is significant that the Italian matrix is a 6 x 6 cross-tabulation, in contrast to Berent's 4 x 4 and Rubin's 5 x 5. This matrix difference should increase the heterogamy rate

in Italy, relative to the other countries. All in all, the above comparisons seem to parallel the evidence that rates of intergenerational occupational mobility are somewhat lower in Italy than in a number of other industrial societies (Lopreato and Hazelrigg, 1972: Chapter XIII).

A few facts stand out in Table 1. First, in terms of expectation, is the extremely high rate of endogamy at the top of the occupational hierarchy. To be sure, the (I, I) cell also shows the second lowest percent of intermarriage (25%) of any on the principal diagonal; but note that these respondents married their next-door neighbors' daughters at a rate at least three times greater than that of any other social-origin category of wives. This tendency to heterogamy by proximity is greater than for any other social origin category. In part, the finding mirrors the "ceiling effect" of being at the top of the classification: statistically, they had only one alternative to endogamy; but much of the high rate represents a real difference in mate selection patterns.

Second, a relative barrier to intermarriage exists between the farm (V and VI combined) and nonfarm (I-IV) categories. Few sons of nonfarm social origin married farmers' daughters, and even fewer farm sons married girls of nonfarm social origin. These tendencies suggest two interesting facts. First, apropos of the post World-War-II exodus of males from the farms, when the petty farmer or farm laborer moved to the city, he generally took along the farmer's daughter (unless he found her in the place of in-migration) or returned for her later, rather than marry a city girl. Only mate selections between agricultural laborers and the lower blue-collar strata exhibited a greater than expected marriage rate across the farm-nonfarm boundary. This pattern is to be expected given the relative similarity of their life styles and the fact that at this level one may be a farm laborer one week and an unskilled urban worker the next. The relative absence of social attraction between farm and nonfarm people results not merely from geographic segregation but also from a cultural distance deeply rooted in Italian history. Traditionally, despite the widespread phenomenon of the Italian urban village in the South especially, agricultural workers

and urban people have experienced the kind of social chasm that Veblen (1899) described for "predators" and "productive" people in general. This chasm still closely mirrors the estate relations of feudal times and is reflected in the widespread illiteracy of agricultural people.

Second, the slight tendency toward hypergamy among farm-origin categories suggests that, in response to population pressure and a history of "land hunger," the first to leave agricultural work in great numbers were the women; the proliferation of ox-carts and animals of burden encouraged this exodus because one of women's major functions in agriculture has been to transport produce. Hence, throughout Italy one finds large numbers of farmers' daughters in the female trades (e.g., tailoring) whose spouses are predominantly from the cities. In a sense, the stratification unit here shifts from the family to the individual: a farmer's child who is not a farmer is quite eligible, within the limits delineated in this paper, to intermarry with a nonfarm person.

Table 1's third outstanding feature is that while the overall pattern shows a slight excess of hypergamy, there were two exceptions in strata comparisons of theoretical observed/expected ratios. The largest occurred toward the top of the hierarchy: more respondents married *up from* than *down to* the white-collar stratum of middle and lower-echelon management and officials, merchants, clerks, and the like (ratio of 2.8 versus 2.3). The other case involved agricultural laborers, for whom the corresponding cumulative ratios were 2.8 and 2.6, respectively. In all other similar comparisons, ratio differences were in the direction of downward marriage for the male respondents.

Heterogamy and Career Entries. Table 2 represents the combined matrices for origin-to-entry occupational mobility and inter-class marriage. Adding a third variable requires using a three-fold occupational scale (which is more conducive to our later analysis of heterogamy as an index of inter-class mobility); otherwise the table is constructed in the same way as Table 1.¹²

Our first question is how does a respon-

¹² Had Table 1 been analyzed as a 3 x 3 matrix, the correlation of husband and wife origins would have been .42 instead of .40.

Table 2. Relationship of Respondent's Career Entry Level to Wife's Social Origin, by Respondent's Social Origin.

Respondent's Social Origin	Respondent's Career Entry	Wife's Social Origin			Total
		Nonmanual	Manual	Agrarian	
Nonmanual	Nonmanual	65 113 <u>1.2</u>	20 34 <u>0.8</u>	15 26 <u>0.7</u>	100 173
		31 25 <u>0.6</u>	42 34 <u>1.6</u>	27 22 <u>1.3</u>	100 81
	Agrarian	20 2 <u>0.4</u>	10 1 <u>0.4</u>	70 7 <u>3.3</u>	100 10
	Total	53 140	26 69	21 55	100 264
Manual	Nonmanual	42 26 <u>2.1</u>	31 19 <u>0.7</u>	27 17 <u>0.8</u>	100 62
		15 43 <u>0.8</u>	50 145 <u>1.1</u>	35 103 <u>1.0</u>	100 291
	Agrarian	19 3 <u>1.0</u>	19 3 <u>0.4</u>	63 10 <u>1.8</u>	101 16
	Total	20 72	45 167	35 130	100 369
Agrarian	Nonmanual	43 21 <u>4.6</u>	22 11 <u>1.4</u>	35 17 <u>0.5</u>	100 49
		11 15 <u>1.1</u>	26 36 <u>1.6</u>	64 90 <u>0.9</u>	101 141
	Agrarian	5 20 <u>0.5</u>	12 49 <u>0.8</u>	83 334 <u>1.1</u>	100 403
	Total	9 56	16 96	74 441	99 593

dent's career-entry level affect mate selection? Assuming (quite reasonably) that he entered the labor force before marriage, how much did the level of entrance influence the marriage, given a particular origin level? The zero-order correlation between respondent's entry level and wife's origin level is .45; the partial coefficients (by respondent's origin level) are: nonmanual, .26; manual, .32; agrarian, .21. Thus, the level at which the respondent entered the labor market appreciably influenced his opportunities in the marriage market. According to Table 2's

percentage distributions, for instance, those respondents who entered at the agrarian level were married predominantly to women of agrarian background regardless of their own social origins (though they, too, were predominantly agrarian in origin).

The prospective groom's origin status was still an important consideration. A comparison of the nonmanual inheritors with ascendants to a nonmanual entry, for example, shows that 65 per cent of the former but only 42 to 43 per cent of the latter married daughters of nonmanual farmers. Thus, to

some extent marriage expressed attraction between families as well as individuals. Nevertheless, the nonmanual entry achievers married up roughly three times more often than manual inheritors (15 per cent) and about eight times more often than agrarian inheritors (5 per cent).

Table 2's matrices fairly clearly support heterogamy as evidence of occupational mobility: those who married up tended to be occupational achievers; those who married down tended to be occupational skidders. This fact is revealed by the theoretical ratios on the table's principal diagonal (the inheritance-homogamy categories), all of which approach unity (1.2, 1.1, and 1.1), in contrast to the largest ratios, all of which are associated with mobile respondents who married into their destination classes (e.g., nonmanual-to-agrarian skidders with wives of agrarian origin, 3.3; manual-to-nonmanual achievers with wives of nonmanual origin, 2.1; agrarian-to-nonmanual achievers married to daughters of nonmanuals, 4.6).

A study of outflow percentages for the categories of inheritors, achievers, and skidders (derivable by collapsing Table 2) yields the following summaries. Of the inheritors, 68 per cent were homogamous, with a range from 50 per cent for the manuals to 83 for the agrarians; hypergamy exceeded hypogamy by a small margin, 19 versus 13 per cent ($Tau_c = .35$). Among the achievers, and with reference to their origin locations, 50 per cent were homogamous and 43 per cent married above origin level. Nearly identical proportions of the agrarian-to-nonmanual and manual-to-nonmanual achievers (43 and 42 per cent) married into their destination class; only 25 per cent of the agrarian-to-manual achievers contracted marriages with daughters of blue-collar workers, but another 11 per cent married even higher. Ranking the achievers first by origin and then by entry status (i.e., M-NM, A-NM, A-M), $Tau_c = .31$. Finally, 28 per cent of the skidders contracted within-class unions and 69 per cent married below origin level (again based on husband and wife origin comparisons). Skidding into agrarian career entries was rare, and usually followed by hypergamy, especially among the nonmanual-to-agrarian skidders. Those who dropped into blue-collar first jobs married into the desti-

nation class about 42 per cent of the time, and another 27 per cent married agrarian daughters. Again ranking first by origin and then by entry status (i.e., NM-M, NM-A, M-A), $Tau_c = .19$.

FINDINGS: HETEROGAMY AND RESOCIALIZATION

The theoretical ratios in Table 2 show a propensity among achievers and skidders to marry in the direction of their mobility. We turn now to our second question: did the mobile respondents who married in the direction of their mobility adopt the socio-political attitudes of their destination classes more often than the remaining mobile respondents? To test this hypothesis, we shall examine attitudes in the following four areas:

1. The respondent's perception of distributive justice in his society, as indicated by answers to the question, "In your opinion, when things go well in Italy, who stands to profit most?"¹⁸ We assume that answers to this question are particularly powerful indicators of class behavior. Hence differences in this respect between the homogamous and heterogamous mobile are especially revealing as evidence of probable class resocialization.

2. The respondent's conception of the degree to which perceived class boundaries restrict social intercourse, as indicated by answers to the question, "In your opinion, do those who belong to a given social class tend to avoid relations with persons belonging to another social class?" Though the question was generally addressed ("those who belong"), it seems reasonable to assume that the responses incorporated individual experiences. An interviewee who answered "yes" was more likely to have encountered class restrictions on social intercourse than one who answered "no." The data here, however, are probably more powerful in distinguishing between homogamous and heterogamous achievers than between the corresponding categories of skidders, assuming that a nonmanual-to-manual skidder was less discriminated against by manual old-timers than a manual-to-nonmanual achiever by nonmanual old-timers.

¹⁸ The response categorizations for the four interview questions are detailed in the notes to Table 3.

3. The respondent's perception of the character of inter-class relations, as indicated by answers to the question, "Judging by your experience, what kinds of relations exist among the various social classes? Would you say that on the whole they are relations of amity, hostility, or something else?" Again, we assume that an interviewee's perception of class relations expressed his own encounters with members of other classes.

4. The respondent's perception of the interest-group representations of his society's political parties, as indicated by answers to the question, "In your opinion, which party in Italy best defends the interests of people like you?" We prefer this question to one which solicits actual or intended voting behavior. It avoids such phenomena as the "generic dissent" vote, prevalent in Italy. Many Italians, especially males, who vote for the Communist Party, for instance, do so primarily to express serious criticism of the existing regime, the state bureaucracy and/or the Church's intervention in secular affairs, and not to express strong sentiment for the Communist Party itself.

Inheritors. Before examining these data, we must first ask if the non-manual, manual, and agrarian career-entry inheritors differed significantly on the four response items. If not, it would be meaningless to interpret observed differences in mobile category items as resocialization evidence.

In fact, comparisons reveal that entry inheritors differed significantly (Table 3, Panel I).¹⁴ The manuals and the nonmanuals formed two extremes, while in three of four cases the agrarians fell roughly in the middle, somewhat closer to nonmanuals than manuals. This finding is not surprising. Agricultural people are the least educated, and hence lack the intellectual power required for "realistic" or self-interested definitions of highly abstract situations of political disequilibria. Moreover, agrarians have been traditionally dominated by the church hierarchy and by their landlords and other gen-

try (the *signori*), and they are accustomed to taking the cultures, including the political cultures that come from the traditional power structures, as convenience, if not fact. New egalitarian beliefs and ideologies have a hard time breaking the "cake of custom." Thus, modern proletarian parties such as the parties of the left purport to be, are more than normally hindered in their efforts at political education by the agrarians' distrust of political parties and bureaucratic phenomena in general (cf. LaPalombara, 1964; Lopreato, 1967b; Tarrow, 1967; Dolci, 1968). There is one instance, however, in which agricultural workers were similar to their urban counterparts and that concerns the perception of a situation in which the results of prosperity accrue to those whose coffers already overflow: the powerful, the privileged, the elite. Here, no mistake is possible: the peasant has for millennia suffered poverty, all manner of abuse, and exploitation. And he is neither unaware of this fact nor reluctant to express it.¹⁵

Continuing with our main objective, Table 3 also reports comparative data for three categories of career-entry mobile respondents: the manual-to-nonmanual and agrarian-to-manual achievers and the nonmanual-to-manual skidders. Data for only one of the two possible patterns of heterogamy in each category are recorded, because of restricted base N's.¹⁶ Of the sixty-two manual-to-nonmanual achievers, for instance, only seventeen had married women of agrarian origin. Note that excluding these comparisons does not weight the data analysis in favor of the hypothesis of heterogamy effects; indeed, examination of the excluded cases indicates precisely the opposite.¹⁷

¹⁵ See Form and Rytina (1969) for a discussion of ideological shifts between abstract and concrete issues.

¹⁶ We have also excluded the nonmanual-to-agrarian, manual-to-agrarian, and agrarian-to-nonmanual mobility categories because of inadequate N's.

¹⁷ As in most studies of mobility and resocialization, the data in this analysis are not longitudinal. We must therefore assume that originally the mobile respondents did not differ significantly in attitudes and evaluations from their then class peers. Furthermore, since mobility comparisons are origin-to-career entry (which for different respondents occurred a different number of years before the interviewing), we must assume that

¹⁴ These comparisons incorporate the control of wife's origin; however, the distributions are nearly identical when the control is removed. For example, respectively 43, 53, and 54 per cent of the total nonmanual, manual, and agrarian entry-inheritor categories saw elitist advantage in distributing rewards in society.

MOBILITY AND SOCIO-POLITICAL ATTITUDES

Table 3. Socio-Political Attitudes of Respondents, by Origin-to-Entry Mobility and Wife's Origin.

Origin-to-Career Entry Mobility Categories	Wife's Social Origin	Percentage of Respondents Who Said:				
		Elites Benefit ^a	Class Restricted ^b	Hostile Relations ^c	Leftist Party ^d	N
Inheritors:						
Nonmanual	Nonmanual	41	41	29	21	113
Manual	Manual	53	60	46	46	145
Agrarian	Agrarian	55	54	35	29	334
Achievers:						
Manual-to-Nonmanual	Nonmanual	38	38	23	35	26
	Manual	58	58	32	37	19
Agrarian-to-Manual	Manual	61	64	53	36	36
	Agrarian	50	59	58	46	90
Skidders:						
Nonmanual-to-Manual	Nonmanual	56	60	24	28	25
	Manual	74	62	44	47	34

^a"In your opinion, when things go well in Italy, who stands to profit most?" "Elite" includes such responses as "the rich," "capitalists," "industrialists," "entrepreneurs," "bankers," "the upper classes," "the government," "the revenue service," "politicians," "the ruling class."

^b"In your opinion, do those who belong to a given social class tend to avoid relations with persons belonging to another social class?"

^c"Judging by your experience, what kinds of relations exist among the various social classes? Would you say that on the whole they are relations of amity, hostility, or something else?"

^d"In your opinion, which party in Italy best defends the interests of people like you?" "Leftist" includes the Communist, Socialist, and Social Democratic Parties, plus a few cases of unspecified "leftist party."

Achievers: Manual-to-Nonmanual. Two items in Panel II, Table 3, clearly reveal that the heterogamous mobile were more likely to resocialize into the socio-political perspectives of the destination class than were the homogamous mobile. Thirty-eight per cent of the hypogamous achievers, in contrast to 58 per cent of their endogamous counterparts, perceived both disproportionate elitist benefit in the distribution of rewards and a tendency toward class restrictions on social intercourse; these percentages are similar to the corresponding figures for the endogamous manual and nonmanual old-timers, and indeed show a slight over-conformity to middle-class behavior by the hypogamous achievers. Similarly, data on mate selection clearly discriminated responses to the class relations item in the

expected direction, though both hypogamous and endogamous achievers evidenced socialization. The frequency with which the two groups selected a leftist party did not differ appreciably, though again resocialization was apparent.

Achievers: Agrarian-to-Manual. Only two of the four response items here test the hypothesis, since endogamous old-timers of the origin and destination classes did not differ appreciably on the class boundary and distributive justice questions. Data on the respondents' mate selections did discriminate among the responses to the remaining two items—party preferences and perceived inter-class relations—but in a direction opposite to that anticipated. For example, those who married into the destination class chose a leftist party and perceived hostile class relations less often than those who married at origin level.

there were no significantly biasing "decay effects" in the attitudes and evaluations being examined.

The factor primarily responsible, in a statistical sense, for these reversals of expected percentage differences was the relatively high rate with which endogamous achievers selected a leftist party and characterized class relations as hostile. Indeed, as many chose a left-wing party as did the urban blue-collar old-timers (46 per cent); and *more* of them (58 versus 46 per cent) saw the classes as hostile toward one another. Why should endogamous manual-entry achievers have displayed "working-class behavior" "with a vengeance" more often than manual-entry achievers who married daughters of working-class people?

At least partial explanation lies in the fact that more endogamous than heterogamous achievers were agricultural day-laborers, who migrate seasonally between farm and town. Their experience of "uprootedness" inclines them heavily toward leftist political attitudes. By the time of the interview, for example, more endogamous than hypogamous achievers (by a factor of about two-fifths) had skidded back to origin level. Unfortunately the relevant cell N's are much too small to permit appropriate tests, but this "career skidding" probably helps to explain the endogamous achievers' leftist proclivity, and thereby the reversal of the expected differences that is recorded in Table 3. Moreover, on the average, agrarian achievers married to wives of manual origin had entered the working class both more permanently and at a higher level. In considering resocialization, one must also recognize that our manual class includes highly skilled categories (like artisans) as well as unskilled occupations. The latter are far more left-wing than the former, and it is these that endogamous manual-entry achievers entered and left. In any case, whatever the facts, our hypothesis is quite problematic at this point.

Skidders: Nonmanual-to-Manual. For three of the four response items, data on mate selection significantly differentiated the responses in the expected direction. Skidders who married at destination level were less like stationary nonmanuals and more like the stationary manuals, while the reverse was true of endogamous skidders.¹⁸ Hypo-

gamous skidders were especially more likely than the homogamous to see elite advantage in the benefits of prosperity, but even here resocialization made homogamous skidders indistinguishable from manual old-timers (56 versus 53 per cent).¹⁹ Finally, the two groups were alike in answering the question on class boundaries, though again both had resocialized.

Summary. In six of the comparisons available in Table 3, the data indicate that mobile individuals who married in the direction of their mobility were more likely to resocialize in the socio-political attitudes of their destination class than mobile persons who married in their origin class. In two other comparisons—perceptions of interclass relations and party preferences among the manual-entry achievers—the data were disconfirming *prima facie*. In the two remaining comparisons, there were no marriage pattern differences in the probability of resocialization.

CONCLUSION

The majority of evidence in the foregoing study supports the hypothesis that of all inter-class mobile men, those heterogamous in the direction of their mobility (concordant heterogamy) were the most likely to have adopted the political culture of their destination class. All six comparisons confirming the hypothesis pertained to origin-to-career entry mobility across the manual-nonmanual boundary. In contrast to the boundary between agrarian and lower-strata manual occupations, the social boundary of style of dress, speech, mien, and the like, that separates the urban blue-collar and white-collar worlds is somewhat more difficult to cross. Thus, it may be argued, differential per-

cent of endogamous skidders, in contrast to only 26 per cent of skidders who married daughters of blue-collar workers, had reclaimed nonmanual positions by the time of the interview.

¹⁹ Indeed, taking nonmanual-to-manual skidders as a whole (i.e., disregarding distinctions of marriage), they not only resocialized into their destination class attitudes on the question of elite benefits, but "overshot" by 16 percentage points. If the manual-to-nonmanual achievers sometimes overshoot upwardly in expressing a "*plus royaliste que le roi*" sentiment (cf. e.g., Lopreato, 1967a), these skidders might be expected to express a contrasting sentiment of "*plus royalement baïsé que toi!*"

¹⁸ An important dynamic undoubtedly at work here was the reacquisition of lost position: 44 per

formance in the marriage market makes a greater difference for the man moving across the manual-nonmanual line than for the man crossing the manual-agrarian boundary.

We have reasoned that concordant heterogamy indicates greater social acceptance by old-timers of the destination class, and that greater acceptance fosters the mobile person's resocialization. We have not, however, directly examined the assumed empirical relationship between concordant heterogamy and social acceptance for want of appropriate data on the latter. Using data on heterogamy as a substitute for direct information on the various dimensions of acceptance, while defensible for a crude aggregate level of analysis, is inadequate for specifying in detail the relationship between resocialization and social acceptance. For one thing, the substitute data encompass not only the social-psychological dynamics of acceptance per se but also structural factors, such as the extension of social networks and alliances across stratum or class boundaries. Achievers who marry upward, for example, should have a proportionately larger number of contacts and alliances with people in the destination class (affinal relatives, friends, etc., in addition to those made otherwise) than those who marry at or below origin level. This circumstance alone constitutes an advantage in the resocialization process: the hypogamous achiever has more human resources upon which to draw in his efforts to become a "proper" member of the higher class.

While structural factors are undoubtedly important, their impact on resocialization will be qualified generally by the social-psychological dimension of acceptance. In the example cited in the previous paragraph, the degree to which the hypogamous achiever can be influenced by his more extensive social contacts will depend in some measure on the degree to which these new peers accept him. A newcomer's acceptance, in turn, will depend on (1) a number of social-psychological conditional factors—for example the old-established members' perception of whether, to what extent, and by what means the newcomer has been mobile—and (2) a number of contextual factors, such as the importance to the old-timers of the perceived fact, extent, and means of the

(observer-defined) newcomer's mobility—i.e., assuming the newcomer is defined as such by the old-timers, under what circumstances and to what degree does that concern them? These and related questions are critical to understanding the dynamics of mobility, heterogamy, social acceptance, and class resocialization (cf. Strauss, 1971). Very likely, concordant heterogamy fosters the newcomer's social acceptance, which in turn fosters his resocialization, under certain conditions and in certain contexts. Future research must discover the conditionally and contextually differentiated impacts of heterogamy on social acceptance and of social acceptance on resocialization.

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THE ANALYSIS OF MOBILITY PROCESSES BY THE INTRODUCTION OF INDEPENDENT VARIABLES INTO A MARKOV CHAIN *

SEYMOUR SPILERMAN

University of Wisconsin

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A major drawback to the traditional Markov formulation of social mobility is that it assumes homogeneity of transition behavior among persons in an origin state. This requirement has detracted from considering the ways in which the transition probabilities vary among individuals. This paper introduces a regression procedure which allows a heterogeneous population to be examined within a Markov framework. The advantages of this formulation are threefold: (a) it facilitates determining the sources of variation in the population transition probabilities; (b) it permits over-time change in the transition probabilities resulting from population shifts on particular attributes to be distinguished from structural change, in which the rules governing transitions have altered; (c) it enables construction of individual-level transition matrices, which are necessary for projecting to the k-step population matrix in the presence of heterogeneity. These techniques are applied to an analysis of geographic migration data.

1. INTRODUCTION

THE use of Markov chains as a structure for analyzing change in the distribution of a population has achieved considerable popularity in recent years. Though stringent assumptions must be met by the data for the Markov model to be formally applicable [see McFarland (1970) for a dis-

cussion of these requirements], it will reproduce most social processes reasonably well for a few time periods even when the requirements are not satisfied. This fact has encouraged its application to problems such as projecting growth and migration (Fuguitt, 1965; Tarver and Gurley, 1965), analyzing social mobility (Prais, 1955; Matras, 1960; Lieberman and Fuguitt, 1967), and measuring social distance (Beshers and Laumann, 1967), in addition to its more conservative use as a base line model for assessing the extent to which a process diverges from the Markov assumptions (Hodge, 1966).

Aside from computational simplicity, this model is attractive because it focuses on interrelationships as a *system*. Over-time change in the state distribution of a popu-

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lation is viewed as a consequence of the interdependencies acting through time and, perhaps, changing in the process as well. This formulation leads to a consideration of properties of dynamic systems such as "equilibrium" and "rate of convergence," concepts rarely discussed in causal analyses of change, yet fundamental to understanding adaptive social systems. Unfortunately, several difficulties emerge when applying Markov models to the study of social mobility, even when each *individual* in the population conforms to the central assumption of a first order Markov process, namely that his probabilities of making particular transitions are determined solely by his present state, independent of past history. These difficulties arise because traditional Markov theory is concerned with state changes by a single individual, while in applications to social mobility we consider the movements of an entire population.

(a) *Heterogeneity*. As several researchers (Blumen, Kogan and McCarthy, 1955; Goodman, 1962; Hodge, 1966; McFarland, 1970) have noted, the Markov model fails to provide correct projections in the presence of population heterogeneity, a condition which is common in many instances of social mobility. If individuals differ either in their mobility rates or in the transition matrices governing their movements, the observed k-step matrix often will display higher diagonal values than predicted from a Markov model. Consequently, there is a need for devising projection methods that are suitable for heterogeneous populations.

(b) *Change in the transition probabilities*. Change over-time in the elements of a transition matrix is commonly attributed to structural alterations in the underlying relationships. For instance, successive intergenerational occupational mobility matrices may differ because technological developments have altered the demand for particular vocational skills. This is usually what is meant by a "non-stationary" process; in this example, the rules which condition a son's occupational alternatives in terms of his father's position would have altered. However, change in the elements of the population transition matrix can also derive from heterogeneity. As McFarland (1970: 469) has indicated, the p_{ijt} elements in the

one-step population transition¹ matrix at time t ,

$$P_t(1) = \begin{pmatrix} p_{11t} & \dots & p_{1mt} \\ \vdots & & \vdots \\ p_{m1t} & \dots & p_{mmt} \end{pmatrix} \quad (1)$$

where $\sum_j p_{ijt} = 1$ for all i , represent averages of the origin state rows from the individual-level transition arrays. Thus $P_0(1)$ and $P_1(1)$, the one step population-level matrices at times $t=0$ and $t=1$, may differ because the distribution of individuals (and hence the weighted sum of the individual-level matrices) is different at the two time points *even though the rules governing transitions have remained unchanged*. Consequently, there is a need to distinguish between genuine structural change (where the rules have altered) and change in $P_t(1)$ which results solely from demographic shifts in the population.

(c) *The analysis of heterogeneity and change*. Related to the above considerations is the necessity for making sociologically meaningful statements about the components of heterogeneity and change. Since the Markov formulation focuses on the *results* of a process (the population transition probabilities and the state distribution of the population) rather than on the determinants of change, it has not been very useful as an analytic tool, as distinct from a projection device. With the explicit consideration of heterogeneity, however, two types of analyses become feasible: (1) the p_{ij} elements in the population transition matrix can be analyzed to determine which social characteristics are responsible for the heterogeneity, and (2) the extent to which demographic shifts on these characteristics account for the over-time change in the transition matrix can be ascertained. Thus, by relaxing the requirement that all persons must transfer according to an identical transition array, we are led to consider the impact of indi-

¹ Strictly speaking, in the presence of heterogeneity $P_t(1)$ is not a transition matrix since it does not provide conditional probabilities for any individual. Nevertheless, the term is appropriate intuitively to this paper's content and will be retained.

vidual differences on the population-level process.

Having argued that these analyses become conceptually meaningful once heterogeneity is formally introduced, our attention is directed to the manner in which they could be carried out. This paper describes a regression procedure which allows the population heterogeneity to be attributed to variations in particular social characteristics. The analysis also provides a framework for apportioning the change in the population transition matrix between demographic shifts and structural alterations. These topics are discussed in section 2. In section 3 consideration is given to constructing individual-level transition matrices, and to projecting from these to the k -step population matrix.

2. HETEROGENEITY IN THE STATE TRANSITION PROBABILITIES

In order to contend with population heterogeneity within a Markov framework, Blumen, Kogan, and McCarthy (1955) proposed dividing the population into two types of persons—movers and stayers. The former are considered to be homogeneous in their transition behavior and follow a Markov chain, while the latter are viewed as permanent residents in their origin states. More recently, Spilerman (1972) has extended this model to a continuous distribution of persons by mobility rate. Each time an individual undergoes a state change, however, his destination is determined by a transition matrix M which is identical for all persons. Thus, in these models, the burden of explaining population heterogeneity is cast entirely upon variations in the rate of movement since, by assumption, the transition matrix is invariant across individuals.

An alternative approach to heterogeneity would be to assume that each individual c makes a single transition in a time interval but follows an M_c matrix unique to him, specifying his probabilities for transferring to the various destination states. Here, heterogeneity would mean that the taste or opportunity for making a *particular* transition is influenced by social characteristics. Thus, in place of locating the impact of individual differences in the rate at which persons move, the present approach sees

them as directly affecting the transition probabilities.

Like the prior extension of the mover-stayer model (Spilerman, 1972), this formulation is also a generalization of the Blumen, Kogan, and McCarthy (1955) approach to heterogeneity. One may conceive of the mover-stayer process as consisting of two types of individuals, *all of whom move in each time interval*. "Stayers" simply transfer according to the identity matrix. The present model is not, however, a generalization of the mover-stayer *extension* (Spilerman, 1972). Were it not for the stochastic nature of that extension, it could be viewed as a process in which every person undergoes a transition in each time interval, with the heterogeneity being expressed by permitting different matrices to be followed. Thus, while some follow the matrix I , others transfer according to M , M^2 , M^3 , or some higher power of M . Now, if a person always moved according to the same M^p matrix, the present formulation would be a generalization of the mover-stayer extension since the individual transition arrays could be either powers of M or other matrices. However, the mover-stayer extension requires only that the *expected number* of transitions be constant for an individual; the actual number (and hence the power of M) varies over the time units in a random manner.

Thus, the present generalization of the mover-stayer model is not an elaboration of the prior extension, but represents a conceptually different approach to heterogeneity. There are instances where either model could apply—industrial mobility and geographic migration are examples. There are also social processes for which only the present model would be applicable such as inter-generational mobility, where the notion of individual differences in the number of transitions made would seem conceptually inappropriate.

(a) *The problem of analyzing heterogeneity.* Casting the problem of heterogeneity in this framework directs attention toward explaining individual differences in the transition probabilities. If persons follow unique transition matrices, this fact should be attributable to differences among them in social characteristics. Consistent with this reasoning, a common procedure for ascertaining

how transition probabilities vary with social attributes is to construct separate arrays for homogeneous subpopulations (Goodman, 1961:867). For example, Rogers (1966) and Tarver and Gurley (1965), using Markov formulations to analyze geographic migration, disaggregate the population by age and race. However, to subdivide a population in this manner is highly inefficient, since even with a large number of observations one usually cannot control on several attributes simultaneously and retain sufficient cases for estimating the transition probabilities. Moreover, the generation of subpopulation matrices complicates the analytic problem of attributing the heterogeneity to particular factors since we must compare arrays which represent complex patterns of change, rather than single elements. Thus, even if it were possible to control on several variables, the problem of disentangling their separate effects would be considerable.

The usual manner for handling complexities of this nature is to resort to regression analysis. In this section we present a method for incorporating independent variables into a transition matrix via a regression formulation and, using the resulting equations, constructing separate transition matrices for subpopulations.² More generally, we wish to consider the reverse of the problem addressed recently by McFarland (1970). Given individual-level transition matrices, McFarland shows how they may be aggregated to obtain the population transition matrix. Our problem, instead, is to *disaggregate* efficiently

the transition matrix for a heterogeneous population. This is a question of greater analytic importance since we seek to expose the effects of heterogeneity on the structure of the population transition matrix.

(b) *The regression model.* Consider the following approach. Let $P_0(1)$ denote the observed one-step transition matrix at t_0 (time 0) for a population (see equation 1; the subscript for time will henceforth be omitted whenever the referent is clear). For the purpose of illustration we will assume that the substantive problem concerns geographic migration although this analysis would apply equally to industrial or occupational mobility. Each cell entry p_{ij} in $P(1)$ therefore denotes the proportion of individuals in system state (geographic region) S_i at t_0 who have migrated to S_j by t_1 . Since $P(1)$ is a *population* transition matrix it has been estimated from the movements of individuals who presumably differ from one another in their probabilities of making a particular transition. For example, the transition from state S_i to S_j may be a function of age, race, occupation, region of birth, or other variables.

Instead of the usual procedure of disaggregating the population into separate transition arrays to reduce the heterogeneity, we construct m^2 regression equations, where m equals the number of system states. For each origin state S_i at t_0 define an indicator variable y_{ij} which equals one for a person if he moved to S_j by t_1 , and is zero otherwise. Corresponding to each origin state S_i , m such variables can be defined ($y_{i1}, y_{i2}, \dots, y_{im}$). Exactly one element of this vector will equal one for an individual originating in S_i , specified by his destination state. Now, using as observations all individuals in S_i at t_0 , regress

$$y_{ij} = a_{ij} + \sum_v b_{ijv} X_v + e_{ij} \text{ for } j = 1, \dots, m \quad (2)$$

where the X_v 's are individual characteristics expected to explain the heterogeneity in the transition probabilities, and e_{ij} is the error term for the ij^{th} equation. This procedure will yield m equations for each origin state S_i , and m^2 equations for the full array.

Consider the ij^{th} entry of the array $P(1)$.

² Somewhat related approaches to heterogeneity have been presented by Coleman (1964:Chap. 6) and Orcutt *et al.* (1961). Coleman decomposes the transition intensities of a continuous-time Markov process by assuming individual flows between states in a structured manner. The states are constructed to correspond to combinations of the independent variables; consequently, the rate of flow between pairs of states indicates the importance of particular variables. By tying the independent variables to states of a discrete-state process Coleman is unable to incorporate variables in a continuous formulation. Also, in contrast with the present analysis, the number of transition intensities to be estimated increases with the number of variables considered. Orcutt *et al.* use regression techniques to estimate probabilities; but, while they discuss Markov processes (1964:286-294), they fail to combine the two models and treat them instead as alternative formulations.

For an individual c in S_i at t_0 , having attribute profile $(\hat{X}_{1c}, \dots, \hat{X}_{rc})$,

$$\hat{y}_{ijc} = \hat{a}_{ij} + \sum_v \hat{b}_{ijv} \hat{X}_{vc} \quad (3)$$

is an estimate of his probability of making the (i, j) transition. This probability interpretation results from the dependent variable having been coded 0-1, and will be of importance in subsequent sections. With the dependent variable coded in this manner, $\hat{y}_{ijc}$ can also be interpreted as the expected number³ of transitions by individual c from S_i to S_j per time unit.

The expected number interpretation is important because, unlike probabilities, these values can be summed over observations. Thus, for the subpopulation in S_i at t_0 , the expected number of S_i to S_j transitions is

given by $\sum_{c=1}^{n_i} \hat{y}_{ijc}$, where n_i is the number of

persons in location S_i at t_0 . Now, letting n_{ij} equal the observed number of persons who have moved from S_i to S_j by t_1 , we have

$$\sum_c \hat{y}_{ijc} = \sum_c y_{ijc} = n_{ij}$$

where the first equality derives from the least squares procedure of fitting a regression line, and the second from the definition of y_{ij} . Since the standard estimation formula for the transition probabilities⁴ of a Markov

chain is $p_{ij} = \frac{n_{ij}}{n_i}$, we obtain

$$\frac{1}{n_i} \sum_c \hat{y}_{ijc} = \frac{n_{ij}}{n_i} = p_{ij}$$

Therefore, substituting from equation 3,

$$p_{ij} = \frac{1}{n_i} \sum_c \hat{y}_{ijc} = \hat{a}_{ij} + \sum_v \hat{b}_{ijv} \left(\frac{\sum_c \hat{X}_{vc}}{n_i} \right) \quad (4)$$

where the terms $\frac{\sum_c \hat{X}_{vc}}{n_i}$ for $v = 1, \dots, V$

can be interpreted as the profile of a "typical" individual in S_i at t_0 .

Equation (4) gives the desired decomposition of p_{ij} in terms of the conditioning variables $(X_1, \dots, X_V)$. We therefore have a matrix of m^2 equations,

$$Y = \begin{pmatrix} y_{11} = \hat{a}_{11} + \sum_v \hat{b}_{11v} X_v, \dots, & y_{1m} = \hat{a}_{1m} + \sum_v \hat{b}_{1mv} X_v \\ \vdots & \vdots \\ y_{m1} = \hat{a}_{m1} + \sum_v \hat{b}_{m1v} X_v, \dots, & y_{mm} = \hat{a}_{mm} + \sum_v \hat{b}_{mmv} X_v \end{pmatrix} \quad (5)$$

in which the significant b 's in an equation indicate the social characteristics responsible for heterogeneity with respect to that transition. In a matrix where all the b 's are insignificant no heterogeneity is present and the traditional Markov formulation would be applicable.⁵

³ For a discrete random variable x ,

$$E(x) = \sum_{x=0}^{\infty} x \text{Prob}(x).$$

If $x=1$ with probability $\hat{y}_{ijc}$ and zero with probability $1-\hat{y}_{ijc}$, $E(x) = 0(1-\hat{y}_{ijc}) + 1(\hat{y}_{ijc}) = \hat{y}_{ijc}$.

⁴ Observed frequencies and estimates of them constrained to yield precisely the observed values will be written without hats. In this paper we do not consider inference problems or distinguish between the observed data as a sample and the parent population.

⁵ Indeed, if the insignificant variables had been deleted and the regression coefficients reestimated we would obtain, from equation (4), $y_{ij} = \hat{a}_{ij} = p_{ij}$.

The matrix Y of regression equations therefore reveals the components of heterogeneity in the population transition matrix. Moreover, $P(1)$ can be calculated directly from Y . When the regressions in each row are evaluated on the "typical" individual profile from their origin state we obtain, $P(1) = \hat{Y}$.

(c) *Disaggregating the population transition matrix.* In addition to reproducing the $P(1)$ matrix, the array Y of regression equations enables transition matrices to be constructed for subpopulations [$P(1)$, matrices]. For example, if one of the variables were a dummy term for race (e.g., $X_R = 1$ for non-whites, zero for whites) then the non-white transition matrix $P(1)_{nw}$ could be estimated from Y by evaluating the regres-

sion equations only on individuals having $X_R = 1$,

$$\hat{P}(1)_{nw} = \left(\hat{p}_{ij} = \frac{1}{n_{i,R} c_R} \sum \hat{y}_{ijc} \right) \quad (6)$$

where $n_{i,R}$ is the number of non-whites in S_i at t_0 , and $\hat{y}_{ijc}$ is specified by equation (3).

This estimate of the non-white transition matrix will not be identical to the observed non-white matrix $P(1)_{nw}$ since the calculations were performed under the assumption that non-whites have b-weights which are identical to those of whites on all variables except possibly an additive term for race (and factors which are interactions with race if these are present). Correspondingly, the $\hat{p}_{ij}$ value for non-whites will differ from its white counterpart as a result of these terms' contributions and because of racial differences in the individual attribute profiles (X_r 's). In this manner, the matrix Y of regression equations can be used to construct a transition array for persons with any given attribute or combination of characteristics.

When dummy variables are used with a single dimension (such as age) or where dummy variable regressors are provided for the main effects and all possible interactions among several dimensions (such as when categorical variables are used for age deciles and for race), then the evaluation of the Y matrix on individuals having a particular attribute combination will yield identical results to that obtained from constructing a separate transition matrix from the observed movements of this subpopulation. The advantages of the regression format are threefold: First, it allows a variable such as age to be treated as continuous. This means a loss of fewer degrees of freedom, and an opportunity to consider several variables simultaneously. Second, by using a continuous variable formulation, one can extrapolate to values for which few or no observations are present. Third, subject to the assumptions of the regression equation, one can construct a separate transition matrix for each *person* in the population. This point will be developed in section 3.

(d) *Geographic migration.* Using data made available by Karl Taeuber from his analysis of residential mobility in the United States (Taeuber *et al.*, 1968), the above procedures were applied to inter-regional

transitions by males. The Taeuber data were collected in 1958 from retrospective reports about prior residences and are described in detail elsewhere (Taeuber *et al.*, 1968).⁶ For the purpose of this study four geographic regions were defined as states of the process: (1) Northeast, (2) North-Central, (3) South, and (4) West. The time points that were used are $t_0 = 1937$, $t_1 = 1944$, $t_2 = 1951$, and $t_3 = 1958$. These were selected to provide residence histories for the adult years of this cohort. Table 1 presents the (t_0 , t_1) transition matrix for the population, calculated from the observed movements.

Individual-level and contextual data are available for a number of population characteristics: birthplace, race, age, occupation in 1958, class of worker in 1958, city size, and duration of stay at each residence. For this illustration the effect of each variable was assumed to be additive. Also, with the exceptions of age and duration of residence, for which there is evidence for non-linear effects (Land, 1969:139; Morrison, 1970: 11, 14), all relationships were assumed to be linear or dichotomous. The following regression model was therefore used,⁷ the observa-

⁶ From the perspective of substantive findings, the reader is cautioned that the data used here are biased. Histories were collected only for the four most recent residences of an individual and for his birthplace. As a result, persons with more than five locations had gaps in their histories and had to be excluded from the analysis. Since individuals with many residences are more likely to have made regional changes, this exclusion results in a reduction in the size of off-diagonal elements, and in the extent of population heterogeneity.

⁷ It is possible to object to this model because of the heterogeneous nature of the $y_{ij}=0$ category when $i \neq j$. Individuals are assigned this value if they failed to move, or if they made a transition to a state other than j . It seems likely that the individual attributes which relate to these two types of behavior will differ; and consequently, persons who make an (i , j) transition should be compared to one or the other of these reference groups, not to a mixture of both. Considering the number of individuals in each group in the present example (Table 1), the regression model is tantamount to comparing $y_{ij}=1$ persons with the subpopulation that failed to move. If the sizes of the off-diagonal elements in the transition matrix were substantial, a formulation which explicitly compares the (i , j) movers with only individuals who make an (i , i) transition should be used. (See Appendix 1 for the development of such a model.) In practice, that model would entail a cost of increasing the discrepancy between the row sums in the estimated transition matrices and the value one.

Table 1. Population Transition Probabilities for Inter-regional Migration, 1937-44

				$\sum_j P_{ij}$	n_i
$P_{1937}(1)$	=	$\begin{pmatrix} .9705 & .0096 & .0121 & .0078 \\ .0069 & .9473 & .0154 & .0304 \\ .0111 & .0280 & .9383 & .0226 \\ .0027 & .0150 & .0168 & .9655 \end{pmatrix}$		1.000 1.000 1.000 1.000	3460 4213 4246 1129

tions being all persons in origin state S_i in 1937:

$$y_{ij} = a_{ij} + \sum_{v=1}^{17} b_{ijv} X_v + e_{ij} \quad \text{for } i, j = 1, \dots, 4 \quad (7)$$

where $y_{ij} = 1$ if a person was in S_i in 1937 and S_j in 1944, and is zero otherwise, and

$X_1 = 1$ if upper white collar, zero otherwise

$X_2 = 1$ if lower white collar, zero otherwise

$X_3 = 1$ if upper blue collar, zero otherwise

$X_4 = 1$ if lower blue collar, zero otherwise

(the farming trades were taken as the base category for the occupation variables⁸)

$X_5 = 1$ if privately employed, zero otherwise

$X_6 = 1$ if government employee, zero otherwise

("self-employed" was taken as the base category for the class-of-worker variables)

$X_7 = 1$ if non-white, zero otherwise

$X_8 = 1$ if resident of a large city in 1937, zero otherwise

$X_9 = 1$ if resident of a medium-sized city in 1937, zero otherwise

("small city" was taken as the base category for the city-size variables⁹)

$X_{10} = \text{age in 1937}$

$X_{11} = \text{years at current residence}$

$X_{12} = \text{number of residences}$

$X_{13} = \text{age in 1937 (squared)}$

$X_{14} = \text{years at current residence (squared)}$

⁸ The occupation categories were constructed as follows: Upper white collar—professional and managerial; Lower white collar—clerical and sales; Upper blue collar—craftsmen and operatives; Lower blue collar—private household workers, service, and laborers; Farm occupations—farmers and farm laborers.

⁹ The city size variables were defined as follows: Large city—population greater than 500,000; medium-sized city—population 10,000 to 500,000; small locale—population less than 10,000.

$X_{15} = 1$ if born in region 2, zero otherwise

$X_{16} = 1$ if born in region 3, zero otherwise

$X_{17} = 1$ if born in region 4, zero otherwise (birthplace in region 1 was taken as the base category for the place-of-birth variables¹⁰).

The most important from among these variables, as judged by a partial F criterion, were selected from each equation by means of a stepwise regression procedure.¹¹ Use of this method is justified when one wishes to obtain an efficient set of predictors by deleting superfluous variables, as we do here, rather than to test theories about particular variables. Table 2 presents the resulting equations.

It is evident from the R^2 values in the bottom row that these seventeen variables do not explain very much of the variation among individuals in migration behavior. None of the entries exceeds .05 in magnitude. Despite these small values it is possible that several of the most important demographic variables which influence migration have been included in the analysis. In dealing with individual-level data relating to an event's occurrence or non-occurrence, a very large idiosyncratic component may be involved so that even small increases in the explained variance require the consideration of many additional variables [see Stinchcombe (1968:67-68) on the relationship between explanations at the individual and aggregate levels].

¹⁰ The region variables were defined as follows: Region 1—New England and Middle Atlantic states; Region 2—East North Central and West North Central; Region 3—South Atlantic, East South Central and West South Central; Region 4—Mountain states and West.

¹¹ The backwards option of the stepwise regression program was used. All variables were initially entered into the equation. They were removed and reentered one at a time according to the test criterion. A .05 significance level was used for cutoff.

Table 2. Unstandardized Regression Coefficients, Estimated from Inter-regional Transitions, 1937-44

Independent Variable ^b	Equation for ^a							
	Y ₁₁	Y ₁₂	Y ₁₃	Y ₁₄	Y ₂₁	Y ₂₂	Y ₂₃	Y ₂₄
Constant	.9420	.0299	.0585	.0240	.1070	.8620	.0394	.0668
X1 x10 ⁻¹	-.2360	.0988	.1150		.0908	-.2310	.1480	
X2								
X3								.0172
X4								
X5 x10 ⁻¹						-.2500		
X6			.0112			-.0453		.0245
X7	.0417	-.0209	-.0220			.0409	-.0256	-.0296
X8 x10 ⁻¹		-.0970						
X9 x10 ⁻²								
X10 x10 ⁻²			-.3120		-.3000	.1380	-.0674	
X11 x10 ⁻¹		-.0714				.2710	-.0479	
X12 x10 ⁻³	.0905	-.0613		-.0450		.1130		-.1070
X13 x10 ⁻³			.4360		.0350			
X14 x10 ⁻³	.0573			-.0239				-.0696
X15	-.1570	.1330		.0302	-.0475			
X16	-.0440	.0265	.0348		-.0472		.0275	
X17						-.1710	.1230	
R ²	.030	.049	.007	.005	.019	.024	.015	.011

	Equation for ^a							
	Y ₃₁	Y ₃₂	Y ₃₃	Y ₃₄	Y ₄₁	Y ₄₂	Y ₄₃	Y ₄₄
Constant	.1060	.0232	.9120	.0225	.0620	.0673	.0534	.8240
X1 x10 ⁻¹		.2800	-.3400					
X2		.0337	-.0397					
X3		.0308	-.0360					
X4		.0248	-.0354	.0165				
X5 x10 ⁻¹	.0682		-.2020	.0937				
X6								
X7	.0217	.0316	-.0636					
X8 x10 ⁻¹					-.1040			
X9 x10 ⁻¹			-.1740	.1200	-.0864			
X10 x10 ⁻²								
X11 x10 ⁻²		-.0761				-.5830	-.1440	.8840
X12 x10 ⁻¹		-.0817			-.0342	-.1090	-.0873	.2220
X13 x10 ⁻³	-.00562		.0239	-.0111				
X14 x10 ⁻³					-.0165	.1520		-.2020
X15	-.0941	.0601			-.0437	.0406		
X16	-.0999		.0647		-.0444		.0382	
X17					-.0435			.0411
R ²	.026	.016	.032	.008	.038	.042	.017	.043

^aEach reported entry has a t-value greater than 2.00.

^bIndependent variables have been scaled by the multiplier listed at the left. For example, the coefficient of X10 in equation Y₁₃ is -.00312.

It must also be remembered that the R² values depend on how well the independent variables distinguish among the movements of *all* persons in an origin state. For example, if Negroes (constituting 10 percent of the observations) were to make a particular transition, but whites were as likely to do so as not, the R² value from a regression against race would be small even though the equation would predict perfectly the migration

behavior of every Negro. It is the b-weights for particular variables and not the R² values which must concern us. Using these we could distinguish the migration behavior of Negroes from that of whites and, in general, any subgroup differences.

Turning to a consideration of the determinants of population heterogeneity, Table 2's equations exhibit the following main patterns:

(1) Upper white collar persons are more likely to make interregional transitions, irrespective of their region of origin (except for the West).

(2) Negroes in the North and Midwest are less likely to move; Negroes in the South are more likely to change region. On the average, a southern Negro has a probability of migrating that is .06 larger than the value for a southern white man with an identical attribute profile (note the coefficient for X_7 in equation y_{33}).

(3) The probability of not making a regional change increases with duration at a residence.

(4) There is a strong tendency for persons to return to their region of birth, whatever the region or wherever they currently reside.

These appear to be the main sources of population heterogeneity with respect to regional change. Not only does the matrix of regression equations provide this information, it also permits separate transition matrices to be constructed for each subpopulation.¹² This is accomplished by evaluating Y on individuals having the appropriate attributes, in the manner described in connection with equation 6.¹³ Matrices for Negroes and upper white collar persons, estimated in this way, are presented in Table 3, alongside the observed transition arrays for these groups. It is evident that this simple regression model, which assumes attribute effects to be entirely additive, reproduces the observed subpopulation matrices reasonably well. These arrays should also be compared with the transition matrix for the entire population (Table 1). The important comparisons involve the main diagonal ele-

ments since the number of observations in off-diagonal cells is very small. These comparisons suggest much the same findings as were reported above in points (1) and (2). The results are not identical, however, since the equations present the *net* effects of particular attributes, while other characteristics of individuals are not held constant in the subpopulation transition matrices.

3. ANALYZING CHANGE OVER TIME

Two problems arise in the analysis of mobility processes within a Markov formulation. One involves determining to what extent demographic shifts are responsible for changes in the one-step population transition matrix. The other concerns how to project forward in time to the k -step transition matrix in the presence of heterogeneity.

(a) *Change in the one-step transition matrix.* The first problem is an analytic one and can be posed as follows: We have profiles of persons which contain, among other attributes, their locations at equally spaced time points, $t_0, t_1, t_2, t_3, \dots$. Using this information, we can construct a sequence of one-step transition matrices $P_0(1), P_1(1), P_2(1)$, etc. These arrays will normally differ from one another as a result of two processes. First, the influence of particular attributes on an individual's probability of making a given transition may change; for example, the demand for skilled labor in a geographic region may decrease, so the contribution from this occupational affiliation to a decision to migrate there would be lowered. Change of this nature means that the b -coefficients in the regressions (equation 5) will differ in successive time periods. This question could be pursued by reestimating the matrix Y of regression equations from the transitions in subsequent time intervals, and examining the sequence of b -coefficients corresponding to each variable.

However, the $P_t(1)$ matrix could also change over time even though the factors determining the movements of individuals, the regression equation coefficients, remain constant. This would result from a shift in the distribution of individuals (attribute profiles) among the system states or, more generally, from a change in the population's demographic structure; that is, in the way particular characteristics are distributed in

¹² This procedure assumes there is at least one person from the subpopulation in each geographic region at t_0 so the regression equations can be evaluated on an attribute profile.

¹³ Because of the requirement that

$$\sum_j p_{ij} = 1$$

in a transition matrix, only $m-1$ equations are actually required for estimating the m probabilities in a row. However, since the error terms of the regressions do not necessarily sum to zero over a subgroup, the row sums will not equal one exactly. All m equations were therefore used to estimate the p_{ij} 's in order to assess the magnitude of the discrepancy. From the resulting row sums, which are presented in Table 3, it is evident that the error term is quite small.

Table 3. Predicted and Observed Transition Matrices for Subpopulations, 1937-44

Predicted ^a	$\sum_j \hat{P}_{ij}$	Observed	n_i
A. Negroes			
$\begin{pmatrix} .9862 & .0001 & .0068 & .0098 \\ .0045 & .9827 & .0058 & .0058 \\ .0275 & .0497 & .8885 & .0289 \\ .0020 & .0112 & .0478 & .9372 \end{pmatrix}$	$\begin{pmatrix} 1.003 \\ .999 \\ .995 \\ .996 \end{pmatrix}$	$\begin{pmatrix} .9687 & .0000 & .0067 & .0067 \\ .0058 & .9825 & .0058 & .0058 \\ .0275 & .0496 & .8881 & .0348 \\ .0000 & .0000 & .0000 & 1.0000 \end{pmatrix}$	$\begin{pmatrix} 150 \\ 171 \\ 947 \\ 12 \end{pmatrix}$
B. Upper White collar			
$\begin{pmatrix} .9496 & .0185 & .0207 & .0082 \\ .0137 & .9321 & .0266 & .0257 \\ .0072 & .0278 & .9461 & .0184 \\ .0018 & .0168 & .0161 & .9645 \end{pmatrix}$	$\begin{pmatrix} .997 \\ .998 \\ 1.000 \\ .999 \end{pmatrix}$	$\begin{pmatrix} .9501 & .0185 & .0206 & .0109 \\ .0138 & .9318 & .0267 & .0277 \\ .0122 & .0277 & .9456 & .0144 \\ .0000 & .0239 & .0269 & .9493 \end{pmatrix}$	$\begin{pmatrix} 921 \\ 1012 \\ 901 \\ 335 \end{pmatrix}$

^aPredictions obtained by evaluating the matrix Y of equations on the subpopulation with the requisite attributes.

the population. For example, as our society becomes more educated, the average earnings of individuals will increase, even though the income return from education may remain unchanged. It is a shift in the distribution of the population on the education variable which is responsible for the change.

Robert W. Hodge's work (1966) contains an example where these considerations would be relevant. Using Gladys Palmer's study of intragenerational occupational mobility in six cities (Palmer, 1954), Hodge computes transition matrices for males during 1940-1944 and 1944-1949. These matrices are not identical, so one may wish to assess the extent to which the differences are due to changes in factors such as the cohort's age, level of education, and industry of employment.

These questions could be examined by estimating the matrix Y of regression equations from data in the initial time interval, then using Y together with the profile of individual characteristics at t_1 (1944) to generate $\hat{P}_1(1)$, and the individual profiles in subsequent time periods, if these are available, to estimate later transition matrices. A measure of the effect of demographic shifts on the p_{ij} elements, assuming a constant relationship between the p_{ij} 's and the individual characteristics, can be obtained by comparing the observed one-step transi-

tion matrix at t_1 , $P_1(1)$, with $\hat{P}_1(1)$, the matrix estimated from Y. The contribution from population shifts is indicated by the extent to which $\hat{P}_1(1)$ provides a better estimate of $P_1(1)$ than does $P_0(1)$, the estimator that would be used with a stationary Markov chain. This procedure therefore measures the change which should occur in a transition matrix from the demographic shifts.

The above discussion can be illustrated with the geographic migration data. For each time interval, 1944-1951 and 1951-1958, the matrix Y of regression equations was evaluated on the profiles of individual characteristics from the appropriate origin year. This operation permits transition matrices to be constructed based on the relationships between individual attributes and migration propensities *which existed during the time interval 1937-1944*. By evaluating the matrix Y on the individual profiles at subsequent time points we can attribute all resulting change in the matrix elements to shifts in the population distribution on particular characteristics.

Tables 4 and 5 present the results of this analysis. Table 4 reports the predictions from the above procedure alongside the observed transition matrices for 1944-1951 and 1951-1958. Comparable arrays are not presented for 1937-1944 since, by the re-

Table 4. Predicted and Observed One-Step Transition Matrices for 1944-51 and 1951-58

Predicted ^a	$\sum_j \hat{P}_{ij}$	Observed	n_i
A. 1944-51			
$\begin{pmatrix} .9733 & .0084 & .0110 & .0061 \\ .0029 & .9621 & .0107 & .0251 \\ .0090 & .0256 & .9502 & .0172 \\ .0014 & .0144 & .0163 & .9668 \end{pmatrix}$	$\begin{pmatrix} .999 \\ 1.001 \\ 1.002 \\ .999 \end{pmatrix}$	$\begin{pmatrix} .9645 & .0087 & .0122 & .0145 \\ .0048 & .9575 & .0120 & .0257 \\ .0114 & .0255 & .9494 & .0136 \\ .0082 & .0291 & .0157 & .9475 \end{pmatrix}$	$\begin{pmatrix} 3437 \\ 4160 \\ 4110 \\ 1341 \end{pmatrix}$
B. 1951-58			
$\begin{pmatrix} .9748 & .0077 & .0141 & .0054 \\ .0025 & .9758 & .0059 & .0218 \\ .0058 & .0229 & .9651 & .0104 \\ .0009 & .0095 & .0131 & .9756 \end{pmatrix}$	$\begin{pmatrix} 1.002 \\ 1.006 \\ 1.004 \\ .999 \end{pmatrix}$	$\begin{pmatrix} .9803 & .0047 & .0091 & .0059 \\ .0022 & .9750 & .0082 & .0147 \\ .0057 & .0134 & .9701 & .0107 \\ .0013 & .0088 & .0067 & .9891 \end{pmatrix}$	$\begin{pmatrix} 3393 \\ 4157 \\ 4015 \\ 1483 \end{pmatrix}$

^aPredictions obtained by evaluating $\hat{Y}$ on the profiles of individual attributes at the origin year.

quirements of ordinary least squares regression, the predicted population transition matrix will be identical to the observed one for this interval (Table 1).

It is evident from comparing the observed transition arrays for the three time periods

that the amount of change in $P_t(1)$ is not very great. Nevertheless, some trends are apparent, the most pronounced being the tendency for the main diagonal elements to increase in size over time. As was suggested, this could be a result of either structural

Table 5. Indices of Dissimilarity Between Observed and Predicted Transition Matrices for 1944-51 and 1951-58

Region of origin	Index of Dissimilarity ^a			
	Between observed 1944-51 transition matrix and--		Between observed 1951-58 transition matrix and--	
	Observed 1937-44 Matrix	Predicted 1944-51 Matrix	Observed 1937-44 Matrix	Predicted 1951-58 Matrix
s_1^b	.7	.9	1.0	.8
s_2	1.0	.4	2.8	.5
s_3	1.2	.3	3.2	.8
s_4	2.0	2.1	1.8	.8
Origin states combined ^c	1.3	.6	2.3	.8

^aID = $50|p_{ij} - \hat{p}_{ij}|$. See Taeuber and Taeuber (1965:236) for a discussion of this index.

^bEach entry gives the percentage of persons who would have to change their destination states to bring the observed distribution into accord with the predicted.

^cThe average of the indices, weighted by the number of observations in an origin state, indicates the proportion of all persons whose destinations would have to be changed to bring the observed distribution into accord with the predicted.

change—an alteration in the attractiveness of regional migration for individuals with a particular set of attributes, or of demographic shifts—changes in the population distribution over the profile of attributes, including region of residence. However, it is clear from the predicted arrays in Table 4 that the pattern of change noted with the observed data can be accounted for by the relationships between regional migration and individual characteristics which existed during 1937–1944. Thus, the increase in size of the main diagonal elements appears largely a consequence of demographic shifts, and not structural change.

Normally, to estimate the one-step transition matrix $P_1(1)$ at some time point t_1 subsequent to t_0 we would use $P_0(1)$. That is, not knowing how the transition matrix is changing, we assume a stationary Markov chain. To investigate the change in $P_t(1)$ over time, and the proportion of it which can be attributed to demographic shifts on the seventeen variables of this analysis, Indices of Dissimilarity were computed between several pairs of arrays. These compare the observed one-step transition matrices at 1944 and 1951 in Table 4, with $P_{1937}(1)$ from Table 1 to obtain estimates of the total change in this array. Also, the observed and predicted matrices for 1944 and 1951 (from Table 4) are compared to measure the amount of residual change not accounted for by population shifts. Table 5 presents the results.

The first four rows report index values for each region of residence in 1944 and 1951. Each entry can be interpreted as the percentage of persons in an origin state who would have to change their destinations to bring the observed distribution of moves into accord with the predicted distribution (Taeuber and Taeuber, 1965:236). For example, the first two entries in row 2 indicate that a 1.0 percent change has occurred in the pattern of movement by individuals residing in region 2 during 1944 to 1951, compared to the preceding interval; but only 0.4 percentage points cannot be accounted for by demographic shifts on the seventeen variables.

Row 5 presents averages of each column's entries weighted by the number of persons in an origin state. These values report the

proportions of the total population who would have to change destinations to bring the pair of matrices being compared into agreement. Although the total change in the observed matrix is small, 1.3 percent between $P_{1937}(1)$ and $P_{1944}(1)$, 2.3 percent between $P_{1937}(1)$ and $P_{1951}(1)$, less than half these values is attributable to structural alterations (0.6 percentage points for the first pair of arrays, 0.8 percentage points for the second). Thus, a major proportion of the change in this cohort's migration behavior is due to shifts in population distribution and not to alterations in underlying relationships.

(b) *The projection problem.* In the absence of heterogeneity, $P(k)$, the observed k -step transition matrix, may be estimated from the Markov property

$$\hat{P}(k) = \prod_{t=0}^{k-1} P_t(1) \quad (8)$$

which reduces to the familiar relation, $\hat{P}(k) = P^k$ when P is constant over time. The projection problem arises because equation (8) does not hold in the presence of population heterogeneity. In this situation, the main diagonal elements of $P(k)$ typically will exceed the corresponding entries on the diagonal of $\hat{P}(k)$.

However, if we assume that each person independently follows a Markov process, then, using the matrix Y of regression equations, we can obtain an estimate of $P(k)$ in the presence of heterogeneity.¹⁴ This is accomplished in the following manner: For each individual c a separate transition matrix $M_c(1)$ is constructed by evaluating the sixteen equations¹⁵ in Y on his attribute profile in 1937.¹⁶ Now, let N_c be a matrix

¹⁴ If none of the b 's in equation (5) is significant, this suggests that the process is Markovian, in which case equation (8) could be used to estimate $P(k)$.

¹⁵ While only $m(m-1) = 12$ regression equations are necessary for estimating the individual $p_{1j|c}$'s, $m^2 = 16$ equations were used to detect the extent to which the row sums will deviate from $\sum_j p_{1j|c} = 1$.

¹⁶ In contrast to estimating the population transition matrix, $\hat{P}_t(1)$, in the preceding section, where only the profiles of individuals at an origin state were used with a row of equations, here each person's profile is inputted to all equations in Y . This method therefore assumes that an individual's

with 1 on the main diagonal of the i -th row and zero in all other cells, where i denotes individual c 's region of residence in 1937. We then have (McFarland 1970:469)¹⁷

$$P(1) = N^{-1} \sum_c N_c M_c(1) \quad (9)$$

where N is a diagonal matrix, $N = \sum_c N_c$. Since each person is assumed to follow a Markov chain with his individual transition matrix $M_c(1)$, the k -step population transition matrix is given by

$$\hat{P}(k) = N^{-1} \sum_c N_c [M_c(1)]^k. \quad (10)$$

Equation (10) holds assuming that the individual transition matrices $M_c(1)$ are constant over time. However, using the methods of the previous section we can relax this requirement and incorporate over-time changes in these arrays *which are produced by identical attribute shifts for all persons*. The most common example concerns age. Change on this variable can be handled by generating time-specific transition matrices $M_{ct}(1)$ in which an individual's age contribution depends on the time period t (and his associated age) to which the matrix pertains. The k -step transition matrix then becomes

$$\hat{P}(k) = N^{-1} \sum_c N_c \prod_{t=0}^{k-1} M_{ct}(1). \quad (11)$$

Although equation (11) derives from a time-varying process at the individual level, all relationships and profile data are from the initial time period. The changing impact of age on an individual's transition probabilities is estimated from the cross-sectional relationships existing in this time interval. Consequently, projection from equation (11) is comparable to projection in a time-homogeneous Markov process in that both models

require data at only two time points for parameter estimation.

The projection procedure can be illustrated with the geographic migration data. Using the individual profiles from 1937 together with a matrix Y' of regression equations, three transition matrices¹⁸ were estimated for each individual: $\hat{M}_{c1937}(1)$, $\hat{M}_{c1944}(1)$, and $\hat{M}_{c1951}(1)$. The array Y' differs from Y , the matrix of equations presented in Table 2, in that variables whose values change with a transition have been removed. Without making further assumptions about the migration process, we have no knowledge concerning characteristics such as city size, duration of residence, or number of residences for an individual subsequent to 1937.¹⁹ For the purpose of projection, Table 2's regressions were therefore reestimated with variables X_8 , X_9 , X_{11} , X_{12} , X_{14} , deleted. By this procedure, the three $\hat{M}_{ct}(1)$ matrices for an individual will differ only as a result of the

¹⁸ Since the p_{1j} elements in $M_c(1)$ are estimated from regression equations which have 0-1 dummy dependent variables, it is possible for the predicted values to exceed one or be less than zero. These are meaningless estimates for a probability, so values outside the 0-1 range were truncated at the probability limits. These adjustments had a negligible effect on the results. On the average, only one out of each 110 estimates fell outside the 0-1 range by as much as .01. Likewise, 96 percent of the time the sum of an individual's row probabilities was within the range .98-1.02. Procedures which constrain the estimated probabilities to the 0-1 range, such as logit and probit techniques (Thiel, 1970), are available. However, the adjustments would have been minor and these methods lack the simplicity of interpretation available to the b -coefficients in a linear regression model which uses a dichotomous dependent variable.

¹⁹ Individual attributes which are contingent on a state change cannot be handled by these computations. Although an $M_c(1)$ matrix indicates probabilities for making various transitions, an individual will make one or another of these, *and his value on such a variable will be altered by the resulting transition*. Variables of this nature can be handled only through a simulation procedure in which each person is assigned to a destination state by comparing a random number with his p_{1j} values (cf. Spilerman, 1971:434), and has characteristics which depend on the transition made assigned to him in a similar fashion. While information on such variables are available from the Taeuber study and were used in analyzing over-time change in $P_c(1)$, we are concerned here with projection into the future and assume the unavailability of these data.

characteristics summarize all relevant information about his behavior. If he were to change states, his transition propensities would conform to those of others in the new state with profiles identical to his.

¹⁷ McFarland fails to note that this equation is well defined only for processes in which some individuals are located in every system state at t . Otherwise the matrix N would be singular and N^{-1} will not exist.

Table 6. Observed and Projected Two and Three-Step Population Transition Matrices for 1937-44 and 1937-51

A. Observed Matrices							
$P_{1937}(2)$				$P_{1937}(3)$			
.9477	.0145	.0194	.0185	.9344	.0176	.0251	.0288
.0083	.9238	.0176	.0503	.0088	.9084	.0199	.0629
.0177	.0445	.9074	.0304	.0203	.0520	.8900	.0377
.0035	.0230	.0186	.9548	.0035	.0239	.0177	.9548
B. Projected Transition Matrices Using Markov Formulation (equation 8)							
$\hat{P}_{1937}(2) = P_{1937}(1)P_{1944}(1)$				$\hat{P}_{1937}(3) = P_{1937}(1)P_{1944}(1)P_{1951}(1)$			
.9362	.0180	.0235	.0219	.9180	.0226	.0318	.0276
.0116	.9084	.0266	.0534	.0136	.8865	.0337	.0662
.0217	.0515	.8916	.0351	.0266	.0627	.8658	.0449
.0108	.0430	.0313	.9150	.0120	.0504	.0370	.9006
C. Projected Transition Matrices Using Method of Equation (11) ^a							
$\hat{P}_{1937}(2)$				$\hat{P}_{1937}(3)$			
.9486	.0179	.0218	.0153	.9336	.0254	.0333	.0225
.0086	.9147	.0238	.0535	.0098	.8978	.0268	.0706
.0181	.0479	.8950	.0381	.0211	.0616	.8669	.0477
.0061	.0290	.0320	.9414	.0101	.0424	.0461	.9288

^aNegative estimates of individual probabilities were set equal to zero, values greater than one were set equal to one.

effect of the seven year-age difference between successive evaluations of Y .

Estimates of $P_{1937}(2)$ and $P_{1937}(3)$ were constructed by using the $\hat{M}_{et}(1)$ matrices with equation (11). These arrays are presented in row 3 of Table 6, below the observed two and three-step transition arrays (row 1), and the estimates of these matrices from a time-dependent Markov chain in which the *observed* one-step population transition matrices for successive time intervals have been multiplied together following equation (8) (row 2). Comparing the main diagonal entries, we see that the present model's estimates are superior to the Markov projections even though the latter are based on observed transitions from each time period. This impression is confirmed when we compute Indices of Dissimilarity between the observed two and three-step arrays and the projected matrices. Table 7 reports these

results separately for each origin state in 1937 and for comparisons between entire matrices. The entries in the last row indicate that while the observed two-step matrix disagrees with the Markov projection by 1.6 percent, the discrepancy with the projected array obtained from the present method is 1.0 percent. The corresponding index values for the three-step projections are 2.4 and 1.8 percent. Thus, with the geographic migration data one does better by projecting from an assumption of heterogeneity, using only data from the initial time interval, than by assuming a Markov process at the population level and using the *observed* one-step transition matrices from each time interval.

4. CONCLUSIONS

A major drawback to the traditional Markov formulation of social mobility is that

Table 7. Indices of Dissimilarity Between Observed and Projected Two and Three-Step Transition Matrices

Region of origin	Index of Dissimilarity ^a between			
	$P_{1937}(2)$ and $\hat{P}_{1937}(2)$.		$P_{1937}(3)$ and $\hat{P}_{1937}(3)$.	
	Projection by-- Markov	eq. 11	Projection by-- Markov	eq. 11
s_1^b	1.2	.5	1.8	1.2
s_2	1.5	.8	2.2	1.3
s_3	1.6	1.2	2.4	2.1
s_4	4.0	1.8	5.4	4.0
Origin states combined ^c	1.6	1.0	2.4	1.8

a,b,c See corresponding footnotes to Table 5.

it assumes homogeneity among persons in an origin state with regard to their transition behavior. This requirement typically leads to underestimating the main diagonal entries when projecting forward in time to the k -step matrix in the presence of heterogeneity; but what is more important conceptually, the Markov model diverts attention from the determinants of the transition probabilities. By formally introducing heterogeneity, however, we can examine the transition behavior of a population in terms of the variables which condition the individual propensities. Thus, the question of who moves and to where receives a sociological answer.

Previous attempts to incorporate heterogeneity within a Markov framework have permitted individual variations in the rate at which transitions occur (Blumen, Kogan and McCarthy, 1955; Spilerman, 1972). At each move, however, a transition matrix which is identical for all persons is used. The problem of heterogeneity was examined from a different perspective in this paper. Instead of requiring that it be expressed in terms of individual differences in the mobility rate, the strategy here was to permit the transition probabilities themselves to vary among persons. Each individual was assumed to follow a Markov process, but in accordance with his own transition matrix, which is estimated by the regression pro-

cedure introduced in section 2. With these individual-level matrices, projection to the k -step array may be accomplished using the method proposed by McFarland (1970).

For sociological analysis a more fundamental matter than projection concerns the analysis of change. The regression formulation contributes to this objective by suggesting the need for distinguishing between two sources of change in the transition probabilities. This may result from demographic shifts, in which the rules governing individual transitions remain intact but the distribution of relevant attributes in the population has altered, or from structural change, in which the rules themselves have altered. In examining geographic migration we found that the former process was primarily responsible for the over-time variation in the transition probabilities. Although our focus in this study was explicitly on accounting for demographic shifts, with structural change being relegated to the residual category, structural change could also be analyzed directly. The most evident procedure would be to re-estimate the regression model in successive time intervals and observe the change in each regression coefficient over the time sequence.

Having now considered two alternative approaches to incorporating population heterogeneity within a Markov framework—by permitting individual differences in the mobility rate (Spilerman, 1972) or in the transition probabilities—what can be said about their comparative merits? Conceptually, there appear to be some social processes for which only the latter model would apply, though both formulations are plausible for a wide range of mobility processes. The present model is preferable in studies of *inter*-generational occupational mobility, since the notion of different rates at which these transitions occur has little conceptual merit. With respect to *intra*-generational mobility, change in industry affiliation, or geographic migration, both formulations seem plausible: persons may differ in the rate at which they move; also, individual characteristics may influence the probability of making particular transitions.²⁰ Thus, the higher geographic

²⁰ Note that an i -to- i transition has different meanings in the two models. In the mover-stayer extension the moves occur randomly in time. An (i, i) transition can therefore be interpreted as a within-region change of residence, if the problem

mobility of white collar persons may be a rate effect since this pattern was noted in all regions; while the differential mobility of Negroes, depending on whether they were residing in the South or elsewhere, could be ascertained only by the model of this paper.

In terms of an ability to make sociologically interesting statements, the model developed here²¹ seems more promising. It enables the different sources of change to be distinguished; also, the notion that individual characteristics relate to the likelihood of making *particular* transitions is appealing. Moreover, it provides more detailed information about the process than does the alternative formulation. In this model, m^2 regressions are carried out,²² one for each pair of origin and destination states; in the mover-stayer extension only a single regression is appropriate to the model (with the dependent variable being the number of moves made by an individual). Nevertheless, the two formulations are oriented to different aspects of heterogeneity and, where both are conceptually plausible, each will complement the other in illuminating the process.

APPENDIX

The regression model presented in the text (equation 5) requires a comparison between individuals who make an S_i to S_j transition and a mixture of two other groups: persons who fail to transfer out of their origin state S_i , and persons who do so but not to S_j . Although both are assigned the same value on the dependent variable, $y_{ij} = 0$, different conditioning variables are probably important for distinguishing (i, j) movers from each of these groups. For example, in geographic migration, duration at a residence is important for differentiating

is one of regional migration, or a within-industry job change if the process concerns industrial mobility. In the present generalization, an (i, i) transition does not distinguish between the absence of a move and an occurrence in which a change of state failed to take place.

²¹ A computer program which performs the calculations associated with this model has been written by David Dickens. Copies may be obtained from University Microfilms, Ann Arbor, Michigan. (Request item S-404).

²² In fact, m^2 systems of equations could be estimated in order to incorporate complex structural assumptions as to the determinants of each p_{ij} in the transition matrix.

movers from non-movers, but probably not for distinguishing (i, j) movers from migrants to a different region. To avoid this heterogeneous character of the reference category, it would seem desirable to compare (i, j) movers with only one of the above groups. A comparison with non-movers seems preferable since the population in the category "movers to a state other than j " will itself change as the destination state is varied.

The most direct procedure would be to perform regressions like those in the text for each off-diagonal element, but take as observations only individuals who make either the (i, j) or the (i, i) transition. Thus, y_{ij} would equal one for persons making the transition of interest and zero for those in the reference group, which now would consist only of individuals exhibiting a common behavior. (The regression for y_{ii} would use the same observations as before since the reference category for non-movers would be all persons transferring out of state S_i .) However, this formulation presents a problem in that the observations for the several regressions in row i no longer will be the same (and equal to n_i). As a result, the simple relationship of equation 4, which allows p_{ij} to be derived from $\hat{y}_{ij}$, no longer pertains. How, then, can this procedure be used to estimate the transition probabilities of a Markov chain?

Consider the following approach: Construct regression equations as indicated in the preceding paragraph. For each transition probability on the main diagonal of $P(1)$ we have, as before,

$$p_{ii} = \frac{1}{n_i} \sum_c \hat{y}_{iic} = \hat{Y}_{ii} = \frac{n_{ii}}{n_i}$$

where the term $\hat{Y}_{ij}$ is introduced for notational convenience and equals $\sum_c \hat{y}_{ijc}$ divided

by the number of observations in the particular regression (which also constitute the range of c). For each off-diagonal term y_{ij} we obtain, summing over the predicted values from *all* individuals in the origin state,²³

²³ Although the regressions have been estimated on $n_{ii} + n_{ij}$ observations, for projection purposes they must be evaluated on all individuals in the origin state.

$$\frac{1}{n_{1.}} \sum_c \hat{y}_{1jc} = \frac{n_{1j} + \epsilon_{1j} g_{1j}}{n_{1.}}$$

where the first term in the numerator results from evaluating the regression on only the individuals used in its estimation [persons making an (i, j) or an (i, i) transition]; g_{1j} is the number of individuals who leave S_1 but make a transition to a state other than S_j ; and ϵ_{1j} is the proportion of these persons who are (incorrectly) predicted by the y_{1j} regression as having made the (i, j) transition.

We assume that $\epsilon_{1j} \approx n_{1j}/(n_{1j} + n_{11})$; that is, the proportion of this subpopulation which is predicted by the y_{1j} regression to have exited from S_1 approximates the proportion in the subpopulation used for estimating the (i, j) regression. Even though all these individuals were observed to leave S_1 , this is a reasonable assumption since the probability of making a transition is actually small for each person.

Substituting for ϵ_{1j} , for g_{1j} ($= n_{1.} - n_{1j} - n_{11}$, by definition), and simplifying the resulting expression, we obtain

$$\frac{1}{n_{1.}} \sum_c \hat{y}_{1jc} \approx \frac{n_{1j}}{n_{1j} + n_{11}} = \hat{Y}_{1j}$$

where the second relation derives from the definition of $\hat{Y}_{1j}$.

Now,

$$\begin{aligned} \frac{\hat{Y}_{1j} \hat{Y}_{11}}{1 - \hat{Y}_{1j}} &= \frac{\left(\frac{n_{1j}}{n_{1j} + n_{11}} \right) \frac{n_{11}}{n_{1.}}}{1 - \frac{n_{1j}}{n_{1j} + n_{11}}} \\ &= \left[\frac{n_{1j} n_{11}}{(n_{1j} + n_{11}) n_{1.}} \right] \left[\frac{n_{1j} + n_{11}}{n_{11}} \right] \\ &= \frac{n_{1j}}{n_{1.}} = p_{1j}. \end{aligned}$$

Consequently, an alternative estimate of $P(1)$ is given by

$$\begin{aligned} &\hat{Y}_{11} \dots \hat{Y}_{1m} \hat{Y}_{11} / (1 - \hat{Y}_{1m}) \\ &\cdot \\ &\cdot \\ &\cdot \\ &\hat{Y}_{m1} \hat{Y}_{mm} / (1 - \hat{Y}_{m1}) \dots \hat{Y}_{mm} \end{aligned}$$

where the calculation of $\hat{Y}_{1j}$ uses all individuals in the origin state.

While the regression equations corresponding to this model derive from compar-

isons which are more valid conceptually than the approach used in the text, the row sums of the estimated probabilities tend to exhibit a greater deviation from the value one. This procedure, therefore, removes the error in the regression specification, but at a cost of introducing error into the forecasting procedure. As a result, it seems preferable to use the text method for constructing transition matrices when the off-diagonal elements are small, since the reference observations would then consist largely of non-movers.

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SOME METHODOLOGICAL PROBLEMS IN EXPLAINING SOCIAL MOBILITY *

ROLAND K. HAWKES

Southern Illinois University

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It is argued and illustrated that conceiving and measuring social mobility in the conventional way, as a difference between two statuses, may lead to gross misinterpretations of evidence. These difficulties may be avoided by considering individual status measures and their causal relationships.

Introduction

TRADITIONALLY, the study of the social mobility of individuals has conceived of mobility as the difference between an initial and subsequent status. Measures have been formed either by directly subtracting the two variables or by similarly reducing the property space formed by the two measures' cross-classification, as by classifying into upward, stable and downward or into some finer set of ordinal gradations. Explaining social mobility, is taken as explaining the difference between earlier and later statuses. For example, intergenerational mobility is viewed as the difference between father's status and son's eventual status. Examples of this approach abound in sociological literature.¹ The problem of explaining

intergenerational occupational mobility is conceived as the problem of explaining that difference.

Blau and Duncan take a new approach (1967). Rather than seeking to explain the difference between father's and son's statuses, they try to explain the son's status, and include the father's status among the explanatory variables.

At first glance, the two approaches seem to differ merely in style and taste. However, the authors (1967, 194-199) show that when mobilities are measured as differences and mobility measure correlations are analyzed, some strange and misleading things begin to happen. Mobility measure correlations are composed of individual status measure correlations and, since the mobility measures are formed by subtraction, some correlations are weighted negatively into the total. The authors show that correlations

* I would like to thank Ernest K. Altx for his helpful comments and suggestions on an earlier draft of this paper.

¹ For example, Coleman and Neugarten (1970) categorize mobility as upward, stable and down-

ward and seek to assess its causes. Another recent example is Thompson (1971) who compares working class "stables" with "upward mobiles."

between mobility measures involve things true by definition which make substantive interpretations of these correlations nearly impossible. In another place, Duncan (1966) shows that in examining consequences of social mobility, individual status variable effects are confounded with the effects of their differences when they are not explicitly controlled for.

This paper develops some of these points and shows that if mobility is measured as a difference between statuses or in some equivalent way, our interpretations will most likely be egregiously in error. The problem will be illustrated first by a mathematical argument with a simple formal model of the stratification process, and, second, by some simple dichotomous contingency tables which show that the problems are not simply an esoteric property of equations.

A Model of the Stratification Process

Let us invent a simple model of the stratification process, measure mobility in the traditional ways and point out the errors of interpretation which occur. The model will include father's and son's statuses. Whether we are talking about education, occupational prestige or some other status variable is not relevant to the discussion since we are concerned with the logical problems involved in the analysis rather than the substance. Let us only imagine that the statuses are measured in some meaningful sense. Next, we introduce a variable that intervenes between the two statuses, like "achievement orientation," defined loosely as the son's psychological need and motivations to achieve status and position.² A plausible model of variable interrelations is that father's status causes achievement orientation in the son and that son's status is caused by achievement orientation and also directly, or at least not through another explicit variable, by father's status. We further conceive that father's status, son's status and achievement orientation have numerous other causes than the model's variables and that these non-model variables may be considered

statistically independent. Further, we consider a social mobility measure which is the difference between father's and son's statuses.

The variable symbols are as follows:

F = Father's status

S = Son's status

A = 'Achievement orientation'

M = Social Mobility

$\zeta_r, \zeta_s, \zeta_a$ = Independent exogenous variables affecting F, S, and A respectively

α, β, γ = Slope parameters relating the variables

The above model may be stated in equations as:

$$F = \zeta_r$$

$$S = \alpha F + \gamma A + \zeta_s$$

$$A = \beta F + \zeta_a$$

$$M = S - F$$

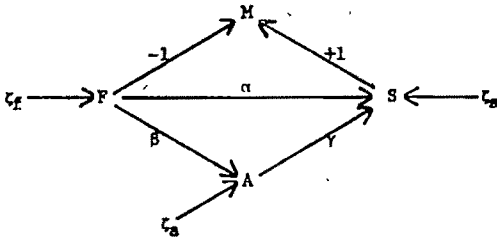
The model may also be expressed by the causal diagram³ shown in Figure I. This mathematical model defines son's status as a linear function of achievement orientation and of father's status with parameters γ and α respectively and of other causes external to the model. Achievement orientation is a linear function of father's status with parameter β and of causes outside the model. Father's status is determined by variables not included in the model. Mobility is defined as the arithmetic difference of son's and father's statuses. There are no other inputs to the mobility measure since its value (with the possible exception of measurement error) is caused only by those two variables. Again, the model's realism and sensibility except in a broad and loose sense is not at issue. It is intended to broadly represent the process which links these kinds of variables and will serve to demonstrate the logic of our argument.

Now, the model relates achievement motivation to son's status through the parameter γ and within the model an appropriate

² The use of the variable *achievement orientation* is intended only to lend intellectually plausible substance to the logical form of the argument. We do not intend to contribute to nor criticize the literature involving that concept.

³ The conventions used in constructing diagrams are the same as those used by Stinchcombe (1968) and the sources he cites, and by econometricians such as Johnston (1963). They are intended to correspond exactly to the equations defining the model and to mean neither more nor less than the equations.

Figure I. A Model of the Stratification Process*



*See the text for a discussion of the meanings of the various symbols used.

solution to the relationship of achievement motivation to status attainment will involve estimating the parameter γ and the proportion of variation in son's status accounted for by achievement motivation. The traditional approach would inquire into the relationship between achievement orientation and social mobility as measured by the difference between the father's and son's statuses.

Some Consequences of the Model

To investigate the kind of answers the traditional method will yield, let us delete reference in the model to the two status variables and focus on achievement motivation and mobility expressed as functions of the exogenous causes. Substituting expressions for S and F for their occurrences in the expressions for A and M yields:

$$A = \beta \zeta_f + \zeta_a$$

$$M = \zeta_s + (-1 + \alpha) \zeta_f + \gamma A = \zeta_s + (-1 + \gamma\beta + \alpha) \zeta_f + \gamma \zeta_a$$

As above, the equations may be represented as the causal diagram shown in Figure II. The equations and the diagram, with reference to son's and father's statuses deleted, show mobility to be a linear function of achievement orientation with parameter γ and of the exogenous causes of father's status with parameter $-1 + \alpha$. The negative *one* results from the original contribution of father's status to the mobility measure and the positive α results from the indirect effect of father's status on mobility through son's status. Achievement orientation is a linear function of the external causes of father's status with parameter β .

At this point we may observe that any technique which examines the relationship between achievement orientation and mobility will reveal not just the relationship between them due to γ , the effect of achievement orientation on son's status attainment, but also a spurious component of association which derives from the joint dependence of achievement orientation and mobility on father's status.

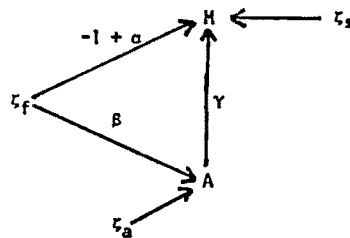
To explore the nature of misinterpretations which may arise, let us formally analyze the sources of association between mobility and achievement orientation. This association is most conveniently expressed as the covariance of the two variables. This covariance will be the numerator of both the correlation coefficient and the regression coefficient between the two.

Let us assume that the variables ζ_s , ζ_f , and ζ_a are uncorrelated and have means of zero and variances σ_s^2 , σ_f^2 , and σ_a^2 respectively. The covariance will then be the expected value of the product of M and A . Its expression may be generated by multiplying the equations for M and A and taking expected values yielding:

$$\text{COV}(M, A) = \beta(-1 + \alpha + \gamma\beta)\sigma_f^2 + \gamma\sigma_a^2$$

With this expression for the covariance of mobility and achievement we may experiment conceptually with the model to investigate the behavior of the covariance. If we know the covariance behavior, we know the correlation coefficient behavior, since the covariance is its numerator. If we further assume that all measured variables are scaled to have variances of one, the covariance becomes the correlation coefficient.

Figure II. A Reduced Form of the Model*



*See the text for a discussion of the meanings of the various symbols used.

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Imagine that γ equals zero. That is, assume that the world is such that achievement orientation depends on socioeconomic background but is not a cause of eventual status attainment. If γ equals zero, then the covariance of mobility and achievement orientation becomes:

$$\text{COV}(M, A) = (-1 + \alpha).$$

Since we are assuming that the measured variables are standardized, the parameters α and β may be assumed to be less than one. Then $-1 + \alpha$ will be a negative number and the expression for the covariance will be negative and hence the association between achievement orientation and social mobility will be negative.

If the world is as we assume, that is, if achievement orientation is unrelated to the son's eventual status achievement, and if social mobility is measured as the difference between the two status variables, we will conclude that achievement orientation is negatively correlated with social mobility. This will be technically correct but our substantive interpretation would be much at variance with the substantively correct conclusion. We should conclude that achievement orientation is unrelated to "social mobility" in the sense that we intended the question, but by measuring mobility as the differences between father's and son's statuses, we will generate a wrong and probably seriously misleading answer, and we will foster the policy prescription that to ensure upward mobility, achievement orientation should be minimized.

One way to view the problem is to recognize that in *subtracting* father's status in constructing the mobility measure, we have ensured that by definition a negative component will be present in the association between mobility and any variable positively associated with father's status. It is this negative component which leads to the substantively wrong conclusion.

A second difficulty may be illustrated by assuming that α equals zero, that is, that the world is such that there is no direct link between father's status and son's eventual status attainment. The entire effect of father's status on son's derives from the intervening variable, achievement orientation. In the tradition of survey analysis we

may say that the relationship between father's and son's statuses is *interpreted by* achievement orientation.

The equation for the covariance of mobility and achievement which results from this assumption is

$$\text{COV}(M, A) = (-1 + \beta\gamma) + \gamma$$

The first term in the equation has both a positive component, $(\beta\gamma)$, and a negative component, (-1) . We can imagine a situation in which the first term in the equation will be negative and the second positive. Moreover, we can imagine a case in which the two terms are equal except for their signs and hence sum to zero. It is plausible, then, that in the case where achievement orientation is the whole link between father's and son's statuses, the covariance and hence the correlation between son's status and achievement orientation may be zero. The proper substantive conclusion would be that achievement orientation is the single crucial key to the relationship between the statuses of father and son. Our substantive conclusion would be that achievement is unrelated to mobility. Simply by measuring mobility as a difference between father's and son's statuses, we are likely to come to substantively wrong conclusions.

The proper way to ask the substantive question is to estimate the parameter γ or some other measure of association between son's status and achievement orientation with father's status held constant. Blau and Duncan (1967) asked the question this way and in doing so made a major intellectual departure from traditional methods of inquiry in the area.

Some Examples

These conclusions are more than esoteric consequences of using equations to represent the models. Some simple examples show that the logical problems are inherent in any mode of analysis. I have invented two which simply dichotomize father's and son's statuses and achievement orientation. In these examples, mobility has been defined as upward, stable or downward according to whether son's status is higher than, equal to or lower than father's status.

This measure is logically similar to subtracting continuous status variables. The

EXAMPLE I.

A. Hypothetical Example of Son's Status by Father's Status and Achievement and Resulting Types of Mobility.*

<u>Father's Status</u>	<u>High</u>		<u>Low</u>	
	<u>Yes</u>	<u>No</u>	<u>Yes</u>	<u>No</u>
<u>Achievement Orientation</u>				
<u>Son's Status</u>				
High	56 (stable)	24 (stable)	6 (upward)	14 (upward)
Low	14 (down)	6 (down)	24 (stable)	56 (stable)
Total	70	30	30	70
Percent High on Son's Status	80%	80%	20%	20%

B. Type of Mobility by Achievement Orientation Resulting from the Relationships Above.*

<u>Achievement Orientation</u>	<u>Yes</u>	<u>No</u>
<u>Mobility</u>		
Upward	6	14
Stable	80	80
Down	14	6
Total	100	100

* Numbers in the tables are raw frequencies except where percents are indicated.

mobility classification is noted for each cell of each table in the examples.

Example I is constructed by assuming that achievement orientation is unrelated to son's status, but related to father's status. That is, in causal terms, father's status is a cause of son's status and of achievement orientation, but there is no direct causal link between achievement orientation and son's status.

The pattern of the relationship may be seen by examining the percentages on the dependent variable. Among persons whose father's status is high, 80 percent of sons have high status whatever their level of achievement orientation. Among persons whose father's status is low, 20 percent of sons have high status whatever their level of achievement orientation. On the other

hand the strong relationship between father's and son's statuses is shown by the differences between 80 percent and 20 percent in each case when achievement orientation is held constant.

The relationship between mobility and achievement orientation which results from these assumptions is shown in panel B of Example I. Achievement oriented sons tend to be more downwardly mobile than those who are not. Among the one hundred achievement oriented people, six are upwardly mobile and fourteen are downwardly mobile; while among those not achievement oriented, the situation is reversed. Hence achievement orientation and mobility are negatively related. This same result is generated by the mathematical model above; *if there is no causal relationship between*

achievement orientation and status attainment, the observed relationship between achievement orientation and mobility may be negative.

Example II illustrates the other point made above. In it, achievement orientation accounts entirely for the relationship between father's and son's statuses. That is, the relationship between father's and son's statuses disappears when achievement orientation is held constant. Where achievement orientation is classified *yes*, 80 percent of sons have high status whatever the father's status; and where achievement orientation is classified *no*, 20 percent of sons are high in status whatever their father's status.

Again, if we classify the eight cells into

mobility types and view the relationship between mobility and achievement orientation, we find, in panel B of Example II, identical distributions of mobility types for the two classifications of achievement orientation. Thus, we may imagine a case where we should substantively conclude that achievement orientation is the sole and crucial link between statuses from one generation to the next and where we actually conclude that achievement orientation and mobility are unrelated. The last conclusion while technically correct, is an artifact of our way of conceiving and measuring mobility. This example corresponds to the second problem discussed with the mathematical model above; *if achievement orientation is the sole causal link between sta-*

EXAMPLE II.

A. Hypothetical Example of Son's Status by Father's Status and Achievement and Resulting Types of Mobility.*

<u>Father's Status</u>	<u>High</u>		<u>Low</u>	
	<u>Yes</u>	<u>No</u>	<u>Yes</u>	<u>No</u>
<u>Achievement Orientation</u>				
<u>Son's Status</u>				
High	64 (stable)	4 (stable)	16 (upward)	16 (upward)
Low	16 (down)	16 (down)	4 (stable)	64 (stable)
Total	80	20	20	80
Percent High on Son's Status	80%	20%	80%	20%

B. Type of Mobility by Achievement Orientation Resulting from the Relationships Above.*

<u>Achievement Orientation</u>	<u>Yes</u>	<u>No</u>
<u>Mobility</u>		
Upward	16	16
Stable	68	68
Down	16	16
Total	100	100

* Numbers in the tables are raw frequencies except where percents are indicated.

tuses, the observed relationship between achievement orientation and mobility may be zero.

Again, conceiving mobility as a difference of statuses rather than viewing the causal links between statuses may lead to egregiously wrong substantive conclusions.

Conclusions and Implications

The methodological difficulties presented here make apparent that substantive problems in social mobility should not be conceived as requiring an explanation of status differences. The problem should be to assess the antecedents of present status, which will include earlier status. Blau and Duncan took this approach and should be emulated in future research.

Though the present discussion has focused on the problem of explaining mobility, similar methodological problems will emerge in studies of the consequences of mobility. Here too, it would be wise to use the component mobility statuses explicitly in explanatory models rather than simply using the difference between them.

In general, we should be warned that investigating the causes or effects of differences of variables is likely to divert us from the truth.

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MEASURING INDIVIDUAL MODERNITY: A NEAR MYTH*

MICHAEL ARMER
Indiana University

ALLAN SCHNAIBERG
Northwestern University

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A variety of scales of individual modernity have been developed to measure a universal set of attitudes and behaviors which presumably better fit men for life in modern society. The present study attempts to assess empirically the equivalence, reliability and validity of four such scales constructed by Smith and Inkeles, Kahl, Schnaiberg, and Armer. Data were collected from multiple-wave interviews of a sample of lower-to-middle class, ethnically heterogeneous, married males in the Uptown area of Chicago.

The results indicate that the scales (a) are moderately equivalent (intercorrelations range from .40 to .64) despite differences in dimensions, items, and scale construction procedures, (b) have moderately high internal consistency ($r_{\text{alpha}} = .56$ to .76) and test-retest ($r_{11} = .66$ to .81) reliabilities, and (c) have low discriminant validity with respect to anomia, alienation, and to a less extent, socioeconomic status. Low construct validity persists when corrections or tests are made for attenuation due to unreliability, acquiescent response, and other possible interpretations.

In short, the modernity scales tend to predict scores on anomia, alienation, and socioeconomic status about as well as they predict other measures of modernity. Conversely, measures of anomia and alienation appear to predict modernity scores almost as well as do the modernity scales. The findings call into serious question the meaningfulness of the construct and/or measurement of modernity.

A MAJOR focus of comparative social psychological research has been to identify and measure "individual modernity," i.e., the set of individual values and beliefs which theoretically are associated with modernization and which better fit a man for life in modern society (Inkeles, 1966). Underlying most research in this area are premises that a single dimension of modernism exists along which societies can be distributed, that societies at each point along this continuum tend to share certain social and cultural patterns, and that these socio-cultural patterns lead to, result from, or occur concomitantly with patterns in values, beliefs and behaviors of individuals. It follows that there exists a single dimension of modernity along which individuals in all societies can be classified and that, in societies at higher levels of modernity, the average level of individual modernity will be higher. Also, individuals in more modern sectors or environments *within* societies will

tend to be more modern than those in less modern sectors.

This statement is too generalized to capture the subtleties and qualifications of specific discussions on individual modernity, but it does make explicit the theoretical rationale which underlies much of the research. It should be noted that, contrary to Stephenson's (1969) criticism, this rationale clearly does not preclude cultural or individual variation; it merely emphasizes the proposition that certain common perspectives and behavior will be found among individuals who experience modern socio-cultural environments, regardless of other personal or collective differences in perspectives and behavior (cf. Lambert, 1964). However, this theoretical rationale, and the term "modernization," imply a unilinear model of change with respect to the selected social-cultural and individual patterns of behavior. The notion of a unidimensional continuum of societal modernization (on which analyses of individual modernity partly rest) has been subjected to considerable criticism (Bendix, 1967; Blumer, 1964; Gusfield, 1967; Illich, 1969). In addition, there have been some highly critical evaluations of the

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concept of individual modernity (Stephen-
cf. Inkeles, 1969a, 1970).

son, 1969; Frank, 1967; Horowitz, 1970;

And yet few sociologists would argue that societal change is random; the process of industrialization appears to be associated logically and empirically with changes in other spheres of social organization and individual behavior (Moore, 1965). With respect to individual perspectives, clear cross-cultural similarities have been demonstrated for people experiencing similar sectors of society (Inkeles, 1960; Smith and Inkeles, 1966; Kahl, 1968; Doob, 1960). In short, while acknowledging the plausibility of many theoretical critiques regarding the unidimensionality of societal and individual modernization, we are struck by what appears to be extensive empirical support for the existence of individual modernity under similar social-cultural conditions. Research shows remarkably strong and consistent relationships of individual modernity to formal education, industrial experience, and urban residence (Inkeles, 1969b; Kahl, 1968; Armer and Youtz, 1971; Schnaiberg, 1971). However, there exist virtually no direct empirical evaluations of the generalizations in most of these modernity studies (cf. Feldman and Hurn, 1966; Feldman and Kendrick, 1968; Jacobson and Kendrick, 1970).

What are the value-orientations which have been used to define individual modernity across cultures? Within the past five years there have been numerous attempts to answer this question at the theoretical level including essays by Peshkin and Cohen (1967), O'Connell (1965), and Inkeles (1966), and several studies have proposed multiple-item measures of modernity (Smith and Inkeles, 1966; Doob, 1967; Dawson, 1967; Kahl, 1968; Schnaiberg, 1970a; Armer, 1970; Armer and Youtz, 1971). Included in these measures have been components such as independence from family ties, a sense of personal efficacy, an empirical orientation toward causality, and receptivity to change.

Several features shared by these scales of individual modernity raise questions about their measurement value in comparative empirical research. First, all have been developed and tested primarily in "third world" nations. We lack information on

whether they provide meaningful measurement among individuals in modern, urban-industrial societies or, for that matter, in small, undeveloped primitive societies. The limitations of "pseudo cross-national" designs (Hyman, 1964) are just as applicable in constructing cross-national instruments as in testing cross-national hypotheses. Evidence is needed on the utility of these scales in discriminating modernity levels among individuals in societies with substantially higher and lower levels of socio-economic development than is represented in Brazil, Mexico, Israel, Nigeria, Pakistan, Chile, Argentina, India and Turkey. (Frey, 1970:202, even suggests that most of these nations are over-sampled and unrepresentative of "third world" nations.)

Second, although all these scales are intended to measure the same or very similar phenomena, they reveal *prima facie* differences in content. This is not surprising in view of the vast array of values, beliefs and behaviors which may be subsumed by the construct of individual modernity. The studies differ to some degree in their definitions of the domain of content as well as in the techniques used to sample and measure items included in scales of modernity. Although no exact procedures can be specified for domain sampling (Bohrnstedt, 1970; Robinson *et al.*, 1958), the resulting item differences raise the question of whether the scales are truly equivalent. To what extent do scales of individual modernity measure a common construct rather than different "modernities" for each scale?

Third, little or no comparable information is available on the measurement properties of these scales, especially information pertaining to retest reliability and construct validity (Campbell, 1960). This lack is particularly critical since some scale components and items approximate the components and items found in measures of anomia, alienation, middle-class values, and other constructs already in use. Although correlations of modernity with these variables may be expected on theoretical grounds, the correlations should be lower than correlations between separate measures of the same construct if it is indeed a distinct construct. Therefore, evidence is needed of the discriminant validity (Campbell and Fiske;

1959)¹ of modernity scales from scales of other well-known presumably distinct, constructs. Moreover, the modernity scales may be subject to various response biases, especially acquiescence response bias. In short, what evidence is there that modernity scales measure a distinctive trait rather than one of the more common traits or measurement factors with which they might be confounded?

These basic questions require investigation if we are to evaluate the meaningfulness and utility of modernity scales. Specifically, evidence is needed (a) that the scales differentiate respondents in a reasonable fashion in modern (and other) societies, (b) that the scales are reasonably equivalent, and (c) that they are reasonably reliable and valid. The objective of the research reported here is to investigate these matters systematically by assessing the similarities and differences among alternative measures of individual modernity in a large urban-industrial setting in the United States. Specifically, we will examine the distributions, equivalence, internal consistency, retest reliability, and construct validity of modernity scales constructed by Smith and Inkeles (1966), Kahl (1968), Schnaiberg (1970a), and Armer (1970), and Armer and Youtz (1971). To the extent that the evidence does not meet the requirements specified above for any or all modernity scales, the utility of those scales must be called into question. If evidence is lacking for all scales, we must consider the possibility that individual modernity is a myth and not a meaningful concept.

At this point, we should comment on the significance of this study for comparative social psychological research on modernization. Existing measures of modernity are being rapidly incorporated in comparative and foreign area research. Inkeles (1969b), for example, reports that one of his measures is being used in more than twenty pure and applied projects in developing countries. But

without evidence of cross-cultural utility and of scale equivalence, it is impossible to say whether discrepancies between findings in different studies are real or an artifact of unsuspected measurement limitations or differences. Moreover, research based on unreliable or invalid indexes may actually obstruct understanding of the changes in perspectives, behavior and needs of individuals in modernizing societies. In short, information is needed on cross-cultural utility, scale equivalence, reliability, and validity of the separate scales of modernity in order to minimize misinterpretation and to maximize the value and the cumulativeness of results from past and future comparative studies.

DATA AND METHODS

The present paper is based on interview data collected in connection with a study of the relationship between modern values and family planning norms and practices which began in August, 1970, in the Uptown area of Chicago. The first wave consisted of a non-probability sample of 156 white married males and was followed by second and third waves at approximately nine-week intervals. The second wave included 109 reinterviews and twenty-two interviews with new respondents, while the third wave included ninety-eight reinterviews and twenty-four new respondents. A team of five male interviewers conducted all three waves. This paper's findings are based on completed data analysis from the first two waves.

Uptown is a low and middle income, ethnically heterogeneous residential area about five miles north of the downtown "Loop" in Chicago. It was selected as a setting for the research on both practical and theoretical grounds. Among practical considerations were resource limitations which made it unfeasible to consider most alternative locations abroad or in the United States. Furthermore, locating in the U.S. avoided major translation problems which hamper foreign language research.

From a theoretical standpoint the site has distinct advantages in that it is comparable to those included in the urban portions of Smith and Inkeles', Kahl's, Schnaiberg's, and Armer's studies. This is an important point of reference since a large majority (over

¹ There have been several recent reformulations of the Campbell and Fiske approach (Jackson, 1969; Joreskog, 1968, 1969), but these are generally variations in statistical manipulation rather than changes in the basic structure of the initial argument. The major sociological applications of the model that we are aware of are Summers *et al.* (1970), and Althaus *et al.* (1971).

75%) of the respondents in all four of the original studies lived in urban areas. Also, Uptown includes a high proportion of recent low-status migrants from rural areas, a composition pattern common to urban areas in developing societies. Finally, a substantial share of the sample is in an unstable, insecure or marginal labor force category. Their situation is thus comparable to that of persons in petty trades, traditional labor, etc. who were involved in the previous studies. Thus, although the overall composition of the sample reflected a more homogeneous and somewhat higher class distribution than previous samples, we find a high degree of equivalence between the Uptown sample and the original research samples while still allowing considerable difference in socio-cultural context. It is hoped that in the future we can offset some of the anticipated lack of variability in individual modernity in the Uptown sample by undertaking similar research in other settings.

The interview schedule included items from Smith and Inkeles' OM-6 Modernity scale, Kahl's Modernity I and II scales, Schnaiberg's Emancipation scale, and Armer's Individual Modernity scale. These scales are all based on a limited number of structured questions which are intended to be applicable across cultures. The fact that the scales are relatively easy to administer and uncomplicated in measurement increases their attractiveness to many social scientists for use in comparative research. In addition, information was obtained on Srole's (1956) anomia, Middleton's (1963) alienation (omitting one item dealing with cultural estrangement), acquiescent response bias, socioeconomic status, and personal background factors.

The modernity scales were included in all three waves and the anomia and alienation scales were included respectively in the first and second waves. Acquiescence was measured by discrepancies in mean scores on a set of eight reversed items corresponding to the eight polychotomous items in Kahl's Modernity II scale. Mean scores on each set could range from a low of 1.00 to a high of 4.00. Those respondents whose average code on the eight reversed items was discrepant from the average code on the orig-

inal items by less than 1.00 were identified as non-acquiescent. Socioeconomic status was a composite index based on a summation of dichotomized responses to three items measuring educational level, (less than four years of high school vs. four years or more), occupational prestige (blue-collar vs. white-collar), and family income (less than \$7,500 vs. \$7,500 or more). The scales of anomia, alienation, and modernity were constructed using scoring procedures and weights identical with those employed in the original sources. (Item weights for Kahl's scales are averages of those reported for his Mexican and Brazilian samples.)

RESULTS

Three major questions are involved in the analysis and comparison of the individual modernity scales. The first is whether the obtained scores from respondents living in a modern, urban-industrial society are reasonable. Do the several scales differentiate and distribute Uptown respondents with respect to individual modernity in a way that appears sensible when compared with the results for respondents in the original research in non-Western, non-industrial societies? Second, are the several scales equivalent? Do they all measure a relatively unitary construct of modernity or do they appear to be measures of different constructs? Third, what are the absolute and relative merits of the modernity scales in terms of standard measurement criteria? To what extent are the scales internally consistent, stable, and valid? In particular, is there clear differentiation of modernity from other well-known constructs or from measurement artifacts such as acquiescent response bias?

Modernity Distribution in Uptown. Item-selection, scoring, and weighting procedures varied considerably in the studies by Smith and Inkeles, Kahl, Schnaiberg, and Armer; thus a comparison of means and standard deviations in their original units of analysis would be ambiguous. In order to facilitate comparison, all four distributions have been transformed by setting the theoretical minimum modernity score equal to 0 and then

dividing by the theoretical maximum modernity score

$$(X_i = \frac{X_i - X_{\min}}{X_{\max} - X_{\min}}),$$

where X_i is the transformed score for the i^{th} case which gives a new range from 0 to 1. Thus, if an individual gave the most modern response possible to each of the fourteen items in Smith and Inkeles' scale or to each of the twenty-two items in Kahl's Modernity I scale, his modernity score would equal 1.00 for each of these scales. The means and standard deviations of the Uptown distributions on each scale are reported in Table 1.

hand, this scoring procedure makes the comparison of absolute modernity in two or more societies difficult, if not impossible, because the scoring of items is not equivalent across samples. These scales, then, have a greatly reduced value for comparative research. One can still compare "relatively modern" or "relatively traditional" individuals across societies, but levels of absolute modernity may differ greatly between these individuals in different societies. In short, this procedure tends to produce a culture-specific measure of modernity even though items and dimensions remain the same across cultures.

Table 1. Transformed Means and Standard Deviations of Selected Individual Modernity Scales

Scales	No. of Items	Uptown Sample (Wave I)		Original Source		N
		Mean	s.d.	Mean	s.d.	
Smith & Inkeles' OM-6	14	.66	.19	unreported ³		---
Kahl's Modernity I	22 ¹	.63 ²	.14	unreported		---
Schnaiberg's	24	.57	.14	.45	.23	803
Armer's	22	.61	.11	.39	.14	591
(N = 156)						

¹Kahl's twenty-two items are only those comprising the multiple-item themes which constitute "core" values in both the Mexican and Brazilian data.

²Kahl's scale has been reversed to be consistent with the other scales in which a high score means more modern.

³Smith and Inkeles (p. 362) report the mean score for their Long Form Overall Modernity (OM) scale in all six nations to be "about 1.54," which is equivalent to .54 on our transformed scale.

One major difference in scoring techniques employed in the original studies should be noted. Smith and Inkeles, and Schnaiberg dichotomized items into modern and traditional responses at the median of each response distribution while Kahl and Armer maintained fixed, largely polychotomous, response codes. Dichotomization at the median sets the mean of each item and of the total scale as close to .50 as possible given the particular response distributions. This technique has the effect of maximizing the variance of items and scales which in turn contributes to the internal consistency reliability and to the maximum possible correlation with other variables. On the other

(See Inkeles, 1969a, for a critique of culture-specific measures.)

Leaving this difficulty aside for the time being, it would seem reasonable to find differences in the degree and distribution of modernity scores in Uptown by contrast with distributions obtained in the original sources. Unfortunately information on means, standard deviations, and other distribution properties is generally absent in the original studies. However, Inkeles (1966) reports that his scale has been successfully used in the United States, and Kahl (1968:50) reports a distribution of Modernity III scores for housewives in the United States which appears to compare well

with his data from Brazil and Mexico. Thus there was preliminary indication that the modernity scales were applicable to modern industrial societies as well as to developing societies.

A more detailed comparison can be made in the case of Schnaiberg's and Armer's scales since the original data from Turkey and Nigeria are available. The Turkish data are limited to the Ankara portion which is most comparable with Uptown data; it consists of a probability area sample of 803 married women. Armer's sample consisted of 591 young men residing in Kano City which is part of the larger Kano metropolitan area of 125,000 in northern Nigeria. Individual modernity distributions for these two samples have been transformed in the same fashion as the Uptown distributions and the means and standard deviations are also reported in Table 1 in the "original source" columns.

It is clear from examination of the means of scales for the Uptown sample that all are above the scale midpoint (.50) indicating central tendencies toward the modern end of the continuum. Smith and Inkeles' distribution is furthest from the midpoint, which is surprising in view of their scoring procedure, and it also has the highest standard deviation (.19) in the Uptown data. Detailed examination of the data reveals that the high mean score is due to extremely skewed distributions on many of Smith and Inkeles' items. Skewed distributions also occurred to a smaller degree on the other three scales and help to account for the means above .50. The higher standard deviation reflects in part the smaller number of items comprising Smith and Inkeles' OM-6 compared to the other scales.

Comparison of the original Turkish and Nigerian data with the Uptown data show modernity scores closer to the traditional pole and below the scale mid-point. It is also apparent from the standard deviations that the Uptown sample is more generally homogeneous on Schnaiberg's and Armer's scales than were the original Turkish and Nigerian samples. In general, these findings are all reasonable and consistent with expectations of higher levels of modernity for comparable cross-national populations in a modern, industrial society like the United States by contrast with Turkey, Nigeria, or

other less developed nations. Furthermore, the lower variation in modernity scores for the Uptown sample when compared to the original samples is understandable in terms of (1) the more restricted range of variation in social status and other factors in Uptown compared to that in Turkey and Nigeria and (2) the general absence of extremely traditional individuals in an urban-industrial center of an industrialized society. Modernizing influences in modern society are extremely pervasive; virtually all residents of Uptown are exposed to the mass media, urban life styles, education, contact with modern individuals, and other modernizing influences (Inkeles, 1966). On the the other hand, these influences are differentially experienced and therefore result in variation in individual modernity as observed in the Uptown data.

In sum, the modernity scores behave in ways which are consistent with theoretical expectations, and they appear sufficiently differentiated to warrant further analysis of scale equivalence, reliability and validity.

Equivalence of Scales. Alternative survey research approaches to measuring modern values are well illustrated by the Smith and Inkeles, Kahl, Schnaiberg, and Armer scales. Smith and Inkeles began with a list of thirty-three themes reflected in 119 items which were reduced through a combination of item-selection procedures to a "final distillate" of fourteen items known as the OM-6 scale. This scale consists of ten dichotomized attitudinal items and four dichotomized behavioral and informational items. Kahl began with fifty-eight items tapping fourteen themes which were reduced through factor analytic techniques to a twenty-two-item scale which included subscales of activism, low integration with relatives, preference for urban life, low perceived community stratification, mass media participation, and low perceived stratification of life chances. All were attitudinal measures except for the mass media items. Armer also used a factor analytic approach and concluded with twenty-two attitudinal items measuring dimensions of mastery (efficacy and activism), tolerance of other groups, belief in empirical causality, orientation to the future, independence from family ties, and receptivity to change. Schnaiberg's scale consisted of twenty-four dichotomized items,

tapping nuclear family role structure, mass media participation, and environmental orientation which were selected by factor analysis from a pool of forty-six items. One of the items asking about wife's leisure activities was omitted as inappropriate for the present analysis.

Despite these differences in dimensions, items, and procedures, all four of the scales are intended to identify a set of attitudes, values, and ways of acting that are associated with a modern society. It is assumed that the items and subscales are intercorrelated and form a general value syndrome of modernism. "On the average, a man who is high on some will also be high on the others, although there is room for variation" (Kahl, 1968:21). None of the authors differentiates types of modernity, and all suggest that their conceptions are relevant to the central values distinguishing modern from traditional societies. Thus individuals who are identified as relatively modern on one scale should also be identified as relatively modern on the others. In short, to the extent that the scales are reliable and valid, they should intercorrelate highly even if they emphasize different components of the general value system of modernity.

Assessment of the degree of equivalence in scale scores can be made from scale intercorrelations reported in Table 2. Values in the diagonal are internal consistency reliability coefficients (Cronbach's coefficient Alpha), and values below the diagonal are correlations corrected for attenuation due to less than perfect reliability. Intercorrelations above the diagonal range from $r = .40$ to $.64$ and thus represent an average explained variance among the scales of approximately 25%. None of the scales correlate perfectly or even near perfectly with the others, although all are associated positively at moderate levels. In short, the scales are interrelated with each other but are not equivalent indicators of individual modernity. While each scale measures a portion of some common phenomenon, each also includes measurements on parts of reality which are not measured by other scales.

The extent to which this lack of equivalence is due to random fluctuations in the measurement operation (i.e., measurement error) rather than to systematic differences in what is measured can be estimated by correcting the correlations for attenuation. Of course, this "correction" does not negate the fact that measurement error interferes with

Table 2. Intercorrelations of Modernity Scales (with Scale Reliabilities¹ in Parentheses and Corrections for Attenuation² Below the Diagonal)

Modernity Scales (Uptown, Wave I)	Smith & Inkeles' OM-6	Kahl's Modernity I	Schnai- berg's	Armer's	Average Correlation	
					Uncorrected	Corrected ³
Smith & Inkeles' OM-6	(.64) ¹	.54	.45	.57	.52	.79
Kahl's Modernity I	.77 ²	(.76)	.40	.64	.53	.77
Schnaiberg's	.64	.57	(.64)	.41	.42	.63
Armer's	.95	.97	.68	(.56)	.54	.87

¹Cronbach's (1951) Alpha = $[n/(n-1)][1 - (\sum \sigma_i^2 / \sigma_x^2)]$. This is a generalization of Kuder and Richardson's KR20 internal consistency reliability formula. Because each scale is seen as a general syndrome of modernity comprised of subscales or items which are correlated but substantively independent of each other (as with most scales of socioeconomic status, intelligence, etc.), the Alpha coefficient of reliability may be interpreted as a lower bound estimate (Bohrnstedt, 1970).

²Correlations below the diagonal are corrected for attenuation due to unreliability. $r_{T_x T_y} = r_{xy} / \sqrt{r_{xx} r_{yy}}$, where r_{xy} is the uncorrected sample correlation, and r_{xx} is the reliability of x.

³Corrected for attenuation due to unreliability.

accurate estimation of individual modernity, but it facilitates evaluation of whether the "true" scores are reasonably equivalent, even if observed scores are not.

The corrected correlations below the diagonal in Table 2 are substantially higher than the uncorrected correlations above the diagonal, especially the intercorrelations of Armer's, Kahl's, and Smith and Inkeles' scales. The corrected intercorrelations of these three scales range from .77 to .97, an indication that about three-fourths of the variance can be accounted for. This is far from perfect equivalence but suggests a high degree of covariation among "true" scores.

Reliability of the scales. Having determined that the scales are measuring largely the same phenomenon and that the scores appear consistent with expectations of individual modernity, we now proceed with an estimate of whether scale reliabilities and validities meet minimal standards. Also, since the preceding evidence suggests that the scales are not highly equivalent, it becomes more important to determine which scales have the highest reliability and validity. It should be noted that these questions regarding the absolute and relative merits of scales can only be answered with respect to particular populations, i.e., the reliability of a scale refers to reliability for a given sample and may vary for other samples.

Two major types of measurement reliability can be estimated in this study: internal consistency and test-retest reliability. Internal consistency reliability is an estimate of the extent to which a scale is free from random variation among items, or "measurement error." The assumption is made that a scale is intended to be homogeneous or unidimensional and thus items should tend to covary. To the extent that the scale produces a high ratio of random variation (instead of covariation) to total variation, reliability estimates are lowered.

For the modernity scales, it follows that reliability is decreased in those scales which were constructed by intentionally selecting items which best measure the separate components of modernity rather than those which best measure the central tendency or general concept. For example, Kahl and Armer selected items to measure a multi-dimensional conception of modernity con-

sisting of several components; this selection tends to reduce correlations between items in different scales which in turn reduces reliability. This effect is offset to some extent by their selection of sub-scales which cohere together. In the case of Schnaiberg's scale, no item-analysis was involved on theoretical grounds and all items and subscales were retained in his factor analyses leading to a measure of general modernity. This procedure also allows low internal consistency. In short, internal consistency reliability has limitations as an estimate of reliability for heterogeneous scales such as the modernity measures in this study. Reliability testing assumptions of nearly equal item difficulty and intercorrelation are not met, especially for scales which select items to represent the separate components rather than overall modernity.

An alternative and perhaps more appropriate estimate of reliability is test-retest correlations. However, interpretation of retest reliability coefficients is not without its own complications due to change in "true" scores, recall of previous answers, and other contaminating factors.

Internal consistency and test-retest reliabilities are reported in Table 3 for each of the modernity scales. No attempt has been made to standardize coefficients for the effects of different test length and scale vari-

Table 3. Internal Consistency and Test-Retest Reliability of Modernity Scales in Uptown Sample.

Scales	Cronbach's Alpha		Test-Retest
	Wave I	Wave II	
Smith & Inkeles' OM-6	.64	.54	.81
Kahl's Modernity I	.76	.80	.80
Schnaiberg's Modernity	.64	.55	.74
Armer's Modernity	.56	.57	.66
Scrole's Anomia	.67	--	--
Middleton's Alienation	--	.59	--
Socioeconomic Status	.54	.56	.87
N =	(156)	(109)	(109)

ance. The internal consistency reliability coefficients for wave I range from .56 to .76, and from .55 to .80 in wave II. These are not exceptionally high but they are comparable to reliabilities computed with the Uptown data for Srole's well-known anomia scale (.67) and for Middleton's alienation scale (.59).

Comparison of wave I with wave II reliability coefficients reveal an interesting anomaly. Reliability coefficients for Kahl's and Armer's scales remain constant or become slightly higher while the coefficients for Smith and Inkeles' and Schnaiberg's scales drop substantially. This irregular finding illustrates the variability across samples of reliability estimates for a given measure. For some reason, the 109 people who were reinterviewed in wave II appear to be less internally consistent in their responses to Smith and Inkeles' and Schnaiberg's items than the forty-seven people who were not reinterviewed, or perhaps they became less consistent between wave I and wave II. But why did it affect these two scales and not all four?

One possible interpretation is that both the Smith and Inkeles and Schnaiberg scales were based on items dichotomized on the basis of the wave I distributions: because of selective sample attrition of more traditional respondents between wave I and wave II, the resulting scales suffered a greater reduction in "true score" variance than was true of the two remaining polychotomous scales (Kahl and Armer). This truncation of variation served to reduce the correlations between items in these dichotomous scales and thus reduced the internal consistency reliability measures. An examination of the decline in normalized standard deviations from wave I to wave II for the four scales confirms this: the wave II normalized standard deviations declined by 16% of the wave I standard deviations for Smith and Inkeles' scale and by 12% for Schnaiberg's scale, by contrast with only 4% for Armer's scale and an increase of 1% for Kahl's scale.

The test-retest reliability coefficient reported in Table 3 for the four modernity scales ranges from .66 to .81 and indicates the consistency of scores across administration waves. The correlations are highest for Smith and Inkeles' and Kahl's scales, which

may reflect in part the inclusion in these scales of some behavioral and information items which are more objective and less likely to vary across waves. Consistent with this explanation is the high retest reliability of the socioeconomic status measure (.87) which is comprised of three objective indicators (annual income, occupational class, and education level).

In any event, a comparison of modernity scales on reliability estimates indicates that Kahl's scale is generally superior to the others in our Uptown sample and that Armer's scale tends to have the lowest reliabilities. All of these scales meet minimum conventional standards with the possible exception of Armer's scale on wave I and all but Kahl's scale on wave II.

Validity of the Scales. There are no independent, error-free criteria of individual modernity against which to validate the modernity scales in this study, but we may investigate their convergent and discriminant validity. "When a construct is proposed," according to Campbell and Fiske (1959:84), "the proponent invariably has in mind distinctions between the new dimension and other constructs already in use. One cannot define without implying distinctions, and the verification of these distinctions is an important part of the validation process." Campbell and Fiske point out that validation requires (1) evidence of relative convergence of scale scores among tests designed to measure the same construct (convergent validation), and (2) evidence of relative independence or lower correlations with tests designed to measure other constructs (discriminant validation). It is important to emphasize that validation requires not only that scales purporting to measure a construct be positively correlated with each other but that they be correlated *more highly* with each other than they are with scales purporting to measure distinct constructs already in use.

Several of the more plausible constructs with which modernity may be confused are anomia, alienation, and middle-class values. Anomia as measured by Srole's scale involves a general orientation of despair which seems to characterize negatively some of the modernity items, especially in Kahl's scale. Alienation, involving components of power-

lessness, normlessness, isolation, and meaninglessness, seems to be inversely reflected in modernity items dealing with personal efficacy, individualism, non-empirical causality, stratification of life chances, and so forth, although the similarity is unrecognized or unacknowledged by the modernity scale authors. Finally, the possible overlap between middle-class values and modern values leads us to include a measure of socioeconomic status in the schedule. This enables us to explore the possibility that the term "modern values" might simply be an alternative label for middle-class values to which most Western social scientists subscribe. In sum, the question is: Are modernity scales indistinguishable from measures of the well-established constructs of anomia, alienation and socioeconomic status, or are they valid measures of a distinct construct.

Intercorrelations of modernity scales with measures of anomia, alienation, and socioeconomic status are reported in Table 4 along with the average correlation of each modernity scale with the remaining three modernity scales. The anomia correlations are based on wave I data and the alienation correlations on wave II, while socioeconomic status correlations are reported for both wave I and II. Evidence of the modernity scales' construct validity is provided to the extent that average convergent correlations between modernity scales are high relative to the discriminant correlations with other scales.

Comparison of the convergent coefficients with the discriminant coefficients for anomia and socioeconomic status in wave I shows higher discriminant than convergent coefficients for Kahl's and Schnaiberg's scales

with anomia ($-.60$ and $-.43$) and almost as high discriminant as convergent coefficients for Armer's scale with anomia ($-.52$), and for Smith and Inkeles' scale with SES ($.48$). In wave II, all of the modernity score correlations with alienation were essentially equal to or higher than the convergence of modernity scales, and Schnaiberg's scale also had a relatively high correlation with SES.

In short, the evidence suggests rather low discriminant validity in both waves for the four modernity scales, especially with respect to scales of anomia and alienation. In other words, the modernity scales tend to predict scores on anomia, alienation, and, to a lesser extent, socioeconomic status about as well as they predict the other scales of modernity. Conversely, measures of anomia and alienation appear to predict modernity scores almost as well as do other modernity scales. These findings cast serious doubt on the discriminant validity of modernity scales.

INTERPRETATIONS

Before drawing conclusions about the conceptual or measurement inadequacies of the modernity scales, several alternative interpretations should be considered. Among these are two methodological interpretations involving the possibility of (a) differential attenuation and (b) acquiescent response effects, and two theoretical interpretations involving conceptions of anomia and alienation (c) as components of modernity or (d) as natural concomitants of minority or deviant value-orientations (traditionalism).

Differential attenuation interpretation. The first alternative interpretation is that correlations of modernity scales with anomia,

Table 4. Convergent and Discriminant Validity Coefficients of Modernity Scales in Uptown Sample.

Modernity Scales	Wave I			Wave II		
	Convergence	Discriminance		Convergence	Discriminance	
	Average r	Anomia	SES	Average r	Alien.	SES
	$\bar{r}$	r	r	$\bar{r}$	r	r
Smith & Inkeles'	.52	-.44	.48	.45	-.49	.38
Kahl's	.53	-.60	.34	.47	-.62	.31
Schnaiberg's	.42	-.43	.36	.36	-.44	.35
Armer's	.54	-.52	.39	.40	-.39	.30
N =	(156)	(156)	(156)	(109)	(109)	(109)

Table 5. Convergent and Discriminant Validity Coefficients of Modernity Scales in Uptown Sample (Corrected for Attenuation).

Modernity Scales	Wave I			Wave II		
	Convergence	Discriminance		Convergence	Discriminance	
	Average r	Anomia	SES	Average r	Alien.	SES
	$\bar{r}$	r	r	$\bar{r}$	r	r
Smith & Inkeles'	.79	-.67	.81	.78	-.86	.69
Kahl's	.77	-.85	.53	.71	-.90	.46
Schnaiberg's	.63	-.65	.61	.62	-.77	.63
Armer's	.87	-.84	.71	.66	-.67	.53
N =	(156)	(156)	(156)	(109)	(109)	(109)

alienation, or SES that are equal to or higher than modernity scale intercorrelations may be a statistical artifact of the higher measurement reliability of these "extraneous" variables. Since the maximum possible observed correlations between any two measures (r_{xy}) is equal to their "true" correlation multiplied by the square root of the product of their reliabilities (i.e., $r_{xy} = r_{T_x T_y} \sqrt{r_{xx} r_{yy}}$, where the "true" correlation is $r_{T_x T_y}$), the lower reliabilities of modernity scales may attenuate their "true" intercorrelations more than their correlations with anomia and alienation. According to this interpretation, correcting for attenuation due to unreliability of the measures should reveal higher "true" correlations between modernity scales than between modernity and other scales.

The corrected correlations are reported in Table 5. Contrary to this possible interpretation, we find essentially the same pattern of correlations as was observed in the uncorrected data in Table 4. The changes which do occur tend to indicate even less discriminant validity than in the uncorrected data. For example, Smith and Inkeles' and Schnaiberg's scales show less discrimination from SES, and all of the modernity scales show less discrimination from alienation, than was observed in the uncorrected correlations. In short, the differential attenuation interpretation is not supported by the available evidence.

Acquiescent response interpretation. The fact that anomia and alienation show such high correlations with modernity, especially with Kahl's modernity scale, suggests an acquiescent response interpretation. Each of the scales, and especially Kahl's, included

items with agree-disagree, yes-no, etc. response alternatives, and it is well known that such items are particularly susceptible to acquiescent response bias. In addition, the direction in which the items are worded is nearly constant in Kahl's scale and in the anomia and alienation scales. Thus, the relatively high discriminant correlations could result in part from an acquiescent response effect. The need to test for response set effects in the validation of scales has been frequently emphasized (e.g., Campbell, 1960).

In order to test this interpretation, a measure of acquiescent response pattern was constructed on the basis of discrepancies between responses to forward and reversed statements of the eight items in Kahl's Modernity II scale. These respondents identified as having acquiescent response patterns were eliminated, and the correlations between modernity scales and the other constructs were subsequently re-examined for the subsample of non-acquiescent respondents (See Table 6).

The evidence in Table 6 indicates that the pattern and approximate magnitude of correlations for the total sample persists for the non-acquiescent subsample. The major change is a decrease between .04 to .09 points in the convergent correlations of wave II modernity scales. This decrease has the effect of increasing the evidence of low construct validity for wave II data. The results clearly do not support an explanation of the lack of discrimination between scales of modernity and other constructs by attributing it to acquiescent response patterns.

Components of modernity interpretation.

Table 6. Convergent and Discriminant Validity Coefficients of Modernity Scales in Uptown Sample (Non-Acquiescent Sub-Sample).

Modernity Scales	Wave I			Wave II		
	Convergence	Discriminance	SES	Convergence	Discriminance	SES
	Average r	Anomia		Average r	Alien.	
	$\bar{r}$	r	r	$\bar{r}$	r	r
Smith & Inkeles'	.51	-.44	.48	.40	-.49	.32
Kahl's	.51	-.60	.33	.43	-.63	.31
Schnaiberg's	.40	-.40	.36	.28	-.41	.30
Armer's	.51	-.47	.39	.31	-.32	.25
N =	(141)	(141)	(141)	(101)	(101)	(101)

A third possible interpretation of the relatively high correlations of modernity with anomia, alienation, and SES is that these constructs should rightfully be considered as additional components of the general construct of modernity. Modernity is perhaps a broader construct than was conceived in earlier research and should include these constructs as an integral part.

If this interpretation is viable, we would expect these new components to behave like the modernity components explicitly recognized in the literature, i.e., to correlate with general modernity at about the same level as do other components. In the Table 7,

Kahl's Modernity I scale is correlated (a) with each of Kahl's component subscales and (b) with the anomia and alienation scales. The results show that Kahl's subscale-to-scale corrected correlations (.20 to .65) are generally lower than the correlations of anomia and alienation with the total scale (-.60 and -.62 respectively). Thus anomia and alienation tend to correlate *higher* with general modernity than do the subscales of modernity themselves.

Also Table 7 reports the modernity subscale correlations with anomia, alienation, and socioeconomic status. Surprisingly, the modernity subscales (with the exception of

Table 7. Correlation of Subscales with Kahl's Modernity I, Anomia, Alienation and Socioeconomic Status.

Subscales	Wave I			Wave II		
	Modernity I ¹	Anomia	SES	Modernity I ¹	Alienation	SES
Mass-media Participation	.20	-.25	.21	.28	-.41	.22
Activism	.46	-.51	.24	.55	-.50	.18
Stratification of Life Chances	.46	-.50	.35	.56	-.45	.30
Community Stratification	.53	-.41	.16	.65	-.45	.25
Urban Preference	.48	-.26	.22	.58	-.34	.28
Independence from Family	.30	-.22	.00	.28	-.30	.04
Anomia	-.60					
Alienation				-.62		
N =	(156)	(156)	(156)	(109)	(109)	(109)

¹All modernity subscale-to-scale correlations have been corrected for part-whole effects.

"community stratification" and "urban preference") tend to correlate as highly with the constructs of anomia ($-.22$ to $-.51$) and alienation ($-.30$ to $-.50$) as they do with the construct of general modernity.

The results are repeated in the case of Armer's scales and subscales (wave I subscale-to-scale r 's = $.05$ to $.36$, wave II subscale-to-scale r 's = $.10$ to $.44$, subscale-to-anomia r 's = $-.11$ to $-.55$, and subscale-to-alienation r 's = $-.09$ to $-.38$). The subscale-to-scale correlation analysis is not repeated for Smith and Inkeles' scale or for Schnaiberg's scale because item-selection on the basis of subscale measurement was not employed in constructing these scales. However, it is possible to examine Smith and Inkeles' average *item*-to-scale correlations (corrected for part-whole effects) for wave I ($.28$) and for wave II ($.22$) compared to average correlations of their modernity items to the anomia scale ($-.22$) and to the alienation scale ($-.23$). For Schnaiberg's scale the average *item*-to-scale correlation (corrected) is $.22$ for both wave I and II compared to average correlations of his modernity items to the anomia scale of $-.18$ and to the alienation scale of $-.17$.

Clearly anomia and alienation do not "behave" like other modernity components and some of the components and items of the modernity scales appear more closely associated with anomia or alienation than with modernity. The evidence does not support the argument that low discriminant validity results from the fact that anomia and alienation are components of modernity.

Traditionalism as deviance interpretation.

A fourth possible interpretation is very similar to that made by Harrington (1962) in *The Other America*: to be traditional (poor) in a society where most people are traditional (poor) is very different than being traditional (poor) in a society where most people are modern (rich). In terms of our study, it could be argued that the relatively high correlations of alienation and anomia with traditionalism are a function of the fact that the "traditional man" in contemporary America is a social deviant and is therefore more likely to be both alienated and anomic. This interpretation focuses on the distribution of modernity (and related social and economic characteristics) within the society

and might be termed the "compositional effect of societal complexity," or the "distributional effect." It presupposes that we would *not* find such high correlations with anomia and alienation in the original studies, or if we did, they would have occurred because there were strong enough modernizing trends and aspirations in these transitional societies that a high proportion of the traditional population was already anomic and alienated (see, for example, Kardiner's discussion, 1967:184; and Armer's, 1970 interpretation of psychological malaise among less educated Nigerian youth).

Unfortunately this problem is often defined away in modernity studies through devices like the use of variable cutting-points (for dichotomization of items into "modern" and "traditional" poles) from one society or sample to another (Smith and Inkeles, 1966). Or the issue is simply ignored or side-stepped by failure to examine the consequences of drawing non-probability and very unrepresentative samples (*vis-a-vis* the total population of the society), consequences which are both substantive *and* statistical (Schnaiberg, 1968:Ch. IV, 1970b). Research designs themselves may be restricting the field of interpretation in that all studies have consistently undersampled rural and traditional populations and thus failed to consider that a high proportion of the *society* studied is traditional (using the same cutting points as for the sample).

The principal limitation of this interpretation for our present purposes is that it doesn't adequately explain why the several modernity scales tend to correlate *higher* with alienation and anomia than with each other, i.e., why they show low discriminant validity. Even if we accept the argument that being traditional in a society where most people are modern contributes to individual anomia and alienation, the correlation between separate measures of the *same* general construct (modernity) should be higher than their correlations with scales of other presumably distinct constructs (anomia and alienation). This situation is obviously not the case in the present data, and the findings cannot be accounted for solely in terms of compositional or distributional effects, although replication is needed in other settings.

DISCUSSION AND CONCLUSIONS

The evidence and interpretation presented above pose serious problems for those engaged in research on individual modernity. What are we to conclude? Is individual modernity a meaningful concept or a myth? In this connection, it is worth remembering that all of the scales have undergone extensive pretesting, refinement, and research use. They are among the least complicated, most widely applicable scales of individual modernity proposed in the literature. Why should a set of measurements emerging from such careful and systematic statistical and theoretical screening raise the spectre of invalidity?

Some may protest that the *real* myth is in the adequacy of the present analysis. It may be argued that the present findings are artificial—a product of (a) our research site in an urban-industrial society, or (b) our measures and techniques of analysis. Let us consider these matters.

As to the site of the research, both Smith and Inkeles (1966) and Kahl (1968) indicate that their measures would be appropriate for use in industrial societies, and Kahl has so used his measure (Kahl, 1968:50). Indeed, the distributions for our replicated measures in Uptown are fairly similar to those in the original studies; we do have somewhat more "modern" response patterns overall, as would be expected in a Western, urban-industrial area, but the gradation of scores is quite comparable to the original studies. What we clearly do *not* find is that "individual modernity," whatever it may be, is a quality that is "required of the citizens of modern societies" (Inkeles, 1966:150).

With respect to our measures of modernity, we have attempted to replicate the original modernity scales as closely as possible, and have omitted only items which did not appear viable across cultures. For example, those items which failed to appear significant in *both* Kahl's Mexican and Brazilian studies were dropped, as were some of the culturally specific "production-consumption" and religiosity items of Schnaiberg's scale. Furthermore, we have used the same scale construction techniques as in the original research, even to the point of replicating factor loadings or weights in appropriate scales. In addition to these efforts toward faithful

replication, we have considered extraneous but contaminating factors such as acquiescence bias, something which few of the original studies attempted to examine or control.

Thus, we feel confident that the measures and techniques are almost as close a replication of the original methods as, for example, each of the six studies of Smith and Inkeles' project represent vis-a-vis each other, or, alternatively, as close as Kahl's Brazil and Mexico studies. To the best of our knowledge, then, while acknowledging the incompleteness of our data and preliminary analysis, we do not find the "myth" of modernity in our research setting or replication methods.

Where does the problem lie, then? What is "modernity"—a construct with inadequate empirical delineation, a social psychological chimera, part of a broad, important but, as yet, ill-defined set of orientations, or what? It is important to repeat that this paper is primarily concerned with the adequacy of scales as measures of the modernity construct, not with the adequacy of the construct of modernity as determined by the confirmation of theoretically predicted correlations with measures of other constructs. Discriminant validity requires that "measures of the same trait should correlate higher with each other than they do with measures of different traits" (Campbell and Fiske, 1959:104). Clearly, all four modernity scales in their present form fail to provide statistically valid² measures of individual modernity among Uptown residents, and

² A recent fundamental critique of the Campbell and Fiske model (Althausser and Heberlein, 1970; Althausser *et al.*, 1971) poses some troublesome questions about interpretation of the discriminant and convergent validity coefficients. The central issue is that of correlated error, or common "methods effects" across scales that are being evaluated. In terms of our findings, however, there appears to be less ambiguity, since the critique appears most applicable to cases where the measures *do* indeed show construct validity, i.e., Althausser, *et al.*, are more concerned that the initial model *overestimates* construct validity. Furthermore, in testing for the effects of acquiescence, as we do, we have in fact incorporated some of the recent critique, treating acquiescence as just such a common "methods factor," and separating out its effects from the evaluation of the scale validities. Interestingly enough, our results are in the direction indicated by the Althausser and Heberlein critique, since the modernity scales' lack of construct validity is even more apparent when we analyze the non-acquiescent subsample.

hence the universal value of the scales is questionable. It may be possible to improve the scales through various operations, but this possibility does not change the fact that the scales are not demonstrably valid measures in the form in which they have been proposed. At best, the *concept* of individual modernity may be meaningful as a distinct variable, but the *measurement* of modernity has apparently been unsuccessful. At worst, the notion of individual modernity as a distinct set of orientations may be a myth; at least for some populations. It should be noted that the findings do not necessarily indicate that modernity scales are universally invalid, just as the previous studies do not show them to be universally valid. Clearly what is needed is further evaluation of the modernity scales in other settings in order to specify and refine them in so far as possible, which is the goal of validity assessment.

Thus, we are left with the final conclusion in doubt: either "modernity" *does* have some distinct empirical reality, and our present measures have *not* adequately captured this; or, there is *no* distinctive universal concept of "modernity" that exists, save in the minds of social scientists. Regardless of which position one takes, the notion that social science has been able to develop a universally valid measure of modernity appears to be false.

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SOCIAL PARTICIPATION AND VOTING TURNOUT: A MULTIVARIATE ANALYSIS *

MARVIN E. OLSEN

Indiana University and Uppsala University

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As an explanation of voting turnout, the social participation theory argues that involvement by individuals in nonpolitical social organizations such as voluntary associations, community affairs, and churches will in turn mobilize them to become politically active. Survey data from Indianapolis show marked correlations between all the above forms of social participation and voting turnout in three recent elections. The partial correlations for each of these measures, controlling the others, remain significant, indicating that each form of participation has independent effects on voting. This is not true with informal interaction among friends and neighbors, however. Evidence for inferring a causal linkage from social participation to voting turnout is found in the fact that most respondents belonged to voluntary associations prior to these elections. Finally, the relationship between social participation and voting remains moderately strong after the compounding variables of age, education, political contacts through the mass media and political parties, and political orientations such as political interest and party identification are all held constant. The mean multiple R with all predictor variables is .58.

POLITICAL democracy assumes that citizens will exercise their franchise on election day. Yet millions in the United States regularly fail to vote. Presidential elections typically attract only about 60 percent of the voting-age population (the estimated figure for the 1968 Presidential election was 62 percent); off-year Congressional elections generally draw less than 50 percent (the estimated figure for 1966 was 46 percent); and separate state and local elections usually have even lower turnouts. Why so many people fail to vote is a critical problem for democratic political theory and for understanding political behavior.

A host of empirical studies, beginning with Merriam and Gosnell's (1924) examination of the 1923 Chicago mayoral election, have investigated relationships between voting turnout and various social and political variables, to discover what kinds of people fail to vote. This research has established that voting turnout in the United States is commonly related to such factors as sex, age, race, marital status, religious preference, education, occupational status, income, membership and participation in

voluntary associations, exposure to the mass media, political involvement of one's parents, contacts by political parties, political discussions with friends, interest in politics, strength of party preference, and feelings of political efficacy.¹

In recent years such writers as Lane (1959), Lipset (1954, 1960), and Milbrath (1965) have offered numerous theoretical explanations of the relationships between these factors and voting turnout, but none of them have subsequently been adequately tested. This paper focuses on just one of these competing theoretical explanations, which I term the "social participation" theory, but subjects it to rigorous empirical analysis. This analysis proceeds in three stages: (a) determining relationships between voting turnout and various measures of social participation, both separately and in combination; (b) exploring the theory's relevance as a causal argument; and (c)

¹ All of these relationships have been substantiated by two or more of the following studies: Alford and Lee 1968; Buchanan 1956; Campbell, Converse, Miller, and Stokes 1960; Campbell, Gurin, and Miller 1954; Connelly and Field 1944; Dahl 1961; Glaser 1958, 1960, and 1965; Glen and Grimes 1968; Hastings 1956; Janowitz and Marvick 1956; Karlsson 1958; Key 1958; Lazarsfeld, Berelson, and Gaudet 1944; Lipset 1960; Miller 1952; Olsen 1970; Orum 1966; and Ranney and Epstein 1966. For an extensive bibliography of studies dealing with all forms of political participation published through 1964, see Milbrath 1965.

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examining the relationship between social participation and voting turnout while holding constant the effects of other known voting correlates, using multivariate analysis.

SOCIAL PARTICIPATION THEORY

The theory of social participation is derived from the broader idea of social pluralism developed by such theorists as Tocqueville (1961), Truman (1951), Dahl (1956), Lipset, *et al.* (1956), Kornhauser (1959), and Nisbet (1962). As used here, however, social participation theory involves both a restriction and an extension of the traditional conception.

The restriction rests on the distinction between the "mobilization" and "mediation" versions of pluralistic theory. The mobilization version—which underlies the social participation thesis—maintains that involvement in voluntary, special-interest, *nonpolitical* associations will in time activate individuals politically. Deutsch (1961) describes mobilization as "the process in which major clusters of old social, economic, and psychological commitments are eroded or broken and people become available for new patterns of socialization and behavior." He uses this concept in the context of modernizing societies, to refer to activities which move people from traditional to "modern" ways of life, and argues that this process is required for national political development. The idea of social mobilization applies equally to the effects of voluntary association activities on political participation in "developed" societies, however, if we assume that many patterns of social activities are inimical to political participation, or at least fail to reinforce it. People caught in these traditional patterns must therefore be mobilized through involvement in new social contexts—like voluntary associations—if they are to become politically active.²

There are many possible reasons why such participation can increase individual political activity: (1) It broadens one's sphere of interests and concerns, so that public

affairs and political issues become more salient for him. (2) It brings an individual in contact with many new and diverse people, and the resulting relationships draw him into public affairs and political activity. (3) It increases one's information, trains him in social interaction and leadership skills, and provides other resources needed for effective political action.

In contrast, the mediation form of pluralistic theory—which does not underlie the social participation thesis—argues that voluntary associations must at least on occasion participate directly in the political system, influencing political leaders and decision making, as well as giving political elites a channel for contacting constituents. In this view, many associations that are normally nonpolitical can temporarily become "parapolitical" actors. This version of pluralistic theory thus sees voluntary associations as mediating between individuals and the political system, focusing on the associations' actions rather than their effects on members.

The mobilization and mediation versions of pluralistic theory are not incompatible, and both could be performed by the same association. But they are separate processes that must be kept analytically distinct. This study will focus mainly on mobilization processes, but will also examine some data relevant to the mediation thesis.

Our extension of traditional pluralistic thinking, meanwhile, includes other organizations besides voluntary associations within the process of social participation. If active involvement in voluntary associations can mobilize individuals politically, why can't participation in other organizations such as churches or communities have the same effect? The organization's nature is unimportant from this perspective, since presumably the above three mobilization processes could occur in any social organization. The crucial factor here, according to the theory, is participation in any organized social activity.

To test this argument, we shall investigate relationships between voting turnout and participation in church and community activities, as well as involvement in voluntary associations. Then, to find whether such participation must occur in relatively formal settings, we shall look at interpersonal friendships in relation to voting turnout.

² Pinard (1968) has argued that the associations and organizations comprising the "intermediate" structure of a pluralistic society can mobilize members to participate in mass social movements, but he did not apply this argument to voting turnout.

Stated as research hypotheses, the social participation thesis predicts that:

Hypothesis 1: Participation rates in voluntary associations will be positively related to voting turnout, whatever the nature of the association.

Hypothesis 2: Participation rates in community and church activities will also be positively related to voting turnout, and these relationships will remain significant when voluntary association participation is held constant.

Hypothesis 3: Participation rates in interpersonal interaction will be related to voting turnout; but holding constant participation in voluntary associations, churches, and community affairs will eliminate these correlations.

Correlations between rates of social participation and voting turnout indicate covariation between these two factors, but say nothing about causation. The argument that one variable causes another is always a logical inference based on available evidence, and cannot be directly tested. We can, however, ask what evidence exists for making a causal inference, and evaluate its relevance and adequacy. This kind of inquiry thus focuses on a logical "expectation" rather than a statistical hypothesis. In this paper we shall examine evidence for the following expectation concerning the social participation thesis: One is justified in inferring a causal sequence from social participation to voting turnout.

Finally, as was noted, voting turnout is known to correlate with a wide range of other variables in addition to social participation. Hence the social participation thesis will be valid only if correlations between voting turnout and voluntary association, community, and church participation remain significant after holding constant the effects of all other compounding variables. That analysis will involve testing a large number of research hypotheses that need not be specified here. Instead, let us summarize them in a second broad expectation: The relationship between voting turnout and participation in organized social activities will remain significant after other voting correlates have been controlled.

These additional correlates of voting turnout can be placed in four categories: (a)

demographic location, including age, sex, marital status, and religion; (b) socioeconomic status, as indicated by educational, occupational, and income levels; (c) political contacts, including parents' political activities, interpersonal political discussion, mass media news exposure, and party mailings and visits; and (d) political orientations, such as party identification, political interest, and political efficacy. Each of these sets of variables influences voting turnout differently, and persuasive arguments for the importance of each set have been advanced. This research, however, is concerned with these variables primarily as compounding factors in relation to the basic relationship between social participation and voting turnout.

PREVIOUS RESEARCH

Every voting turnout study that has included a measure of associational participation has found these two variables to be fairly highly correlated.³ But most of this research has been straight-forward descriptive analysis, attempting neither to (a) relate the empirical correlation to any theoretical argument, or (b) perform multivariate analysis, examining the relationship while holding constant other variables. Of the few multivariate analyses of voting turnout, some have been quite narrow in scope (Lazarsfeld, 1944,⁴ and Connelly and Field, 1944⁵), others have examined only a single explanatory variable (Glen and Grimes, 1968⁶),

³ Among the better-known of these works are the following: Berelson, *et al.* (1954:336-7; Campbell, *et al.* 1952:29 and 1954:70-73; Hastings 1956; Lane 1959:45-62; Lipset 1960:179-219; Lazarsfeld *et al.* 1944:40-51; and Zimmer and Hawley 1959.

⁴ Lazarsfeld found that controlling for level of political interest eliminated the relationship between voting turnout and education, income, age, and religion (but not for sex), thus suggesting that interest in politics is an intervening factor between these independent variables and voting.

⁵ In this study, with level of income controlled, the voting rate became the same for all educational levels except college graduates for whom it remained higher.

⁶ Whereas most previous studies had found that voting rates declined after age sixty, they demonstrated that this decline is due largely to lower education levels and a preponderance of females among the old. With these two variables controlled, the voting turnout rate in their sample did not begin to decline until age eighty.

and still others (Erbe, 1964,⁷ Alford, 1968, and Alford and Scoble, 1968⁸) have used voting turnout as only one among many variables in composite indexes of overall political participation, so that their results do not apply directly to the act of voting. The present research, in contrast, treats voting turnout as a single dependent variable within a complex, multivariate theoretical framework.

RESEARCH DESIGN

Data for this research were taken from the 1968 Indianapolis Area Project of the Institute of Social Research at Indiana University. The sample for the study, consisting of 750 adults, was drawn from the Indianapolis "urbanized area" (central city and surrounding suburbs) using probability sampling with quotas.⁹ Trained graduate students and professional interviewers conducted the survey during January–March 1968.

As dependent variables, rates of voting turnout in three different national elections were examined: (a) the 1966 Congressional election, which was the last national election prior to this study, (b) the 1964 Presidential election, and (c) the 1960 Presidential elec-

tion.¹⁰ Whereas many earlier voting studies computed the proportion of the total sample voting in an election, this research *expresses the voting rate for each election as a percentage of the number of persons eligible to vote in that election*, and omits those made ineligible by residency requirements and/or age. Thus, the category of "nonvoters" includes only eligible but unregistered persons, and registered persons who did not vote. Hence the population base differs for each election: for 1966 it is 651 of the sample of 750; for 1964 it is 649; and for 1960 it is 592. Data from the three elections are analyzed separately, rather than being combined into a single index, for two reasons: (a) to insure that the results are not affected either by the nature (Presidential versus Congressional) or time of the election; and (b) because the pool of eligible voters (and hence the N) differs for each election.

The social participation independent variables were operationalized as follows: *Voluntary Association Participation Index*: For each voluntary association (including labor unions but excluding churches) to which a person belonged, he received one point for membership, two points if he attended at least half the meetings, and three points if he had ever held office or served on a committee.¹¹ *Church Participation Index*:

⁷ He found organizational participation more highly correlated than either socioeconomic status or political efficacy with overall political participation; but in multiple correlations SES evidenced a slightly stronger partial relationship than did participation, while efficacy became nonsignificant.

⁸ This research, which examined relationships between some twenty independent variables and a combined index of political participation, showed that education, voluntary association activity, and home ownership were the most basic and strongest predictors of overall participation. (These predictor effects were approximately equal.)

⁹ This procedure, developed by the National Opinion Research Center, uses probability sampling down through the selection of blocks (or block clusters), but chooses individual respondents by quotas within five categories defined by age for men and employment status for women. (These factors have been found to affect availability for interviewing most directly.) Quotas for each category in each block are determined by the population composition in that census tract. In addition, strict controls are imposed on interviewers to insure that no dwelling units with potential respondents are skipped. Though this procedure increases sampling error slightly (less than 10 percent in most cases), it saves considerable time and cost. For added details of this technique, see Sudman (1966).

¹⁰ Survey questions on voting always evoke bias—in this case toward over-reporting voting rates, especially in earlier elections—but there is no simple technique for eliminating this bias. Rather elaborate statistical adjustments can reduce the bias if one wishes to provide accurate descriptive figures, but our concern is to analyze relationships between variables rather than describe the total population. The following three questions were used to elicit voting turnout: "Did you vote in the 1966 Congressional elections, when we elected members of the U. S. House of Representatives?" (Indiana did not elect a U. S. Senator in 1966), "Did you vote in the 1964 Presidential election, when Johnson ran against Goldwater?" and "Did you vote in the 1960 Presidential election, when Kennedy ran against Nixon?" If the respondent said "no," he was asked if he were registered to vote in that election, and if not was he eligible to register at that time.

¹¹ Questionnaire copies, with the exact wordings of these questions, as well as more detailed descriptions of the procedures used in index construction, are available from the author on request if they are needed to replicate any portion of this research.

Points were awarded similarly for church membership, frequency of attendance at services, and membership and participation in church-sponsored groups. *Community Participation Index*: Points were given for frequency of participation in public community events and community-wide service projects. *Friends Interaction Index*: Based on the number of one's close personal friends in the Indianapolis area, frequency of joining them for informal activities, and membership and activities in informal friendship groups. *Neighbors Interaction Index*: Based on the number of people in the immediate neighborhood addressed on a first-name basis, and frequency of talking with them. The remaining independent variables will be described as they appear in the analyses.

All the analyses reported here were performed with the Multiple Classification Analysis computer program, which provides zero-order (η), partial (β), and multiple (R) correlation coefficients with nominal and ordinal data. These coefficients are roughly analogous to Pearsonian r 's, and η and R can be interpreted, when squared, as the proportion of total variation explained by the predictor variable(s). And although squared betas cannot be interpreted in terms of variation explained, they do indicate what the strength of an observed relationship between two variables would be if the compounding effects of all other variables in that analysis were eliminated; it is therefore legitimate to compare the relative sizes of various beta coefficients. The program uses an additive model and assumes that none of the predictor variables are highly intercorrelated, but it does not assume a linear model or designate the direction of relationships.¹² In this paper, only coefficients of .10

They will eventually be given in a monograph based on these data, titled *The Social Contexts of Political Participation*.

¹² Because the MCA program is based on an additive model, it does not measure any interaction effects that may occur between pairs of predictor variables. Thus the program's multiple R coefficient represents solely the sum of the separate effects of individual predictor variables, and does not include any interaction effects on the dependent variable. As a result, the R may sometimes be smaller than it would if we also measured and included whatever interaction effects that might occur. But failure to include such effects does not invalidate the program's R 's; it merely reduces

or larger will be viewed as substantively as well as statistically significant.¹³

FINDINGS FOR SOCIAL PARTICIPATION

To test Hypothesis 1, we first correlate the Voluntary Association Participation Index with voting turnout in the 1966, 1964, and 1960 elections. The resulting zero-order (η) coefficients are .30, .30, and .32, respectively—all of which might be described as moderately strong. These coefficients, as well as the percent of respondents in each category who voted in each election, are reported in the three left-hand columns of Table 1's top panel. (These percentages must be examined to see if the relationship is monotonic, since the coefficients do not assume a linear model.)

The hypothesis also states that the above relationship should occur among all types of voluntary associations. Consequently, all the organizations to which respondents belonged were coded into one of twelve categories, as follows: labor union, fraternal association, veterans or patriotic association, business or civic association, educational association, youth-serving association, cultural association, nationality or ethnic association, sports or hobby group, professional or scientific association, social or recreational association, and charitable or welfare association.

our total ability to explain variation in the dependent variable. That is, the MCA program does not assume that no interaction is occurring; it simply fails to take this into account. A more thorough data analysis might have looked for possible interaction effects, but our theoretical argument does not suggest that interaction should be relevant. Our aim here is to evaluate that theory, not search randomly for all possible sources of variation. The MCA program does assume, however, that none of the predictor variables are highly interrelated. Only the indexes of Political Contacts and Political Orientations, for which the η coefficient is .51, approach high relatedness—and this figure is within the program's usual tolerance limits. Moreover, as we shall note later, when these indexes are simultaneously related to voting turnout, both retain significant separate effects on the dependent variable.

¹³ Statistical significance at the .05 level with these data requires a coefficient of between .08 and .10, depending on the N . Substantively, however, a coefficient of less than .10 is not meaningful, even if it is barely significant statistically.

Table 1. Zero-Order, Partial, and Multiple Relationships Between Social Participation Variables and Voting Turnout in the 1966, 1964, and 1960 Elections.

	Zero-Order Relationships: Actual Voting Rates and Correlations			Partial Relationships: Adjusted Voting Rates and Correlations		
	1966	1964	1960	1966	1964	1960
Voluntary Association Participation Index*						
None	56%	69%	72%	61%	74%	76%
Low	64	77	81	67	79	82
Moderate	79	94	96	74	89	91
High	93	95	98	84	87	91
Eta/Beta	.30	.30	.32	.18	.16	.18
Community Participation Index*						
None	52%	66%	69%	61%	72%	76%
Low	64	78	79	67	78	83
Moderate	76	84	93	77	89	91
High	86	96	95	79	89	87
Eta/Beta	.28	.30	.30	.14	.17	.13
Church Participation Index*						
None	46%	61%	71%	53%	68%	75%
Low	63	74	76	65	76	77
Moderate	75	89	90	74	87	89
High	85	92	93	78	84	89
Eta/Beta	.28	.30	.29	.18	.17	.17
Friends Interaction Index*						
None	52%	62%	66%	63%	70%	74%
Low	72	79	84	75	81	85
Moderate	67	83	87	70	83	86
High	79	90	91	68	83	83
Eta/Beta	.18	.24	.23	NS	.11	NS
Neighbors Interaction Index*						
None	53%	64%	67%	64%	76%	78%
Low	67	82	84	69	85	86
Moderate	74	83	87	70	79	84
High	74	86	90	69	79	84
Eta/Beta	.16	.18	.20	NS	NS	NS
Multiple Correlation R (Voluntary Association, Community, and Church Participation only)				.36	.38	.38
Social Participation Combined Index*						
Low	45%	59%	61%			
Moderately low	68	79	84			
Moderately high	77	87	91			
High	80	93	94			
Eta	.40	.40	.41			

*All of these indexes contain six categories (eight for the Combined Index), and the eta and beta coefficients are based on the full range of these categories, but for ease of presentation in this table each index has been collapsed to four categories.

With one exception, voting was significantly related to participation in each kind of organization, with coefficients ranging between .12 and .18. The exception was labor unions, in which participation was uncorrelated with voting turnout. This finding is easily interpreted, however, since for many workers

union membership is not voluntary, but is required for employment. Apparently the political mobilization process occurs only in truly voluntary organizations. We could speculate that the process doesn't occur in other semi- or nonvoluntary organizations such as prisons, mental hospitals, or the

military; but our data do not permit a test of this possibility.¹⁴

From these findings, we conclude that Hypothesis 1 is confirmed: participation in truly voluntary associations is positively and somewhat strongly related to voting turnout, regardless of the nature of the organization. Notice, moreover, that the coefficients for the combined Voluntary Association Participation Index are much stronger than the figures for any specific kind of association (and this remains true even if the Index is reduced to a dichotomy). This suggests that social participation effects on voting are cumulative. The greater the number of one's organizations, the more likely will he vote. Indeed, a simple count of the number of organizations to which respondents belonged correlates with the three elections at .27, .26, and .26, respectively.

Hypothesis 2 states that participation in community and church activities will also be related to voting turnout, even with voluntary association participation controlled. Voting percentages and correlation coefficients for these variables are shown in the second and third panels of Table 1. The zero-order (η) coefficients for both factors (given in the table's three left-hand columns) are approximately the same as those for voluntary associations—around .30.

The three right-hand columns in the table report the partial (β) relationships between voting and each predictor variable, simultaneously holding constant the effects of all other predictor variables. Under these extensive controls, the coefficients for voluntary association, community, and church par-

ticipation all decline in strength, but remain statistically significant. In other words, each measure of social participation effects voting turnout separately, and no observed zero-order relationship is spurious. Hypothesis 2 is therefore verified.

Hypothesis 3, concerning interpersonal interaction, can be investigated with the data in panels four and five of Table 1. The zero-order coefficients for interaction with both friends and neighbors are significant but not strong, ranging from .16 to .24. When all the other participation measures are held constant, moreover, five of the six partial coefficients for these variables become non-significant, and the remaining one is very low. In short, neither interpersonal interaction index appears to explain significant amounts of variation in voting rates, once participation in more formal voluntary associations, community activities, and church events has been accounted for. It may be—though these data cannot test the possibility—that personal friendships occurring within more formal settings do effect political mobilization in some way. But we can at least infer that personal friendships do not, by themselves, influence voting turnout. Consequently, Hypothesis 3 can be accepted; and we shall discard the Friends Interaction Index and Neighbors Interaction Index from further analyses.

The multiple correlations in Table 1 are therefore based only on the voluntary association, community, and church participation indexes. Since these multiple R 's are all much larger than any beta coefficient for their components, it is clearly preferable to use all three factors together to predict voting turnout, rather than any one alone.¹⁵

¹⁴ Despite these negative findings for labor unions, unions were kept in the overall Voluntary Association Participation Index on the theoretical grounds that all discussions of pluralistic theory have included them. Thus an adequate test of this thesis must take them into account. Leaving unions in the index had the effect of reducing the observed correlations, since participating in these organizations does not contribute to the tendency for voting turnout to increase with association participation. Conversely, were we to exclude unions from the index, the observed correlations with voting turnout would be even higher. Further research might explore this distinction between union and non-union associations—or more generally, between economically and non-economically oriented associations.

¹⁵ As an alternative measure of community participation, we examined length of residency in Indianapolis—a variable that most studies have found to correlate with voting. Length of residency is moderately related to voting turnout, as follows: 1966 = .23, 1964 = .18, and 1960 = .15. However, these coefficients are not as high as those obtained with the Community Participation Index; and when the two measures are simultaneously related to voting, the resulting partial correlations for residency are much lower than those for the participation index. In fact, the observed relationship between community residency and voting turnout appears to be largely a spurious consequence of the fact that older people (who tend to vote reg-

Finally, for convenience, these three variables were combined into a single Social Participation Combined Index, as shown in the last panel of Table 1.¹⁶ Comparison of the eta coefficients for this index with the corresponding multiple R's based on its component variables reveals that the Combined Index predicts voting turnout slightly more strongly, and hence does reflect the overall dimension of social participation.

Thus far we have been examining the effects of social participation on the voting turnout of individuals, as a test of the mobilization thesis. But this has ignored the possibility that voluntary associations may become involved in political activities as organizations, enacting a mediating role in politics. Though this topic takes us outside the social participation theory, it will be interesting to see whether our data provide any support for the mediation version of pluralistic theory. Three pieces of data are relevant to this question: (1) Most voluntary associations remain totally nonpolitical most of the time. Only 18 percent of the respondents' organizations had ever (in their memories) been politically active in any way, and only 7 percent of the respondents had personally participated in any of these actions. (2) For these 7 percent a Voluntary Association Political Activities Index was constructed, which included the number of politically active organizations to which a respondent belonged, plus the number of times he had participated in their political activities. This index correlated with voting turnout in the 1966, 1964, and 1960 elections at only .21, .20, and .22, respectively. (3) Holding constant respondent scores on the overall Voluntary Association Participation Index reduces all of the above correlations to nonsignificance. These findings

ularly) are also more likely to have lived in the community longer. The beta correlations for community residency drop to .14, .16, and .14, respectively, with age held constant. With the three major social participation indexes added as controls, these relationships for community residency become nonsignificant.

¹⁶ The procedure used in constructing this Combined Index—as well as all the other combined indexes—was to sum a respondent's scores from the component variables, and then group these into eight categories as nearly equal in number of cases as possible. A more detailed description of this procedure is available on request.

tentatively suggest that in voluntary associations the mobilization process is much more crucial than the mediation process in producing political participation.

CAUSAL INFERENCE ARGUMENT

We have now determined that three measures of social participation—in voluntary associations, community affairs, and church events—correlate independently with voting turnout. But which causes which? How relevant is the social participation theory as a causal explanation of voting turnout? Though causation cannot be directly tested, it can be inferred with some confidence if, in addition to covariation and a reasonable theoretical causal explanation, we can demonstrate temporal sequence between the presumed cause and effect. In this case, what evidence have we that social participation precedes voting?

Though we didn't solicit their life histories, we did ask respondents for the year when they joined their current voluntary associations. Regrettably, we did not gather comparable data for community and church participation, which will limit this analysis. Nevertheless, if we can show that people do belong to voluntary organizations prior to voting, we will have some additional evidence for inferring causation from social participation to voting turnout.

Since the 1960 election is so remote from the 1968 data on voluntary associations, we shall here examine only the 1966 and 1964 elections, in three analyses.¹⁷ The first pertains to each respondent's "most important" association. (If he belonged to only one, this automatically became his most important organization; if two or more, he was asked to identify the one most important to him.) Of the 310 respondents voting in 1966 who had a "most important" organizational membership in 1968, 94 percent belonged to the

¹⁷ We omitted the 1960 election because all data on voluntary associations pertain to organizations to which respondents currently belonged. Those who had been members of an association in 1960 but had either changed organizations or dropped out before 1968 would have been counted as nonmembers in the 1960 election and that would clearly have distorted the findings. This distortion could also occur with the 1964 and 1966 elections, but the shorter time spans involved for them should prevent its frequent occurrence.

association at least one year prior to that election. Of the 354 respondents voting in 1964 who had a "most important" organizational membership in 1968, 91 percent belonged before the election.

The second analysis includes only respondents belonging to two or more organizations, and pertains to their "second most important" association. A total of 173 persons voted in 1966 and named a second organization, while 198 did so for the 1964 election. In both cases, 88 percent belonged to their second organization prior to the election in question.

The third analysis pertains to the "third most important" organization among persons with three or more memberships. Of the 101 people with a third association who voted in 1966, 92 percent had joined prior to that election. Of the 111 people with a third association who voted in 1964, 90 percent were members before the election.

These findings support the causal argument that social participation tends to influence voting turnout in a temporal sequence, though two possible biases (in addition to the omission of data for community and church participation) should be noted: First, the data for both organizational participation and voting pertain only to the past few years, and many respondents may have begun voting long before joining any voluntary associations. Even so, we can argue that voluntary association participation may continually reinforce the habit of voting and hence influence current voting turnout, whatever the initial causes. Second, these analyses are limited to persons belonging to at least one voluntary association; and since organizational participation tends to increase with age and SES, we may have introduced bias by looking disproportionately at older and higher status persons, who also tend to vote more regularly. However, as the next section shows, controlling for age and SES does not eliminate the relationship between voluntary association participation and voting, so that the effects of any such bias cannot be great.

Additional support for this causal argument is provided by Maccoby (1958). He examined the effects of membership in one voluntary association (concerned with de-

veloping a publicly sponsored community recreation program) on voting turnout in two primary elections—one held before the organization was formed and the other two years after. The organization took no part in the second campaign, and its objectives had no ties with election issues. Nevertheless, among nonvoters in the first election, participants in this association were much more likely than nonparticipants to vote in the second election: 66 percent to 39 percent, respectively. Moreover, the voting rate rose to 80 percent among those who had become highly involved in association activities. These findings are fairly compelling, since the research design approximated a controlled experiment.

Based on both our and Maccoby's data, we conclude that it is tenable to infer a causal sequence from social participation to voting turnout, in accordance with our initial expectation.

FINDINGS FOR COMPOUNDING VARIABLES

As we noted earlier, previous studies of voting turnout have found that it correlates with a host of other social, economic, and political variables. And since many of these factors are also related to social participation, their effects must be held constant before we can claim that social participation has independent effects on voting turnout. This section therefore examines these compounding variables, both separately and in combination with social participation, to investigate our general expectation that the basic relationship between social participation and voting turnout will remain significant under extensively controlled conditions. To keep the discussion manageable, many of the findings will be briefly summarized without reporting the supporting data, but all these tables are available on request.

Demographic Variables. Past research has established that men tend to vote more often than women, whites more often than blacks, middle-aged and older people more than the young, married persons more than unmarried ones, Jews more often than gentiles, and Catholics more than Protestants. These demographic variables of sex, race, age, marital

status, and religion were therefore related to voting turnout in the three elections.

As expected, men in this sample vote slightly more often than women, but the differences are statistically nonsignificant. With respect to race, blacks appear to have voted slightly more often than whites, but again the differences are nonsignificant.¹⁸ Age is moderately correlated with voting turnout (mean eta for the three elections = .22), with the relationship remaining monotonic up to age seventy and declining only slightly up to age eighty. Married persons vote more extensively than single people, but the rates for the divorced and widowed are as high as those for married people. All three of these latter correlations are quite weak, and become nonsignificant with age controlled. Religious preference (coded conservative Protestant, liberal Protestant, Catholic, and no preference¹⁹) correlates moderately with voting (mean eta = .22). The interesting finding here, however, is that liberal Protestants tend to be as active as Catholics, while both conservative Protestants and persons with no religious preference vote much less often.

Since the correlations between voting turnout and sex, race, and marital status were all weak or nonsignificant, and since further analyses showed these variables to have no effects on any other relationship in this study, we shall discard them here. Age, how-

ever, remains significantly related to voting with all other compounding variables controlled, as we shall see later. The same is not true of religion. Though controlling for age has no effect here, holding constant the variables of education, occupation, and income reduces all three relationships for religion almost to nonsignificance. And when the three social participation indicators are also added as controls, the three relationships become statistically nonsignificant. (Controlling religion does not affect the correlations for the social participation measures at all.) Hence the observed differences in voting rates among the religious preference categories are apparently spurious, and the religion variable can be discarded. Of the original five indicators of demographic location, only age remains as a significant independent predictor of voting participation.

Socioeconomic Status. All previous voting research has found that the higher a person's socioeconomic status—as shown by educational level, occupational status, and annual income—the more likely is he to vote. Our data support these generalizations for all three elections (mean eta for education = .26, occupation = .21, and income = .19).²⁰ When each of these variables was examined while holding constant the other two, education and occupation remained significantly related to voting turnout (though the coefficients for occupation were quite low), but income became nonsignificant. Moreover, the occupation correlations also became nonsignificant when the three social participation measures, as well as education, were held constant. (The relationships for the social participation measures were not substantially affected when either income or occupation, or both in combination, were used as controls.) Hence

¹⁸ In a separate paper (Olsen, 1970), I examined voting turnout and other kinds of participation among blacks, with socioeconomic status and age held constant. Under these controlled conditions, voting rates among blacks are considerably higher than among whites of comparable statuses, and the differences are significant in the 1964 and 1960 Presidential elections.

¹⁹ The sample was not large enough to permit analysis of each separate Protestant denomination, but preliminary examination of the data indicated that voting rates among the denominations varied too greatly to justify combining them into a single "Protestant" category. Hence the compromise decision was reached to dichotomize Protestant churches into the categories of "conservative"—including the pentecostal churches, Church of Christ, Disciples of Christ, Nazarene Church, and all Baptists—and the more "liberal" churches of Methodists, Presbyterians, Episcopalians, Congregationalists, and community churches. Because only six Jews fell into the sample, they could not be analyzed separately and were discarded from all analyses involving religion.

²⁰ The respondent's education was measured by total number of years of schooling completed, including technical and vocational training. Occupation of the head of the household was classified as "low manual" (all semiskilled, domestic service, and unskilled jobs), "high manual" (skilled workers, public service workers, and foremen), "high nonmanual" (clerical and sales workers, and managers and owners of small businesses with less than five employees), and "high nonmanual" (owners and managers of larger businesses, technicians, and all professionals). Family income was the total gross income in 1967 before taxes.

we shall retain only education as an indicator of socioeconomic status.

Political Contacts. This variable set is composed of four indexes measuring the respondent's exposure to the political system: Parents' Political Activities Index,²¹ Political Discussion Index,²² Mass Media News Exposure Index,²³ and Party Contacts Index.²⁴ All four indexes were found to have moderate to relatively strong correlations with voting turnout (mean eta for parents' participation = .20, political discussion = .27, political news exposure = .33, and party contacts = .37). However, the coefficients for parents' participation became nonsignificant when the other measures were simultaneously controlled. This finding does not necessarily mean that the degree of parents' political activity does not influence their children's later voting participation, but that these effects apparently operate indirectly, through more immediate factors such as exposure to the mass media or reading party literature. Nevertheless, this variable is not relevant here, since controlling it does not affect the relationships between social participation and voting turnout. Hence we discard it.

The correlations for political discussion also decline markedly when the other political contacts measures are controlled, but they remain barely significant because per-

sons who never discuss politics score lower than all other respondents. (There are no major voting rate differences among any of the other categories of political discussion.) While we cannot drop this variable, its importance is minimal.

Political contacts via the mass media and political parties are much more relevant for voting turnout, with the latter correlating more strongly than the former under simultaneous control (mean beta for party contacts = .24, mass media = .18). These findings suggest that house-to-house canvassing and other political leg-work are still greatly important in getting out the vote.

As a summary measure of this political contacts dimension, a Political Contacts Combined Index was constructed from the variables of political discussion (dichotomized), political news exposure, and party contacts, using the same procedure as with the Social Participation Combined Index.²⁵ This index correlated with voting turnout in the three elections as follows: 1966 = .41, 1964 = .41, 1960 = .37. This combined index, rather than its separate components, will be used in subsequent analyses.

Political Orientations. The three indexes in this set—Political Interest,²⁶ Party Identification,²⁷ and Political Efficacy²⁸—measure a person's various cognitive and evaluative orientations toward the political system. Political interest and party identification were found to exert approximately equal, and rather strong, effects on voting turnout (mean eta for political interest = .43, party

²¹ Constructed from questions on whether or not his father and mother had political party preferences, the frequency with which his parents discussed politics while he was growing up, whether they usually voted, whether they belonged to any political organizations, and whether they ever did volunteer work for a party or candidate.

²² Based on frequency of talking about political topics with personal friends, neighbors, relatives, and coworkers.

²³ Points were given for frequency of watching national network news broadcasts on television, listening to local news broadcasts on television and radio, reading national and international news in newspapers, reading editorials and columnists in newspapers, and reading political articles in magazines.

²⁴ Based on the frequency with which the respondent received mail from a political party and whether or not he read this mail, the number of times a party worker had called at his house and how long he usually talked with this worker, and other personal contacts with people active in politics.

²⁵ Since the only significant difference on the Political Discussion Index lies in the distinction between those who do and don't discuss politics with others, this index was reduced to a dichotomy. Because the maximum score on this index becomes 1, it contributes much less to the Political Contacts Combined Index than the other two indexes.

²⁶ Constructed from a series of questions on how interested the respondent was in political issues and activities in Indianapolis, in Indiana, at the national level, and in foreign affairs.

²⁷ Points were given for having a party preference in national politics, having a preference in local politics, and for being able either to describe some difference between the two major parties or tell why he thought they did not differ.

²⁸ Measured with the political efficacy scale devised by Campbell, *et al.* (1954), but focused on national politics.

identification = .41), but the political efficacy correlations were much lower (mean η^2 = .16). With all three indexes simultaneously related to voting, the partial coefficients for the first two remain fairly high and roughly equivalent (mean beta for political interest = .32, party identification = .28). Those for political efficacy become nonsignificant, however, so that this index was discarded.

Multivariate Analyses. We can at last gather the remaining control variables in a series of multivariate analyses, to evaluate our expectation that the relationship between voting turnout and social participation will remain significant under extensively controlled conditions. We will also be able to determine the independent effects of each compounding variable on voting turnout, holding all other variables constant.

The control variables to be included in this analysis are age, education, the Political Contacts Combined Index (based on the separate indexes for political news exposure, party contacts, and political discussion), and the Political Orientations Combined Index (based on the indexes for political interest and party identification). To measure participation in organized social activities, we shall use the Social Participation Combined Index, which is based on the separate indexes for voluntary association, community, and church participation.

Before combining all these variables in a single analysis, let us briefly examine three subsets that yield interesting findings. First, when just age and education are simultaneously related to voting, the partial (beta) coefficients for both are slightly higher than their original zero-order (η^2) coefficients, as follows: for age, mean η^2 = .22, mean beta = .27; for education, mean η^2 = .26, mean beta = .33. This occurs because both age and education are positively related to voting, but negatively related to each other (η^2 = -.25). Hence controlling for one enhances the other's positive correlation with voting turnout. (These effects do not occur when education is replaced by occupation or income, since both correlate positively with age.) This interactive effect of age and education can be viewed as meaning that young and poorly educated persons are

quite unlikely to vote, while older and highly educated persons are very likely to go to the polls—but that neither type exists in great numbers in the total population. For the majority, age and education pull in opposite directions, partly cancelling out each other's effects on voting turnout.

Second, when education (as an indicator of socioeconomic status) and the Social Participation Combined Index are simultaneously related to voting, we can answer the oft-debated question: What are the relative effects of socioeconomic status versus social participation on voting turnout? Some previous studies have favored socioeconomic status, others social participation, and still others have found their effects on political participation to be approximately equal.²⁹ Our data clearly favor social participation: for the three elections, the mean beta for social participation with education controlled = .35, while that for education with social participation controlled = .14. (The results are almost identical when a Socioeconomic Status Combined Index, based on education, occupation, and income is used in this analysis.) This finding should not be interpreted to mean that education (or socioeconomic status) is unimportant as an influence on voting, but that many of the effects of this dimension on voting are conveyed indirectly through participation in voluntary associations, community events, and church activities—all of which relate directly and strongly to education.

Third, since the political dimensions of contacts and orientations are highly interrelated (η^2 = .51), we must ask whether each affects voting turnout independently. When these variables are joined in a multiple correlation, the resulting mean beta coefficient for political contacts with orientations controlled = .24, while the mean beta for political orientations with contacts con-

²⁹ Erbe (1964) found socioeconomic status to have a slightly higher partial correlation with political participation than voluntary association participation. Nie, *et al.* (1969) reported a somewhat higher zero-order correlation for organizational participation than for socioeconomic status with political participation, based on data from five nations. Alford and Scoble (1968) believed the effects of these two factors to be roughly equal.

trolled = .40. Thus both dimensions exert separate effects on the decision to vote, though political orientations is a noticeably better predictor.

We can now bring all of these voting correlates together in a single analysis. We note first that all four remaining control variables are at least moderately correlated with social participation: age = .20, education = .41, political contacts = .45, and political orientations = .37. Consequently, holding constant the effects of these variables will doubtless reduce the relationships between social participation and voting turnout. But by how much? Conversely, when simultaneously controlled, will all the compounding variables remain significantly related to voting? For answers, let us examine the data in Table 2, which joins the Social Participation Combined Index, age, education, the Political Contacts Combined Index, and the Political Orientations Combined Index in multiple correlations with voting turnout rates in the three elections.

Several conclusions can be drawn from these data:

(1) Even under these extensively controlled conditions, social participation remains relatively strongly related to voting turnout. Thus our general expectation is verified and the social participation thesis is supported. The mean beta for this index from the three elections is .22, which is surpassed in magnitude only by the figures for political orientations.

(2) Age remains moderately related to voting turnout, though the beta for the 1966 election is higher than the figures for the other two elections, which suggests that age has more impact on voting in Congressional than Presidential elections.

(3) Education is significantly but extremely weakly correlated with voting in the 1966 and 1960 elections, while the 1964 figure becomes nonsignificant. Once again, we interpret this finding to mean that education (or more broadly, socioeconomic status), does not directly affect voting turnout, but rather that its effects are transmitted indirectly through other related factors.

(4) All partial coefficients for the Political Contacts Combined Index remain significant, but not strong; the mean beta = .13.

Note that the original eta coefficients for this index (1966 = .41, 1964 = .44, 1960 = .40) were slightly higher than those for the Social Participation Combined Index (1966 = .40, 1964 = .40, 1960 = .41) in two of the elections, but that in Table 2 social participation has become a considerably stronger voting predictor than political contacts. (Three separate analyses, using just age, education, and political orientations as single control variables, showed that each reduces somewhat the relationship between political contacts and voting, and that their effects are cumulative.)

(5) Last, the Political Orientations Combined Index remains quite strongly related to voting under these controlled conditions (mean beta = .36), especially in the 1964 and 1960 Presidential elections. At this point, political orientations seem to predict voting turnout better than social participation, but presently we shall question this interpretation.

Before leaving Table 2, we should also note that the multiple coefficients obtained from these correlations are all relatively strong for social science research with ordinal data. These multiple R^2 's of .56, .59, and .58 for the three elections (the corresponding R^2 's are .31, .35, and .34) indicate that we are explaining approximately a third of the variation in voting turnout rates with all predictor variables combined.³⁰

DISCUSSION

The main empirical findings of this research are as follows: (1) Participation in voluntary associations correlates with voting turnout at mean eta = .31. This relationship occurs with all kinds of organizations except labor unions, which suggests that the political mobilization process occurs only in truly voluntary organizations. (2) Participation in community events and church activities are also both related to voting, with mean etas for both of .29. Moreover, all three of these

³⁰ This multiple correlational analysis was repeated using all the separate variables rather than the combined indexes for social participation, political contacts, and political orientations. The results were essentially equivalent to those reported in Table 2, in that all these variables remained significantly related to voting turnout.

Table 2. Partial and Multiple Relationships of Age, Education, Social Participation Combined Index, Political Contacts Combined Index, and Political Orientations Combined Index with Voting Turnout in the 1966, 1964, and 1960 Elections.

	Adjusted (Partial) Voting Rates and Correlations		
	1966	1964	1960
Social Participation Combined Index			
Low	52%	68%	69%
Moderately low	67	78	83
Moderately high	75	83	87
High	77	89	91
Beta	.22	.20	.24
Age			
21-29	48%	65%	72%
30-39	67	81	83
40-49	71	80	83
50-59	76	84	85
60 and older	82	87	89
Beta	.25	.17	.12
Education			
0-8 years	62%	79%	79%
9-11 years	67	80	87
12 years	70	79	83
13-15 years	70	82	86
16 or more years	80	82	86
Beta	.11	NS	.10
Political Contacts Combined Index			
Low	68%	75%	80%
Moderately low	65	81	84
Moderately high	67	80	84
High	74	84	85
Beta	.10	.15	.14
Political Orientations Combined Index			
Low	48%	55%	61%
Moderately low	70	86	89
Moderately high	78	89	93
High	78	89	88
Beta	.29	.40	.39
Multiple R	.56	.59	.58

social participation measures remain significantly related to voting with the other two factors controlled. The Social Participation Combined Index, based on all three measures, correlates with voting turnout with a mean eta of .40. (3) Measures of informal interaction with friends and neighbors correlate only weakly with voting, and both relationships become nonsignificant with the above measures of participation in more formal settings held constant. (4) Among respondents belonging to one or more voluntary associations who voted in the 1964 and 1966 elections, almost all belonged to these organizations before the election. These data provide some basis for inferring causation from association participation to voting turnout. (5) Among the other voting cor-

relates examined in this study, the factors of (a) age, (b) education, (c) political contacts via the mass media, partisan mailings, and party workers, and (d) political orientations such as party identification and interest in politics, all remain significantly related to voting turnout when simultaneously controlled. Of these four variables, political orientations has the strongest partial correlation, at mean beta = .36. (6) With the effects of these four compounding factors all held constant, the Social Participation Combined Index remains significantly and moderately correlated with voting, at mean beta = .22. (7) Taken together, all five predictor variables produce a mean multiple correlation with voting turnout of $R = .58$ ($R^2 = .33$).

From these findings, we conclude that participation in the activities of voluntary associations, one's community, and one's church does mobilize people to vote. In addition, the efforts of political parties and candidates to reach voters through the mass media, partisan mailings, and personal visits do have some further effects on getting out the vote—though these are minimal in comparison with the effects of social participation. Voting rates also rise steadily with age, at least until retirement, although rising educational levels among the young may in time erase many of these age differentials. Education does show a moderate relationship with voting, but the main causal process here appears to go from education to social participation to voting turnout. The other socioeconomic status indicators of occupation and income, finally, seem to be relatively unimportant for voting turnout.

Of all the variables examined in this study, the dimension of political orientations—having a party preference and being interested in politics—appears at first to be the most relevant for explaining voting. But this assumes that political orientations act as independent causal factors. It is equally plausible to argue, however, that this dimension of cognitive and attitudinal responses toward the political system acts as an intervening variable between the other predictor variables and voting. From this perspective, political orientations would be seen as providing a vehicle through which the other phenomena—especially one's social participation and political contacts—affect one's decision to vote. Such a view does not diminish the importance of political orientations in the total theoretical model, but argues that they should not be taken as initial causes.

By this reasoning, we might remove political orientations from the multiple correlations in Table 2. When we do so, the multiple R 's are only slightly reduced, so that mean $R = .49$ ($R^2 = .24$). This finding indicates that political orientations may in fact be operating mainly as an intervening variable, since removing it does not reduce the multiple correlations too greatly—though it does have some small independent effects on voting (or at least effects that are independent of the other predictor variables

examined here). In this multiple correlation, the mean partial beta coefficients for the remaining variables are as follows: social participation = .27, political contacts = .19, age = .21, education = .14. Thus with political orientations omitted as an independent causal variable, the dimension of social participation becomes the most important predictor of voting turnout.

To conclude, let us return to the theory of social participation as an explanation of voting turnout, which is the central concern of this study. This theoretical explanation, derived from the mobilization version of social pluralism theory, argues that active involvement in voluntary, special-interest, nonpolitical organizations—including voluntary associations, community activities, and churches—tends to bring individuals into contact with political issues, actors, and affairs, and provides them with information and skills necessary for voting and other kinds of political participation. Partially as a result of this stimulation, the individual gives greater attention to political messages from the mass media, party workers, and other sources, and also develops stronger party identification and political interest. All of these factors then combine to propel individuals to the polls. The main import of this argument is that we must look beyond the political system for many of the crucial causes of political participation. In addition to the commonly examined nonpolitical factors of age and education, we must give special attention to the individual's involvement in organized social activities which may have little or no formal connection with politics. The obvious prior question—which awaits further research—is what factors lead people to join and become involved in voluntary associations and similar activities? The social participation theory by itself provides only a partial explanation of voting turnout, but the results of this study indicate that it must be given serious attention if one wishes to understand why people go to the polls on election day.

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BLUE COLLAR ANGER: REACTIONS TO STUDENT AND BLACK PROTEST *

H. EDWARD RANSFORD

University of Southern California

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The hypothesis that working class respondents are especially antagonistic toward the black and student movements is tested with a sample of white Los Angeles residents (N=477). In support of the hypothesis, working class persons and those with less than a high school education (in contrast to those higher in the occupational and educational hierarchies) are more likely to:

- (a) *express punitive attitudes toward student demonstrators,*
- (b) *oppose granting students more power, and*
- (c) *feel blacks are pushing too hard for things they don't deserve.*

To locate rationales that would explain these relationships, intervening and specification variables were introduced in the analysis. These variables are: respect for authority, belief in the American Dream, belief that the needs of the working man are neglected, and perceived powerlessness. Substantial support for these explanations of blue collar anger is found.

LITTLE is known about the reactions of the white majority to campus protest and black demands. It seems obvious that a great many majority group Americans are angry, even outraged, by such incidents as student takeovers of buildings or by black power demands for reparations or preferential hiring in industry. The major question

of this paper is whether this anger is randomly distributed in the socioeconomic structure, or is far more likely to be found in the working class environment. Are blue collar people uniquely antagonistic toward the goals and methods of student and black demonstrators?

A number of recent themes in the race and stratification literature suggest that working class people should be more antagonistic toward black protestors than those higher in the socioeconomic structure. Van den Berghe observes that race relations in the United States have shifted from total subordination of blacks in power, privilege, and prestige ("paternalistic" race relations) to a kind of competitive relationship found in advanced industrial societies.

In such a dynamic industrial society with its great geographical mobility and its stress on impersonal market mechanisms and universalistic and achieved criteria of occupa-

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tional selection, race relations are quite different from what they are under agrarian conditions. The master-servant model with its elaborate caste etiquette and its mechanisms of subservience and social distance breaks down to be replaced by acute competition between the subordinate caste and the working class within the dominant group. (van den Berghe, 1967:29, 30)

In industrialized, multiracial societies when the caste-paternalistic lid is lifted, the white working class is most likely to feel threatened by black upward mobility. Working class people have small, but hard earned, amounts of power, privilege and prestige that they are anxious to protect. (Skolnick, 1969:210-240)

In addition to van den Berghe's description of the macro processes, a number of more specific rationales link blue collar position with anger toward protest activity. It is these more middle range and measurable rationales that form the core of this paper.

THEORETICAL EXPLANATIONS AND HYPOTHESES

At least three somewhat distinct (though partly overlapping) explanations can be advanced for the hypothesis that working class people will be more antagonistic toward student and black demands than people higher in the class structure. All three indicate that the protest methods and demands of students and blacks grate especially hard on the values, outlooks, and economic fears of the working class person.

1. *The Conformity-Idealization of Authority Explanation.* A number of studies indicate that the blue collar environment stresses the value of respect for authority. In the socialization of children, for example, a recent study (Kohn, 1969) shows that blue collar parents are more likely to emphasize obedience and neatness in contrast to middle class parents who emphasize internal dynamics such as curiosity and self realization. The stress on conformity and obedience is due in part to occupational environments. Blue collar persons typically are in occupations which demand repetition, conformity and adherence to rules. Thus Kohn and Schooler (1969) find that men's opportunities to exercise occupational self direction—that is, to use initiative,

thought and independent judgment in work—interpret much of the relationship between class and authoritarian orientations. Other studies indicate that working class persons are less sophisticated in cognitive thinking. Being less well read and educated, as well as isolated from people with different views from their own, their thinking is concrete, simple and stereotyped (Lipset, 1960:87-126). Lane and Lerner (1970:105) make the same point when they note that blue collar men emphasize attributes of individuals rather than attributes of abstract systems. They hold to simple moral and ethical perceptions of the world rather than perceptions involving relativity of judgment or competing values. The lesser degree of cognitive or abstract thinking feeds into the same point, that working class people are more likely to conform to basic institutions than to question the existing system.

From this perspective, it follows that working class persons will be more outraged by campus protest and black demands than those higher in the class structure. Student protest and black protest symbolize a classic flaunting of authority and downgrading of institutions. The four letter words, the styles of dress, and the direct confrontation methods all suggest disrespect for authority.

2. *Belief in the American Dream—Neglect of Workingman's Needs.* This revised class conflict model, as opposed to a Marxian model in which the white working class is a revolutionary force for change, sees the blue collar worker as reaffirming traditional beliefs in the openness of the American system. Hostility is directed toward the black lower class as undeserving of special opportunity as well as toward white liberal persons in power who seek to remake traditional America at the workingman's expense. Working class people have made modest economic gains through union victories and through their own hard work and sacrifice. They are far from economically secure, however. They believe poor people and black people are at the bottom of the class structure because of their own laziness, not because of racism or other institutional barriers (Lane and Lerner, 1970). Given their beliefs in hard work and the openness of the American structure, working class persons are hostile toward ghetto rioters and black demands for quotas

and preferential job treatment. It is not simply that they feel blacks do not deserve special opportunity. They are angry because it seems to them that they are being asked to pay the biggest price for "social justice" (See Schneider, 1970). If they yield to demands for special opportunities and super-seniority for black workers, they, not the secure upper-middle-class people, face the greatest threat of being laid off.

From the workingman's point of view, far too much attention is being paid to the poor, and especially the black poor. In a recent survey (Newsweek, 1969), sixty-five % of white middle Americans felt that blacks have a better chance than whites to get financial help from the government when they're out of work. Increasingly, white workers perceive that it is easier for an unemployed black to get aid and sympathy than a hard working white. Further, working class people are angry because they perceive that they are taxed heavily to support welfare budgets for the poor, and they have fewer mechanisms like business expense accounts, to escape taxation. In short, they feel they are paying for the special opportunities given minority persons—compensations they believe to be unnecessary in a free and open society.

Although this second explanation for blue collar anger (the American Dream and neglect of the working man's needs) is more clearly related to antagonism toward black militance, it also ties in with antagonism toward campus activism. A perception of the American system as open and just is antithetical to the student activist view that the American system is racist, elitist, exploitative, and excessively authoritarian. Further, the quintessence of the American Dream is sending one's children to college so they can advance in the social structure and have a better life. It is only with great sacrifice, however, that the working class family can send their children to college. The shouting of affluent youth, "On Strike—Shut It Down!" is a multiple outrage to blue collar people. Activist students are not only attacking American values, but threatening to close an only recently opened channel of mobility for working class children.

3. *The Powerlessness Explanation.* From this point of view, a critical dimension of the

disaffection of white working class people is their sense of powerlessness over "radical" changes occurring in the country, and over political decisions directly affecting their lives. Working class people may perceive that they have few means to affect political change. Their occupational roles do not evoke respect and power in the larger society. Even in their own union the rank and file often have no real voice in making policy.

Recent student and black protest has, no doubt, heightened these feelings of political powerlessness. The worker may perceive that black and student militants have reached power centers and are forcing institutional changes that seem to him a distributive injustice or direct threat (for example, preferential hiring for blacks, relaxed entrance requirements and special aid for minority college students, and plans for increased school and residential integration that may affect him especially as an inner city resident living close to the ghetto). Of the "power structure's" various components, blue collar workers may view the government as especially hard to move and preoccupied with the problems of blacks.

The powerlessness explanation would predict that white working class people who feel politically powerless i.e., who expect public officials to be unresponsive to their needs, will be especially antagonistic toward student and black activists. Each gain in power for militant students or blacks may be perceived as a reciprocal loss in power, status, or a way of life for the working class person.

Note that the concept of powerlessness used in this study denotes a perception of the social system and does not necessarily refer to personal apathy or fatalism. Indeed, the more militant white action responses to black and student protest (such as "hard hat" demonstrations or the Tony Imperiale Armed Citizens Committee) suggest a high degree of personal confidence and efficacy combined with a low expectancy of being able to move a large impersonal system through normative action.¹

¹ The several definitions and measures used for the concept powerlessness have caused confusion in recent studies. For example, the consistently used interpretation for the correlation between power-

There is a distinct difference in the logic of the first two explanations and this third powerless explanation. The first two view class position as the source of certain outlooks and values (respect for authority, belief in American Dream, and belief that needs of working class persons are neglected); and these in turn prompt antagonism toward student and black rebels. That is, we are dealing with "interpretation" logic or variables that intervene between X and Y.² In

lessness and violence in my Watts research (Ransford, 1968) was powerlessness as a *low expectancy of gaining redress through institutional channels* (voting, the courts, civil rights movement etc.). However, a few recent reports on riot participation and riot attitudes have cast my findings into the category of apathy and fatalism (Forward and Williams, 1970; Paige, 1971). For a comment and rejoinder on the relationship between powerlessness and black militancy, see Ransford (1971) and Forward and Williams (1971).

Another conceptual distinction relevant to this study distinguishes political efficacy from political trust (Paige, 1971). The study suggests that self reported rioters in the Newark disorder were high on political efficacy but low on political trust. Paige notes that most measures of political efficacy blur these two dimensions. Applied to this study, it is possible that working class whites scoring high on political powerlessness feel equally a distrust of government as well as an inability to influence it. Although the distinction between efficacy and trust is important, the problem is to develop a measure of efficacy/powerlessness that is not contaminated by distrust. Thus in Paige's study, to avoid the distrust element, political efficacy was operationally defined as "political information" and finally measured by the (black) respondent's ability to identify the race of nine black and white officials. Obviously, there is a great gap between the nominal definition of efficacy and the final measure of racial information. The problems of developing a measure of system powerlessness free of system distrust are so great that one seems required to choose between a certain amount of distrust "mixed in" or an index with a serious problem in internal validity. Although some distrust may be built into our measure of powerlessness, the items seem most directly concerned with a low expectancy of effecting government through normative action.

² When one treats a psychological variable (e.g., "respect for authority") as intervening between a social structural variable (e.g., occupational status) and another psychological variable (e.g., attitudes toward black demands), it is possible that the intervening variable is not historically or even logically prior but merely part of the same psychological outlook—in this case, a specific example of a more general orientation. However, in this analysis a more plausible interpretation is that the intervening attitudes result logically from the working class situation, have developed over a period of

the powerlessness explanation we are specifying which segments of the white working class will be most antagonistic toward student and black activists, i.e., using a "specification" logic. Stated differently powerlessness per se does not clearly lead to antagonism toward student and black activists (many supporters of student and black demands may feel alienated from establishment politics); but rather, it is the combination of the blue collar perception of distributive injustice and feelings of powerlessness that should prompt antagonism toward students and blacks.

HYPOTHESES

From the above discussion, we hypothesize that working class respondents compared to those higher in the socioeconomic structure will be more likely to report: (a) hostility toward student demonstrators, (b) opposition to the student demand for increased power in running the university, and (c) a feeling that blacks are pushing too hard and too fast for things they don't deserve. In each hypothesis we predict a negative linear relationship between socioeconomic status and antagonism toward student and black militants.

Using a partial correlation technique, we develop refinements of these basic hypotheses to test for the importance of the three above theoretical explanations.

In addition to the test of main hypotheses, two supplementary questions will be explored: will class position continue to predict hostility toward student and black demonstrators with (1) age held constant and (2) sex held constant? For example, have recent developments in generation consciousness and youth culture become so important that young adults will be more sympathetic to student and black protest regardless of socioeconomic position?

SAMPLE AND MEASUREMENT OF VARIABLES

Data for this study consist of the re-time, and logically precede current attitudes toward student and black protest. Note in addition, there is no direct contamination between the intervening and dependent variables, that is, no mention of students, blacks, or protest in the intervening variable items.

sponses of a probability sample of 477 Caucasian adults (twenty-one years or older) to a personal interview schedule. The sample was drawn from white areas of Los Angeles, primarily in the San Fernando Valley during the latter part of 1969 and the first few months in 1970. Not only was this a period of extreme campus unrest nationally, but on two campuses in or near the sample area, student demonstrators took control briefly of campus offices. The valley section of the Los Angeles Metropolitan Area contains a population of approximately one million. Although the sample is undoubtedly typical of some suburban areas across the country, a somewhat special characteristic of this territory should be noted. The San Fernando Valley is a large sprawling suburban region without the white, ethnic communities one finds in Newark, Chicago, Wisconsin, or New York. For example, there is no Italian or Polish neighborhood in the valley. Some evidence suggests that blue collar hostility toward demonstrators is apt to be especially acute when working class position is mixed with ethnic identification (Greeley, 1969). In such working class ethnic communities, common frustrations and interpretations of a demonstration may be easily shared and developed. Accordingly, any relationship that we find between socioeconomic position and hostility may exist even more so in working class ethnic communities elsewhere in the country.³

Though subjects were selected by a probability model (involving random areas, blocks, and block corners as interviewer starting points) the sample should be classified as "disproportional stratified" since blue collar areas were oversampled to provide

sufficient cases for testing the hypotheses.⁴ We controlled carefully for the respondent's sex. Approximately equal numbers of males and females (one respondent per household) were interviewed.⁵ Finally, it should be emphasized that the working class respondents in our sample are employed and in predominantly semi-skilled or highly skilled positions. That is, we are testing hypotheses dealing with the "organized" working class vs. strata above this group. There were too few lower blue collar respondents in our sample to view separately (80% craftsmen, foremen, and operatives vs. 20% unskilled).

Independent Variable. Socioeconomic status, the study's independent variable is indexed by occupation and education taken as separate measures. Though occupation and education are commonly combined into an index of SES, it seemed important in this paper to view their effects separately since they tap slightly different aspects of the working class anger thesis. For example, education seems more likely to be associated with intellectual flexibility and breadth of perspective. Occupation, at least in Marxian theory, would seem more likely to be associated with work-related hostilities and peer group communication. For example, blue collar workers may develop common interpretations of radical demonstrations because of their occupations, quite apart from their educational experiences.

The occupations were coded into an ordinal scale with blue collar lowest, business

³ Another possible sample bias is that only those who voted in the 1969 Yorty-Bradley mayoral election were included. This selection procedure could have produced a bias toward the best informed and most politically active residents. However, due to the extremely emotional issues in this election, (a take-over of city hall by black power extremists and a drastic decline in law and order) in my judgment the sample is not highly biased toward either the best informed or the most politically active. Unusually high voter turnout indicated that many normally politically apathetic persons did vote in this election.

⁴ The decision to sample primarily from the San Fernando Valley was based on the need for a predominantly white area with widely differing socioeconomic levels within the Los Angeles city limits (city residents were needed to study the Yorty-Bradley mayoral election, another aspect of the project). As a result, integrated racial areas such as Crenshaw-Baldwin Hills district as well as areas of white concentration outside the city limits were excluded from the sample design. The San Fernando Valley was divided into 115 units of equal geographical size. Twenty areas from the Eagle Rock section of Los Angeles (not a part of the Valley) were also included. Random areas were selected as well as random blocks within these areas. Approximately eleven interviews were conducted in each selected area (fifteen to twenty interviews in blue collar areas).

⁵ Since ninety percent of the females did not have full-time occupations outside the home, most were classified by their husband's occupation. Their education level, however, is their own.

white collar (clerical, sales and managerial) intermediate, and professional white collar high. Professionals were ranked above "business white collar" because so few high ranking executives and major business owners are in the sample compared to the more uniformly high status for the professionals. For example, using the Bogue scale (Bogue, 1963) which ranks occupations in status from 44 to 180, the mean score for the blue collar group was 92.5, for business white collar 116.1 and for professionals 137.1.

Dependent Variables. Hostility toward student demonstrators is measured by the following three items (marginal percentages in parenthesis):

1. Even if they don't break the law, college students who are involved in demonstrations should be expelled (Agree 36% Disagree 64%).
2. College students who break the law during campus demonstrations should be given the strongest punishment possible by law (Agree 70% Disagree 30%).
3. College students who engage in illegal demonstrations deserve to be beaten by the police (Agree 13% Disagree 81% DK 6%).

On finding a fairly high association among the items (Gammas ranging from .60 to .93) a trichotomized index was constructed. Those agreeing with two or three of the items were called "high" in hostility toward student demonstrators; those agreeing with one were called "medium" and those disagreeing with all were called "low."

The following single item was used to measure feelings toward the issue of student power:

Students should be given more say in running the colleges (Agree 47% Disagree 53%).

Antagonism toward black protest was measured by the following items:

1. Negroes deserve the things they are asking for (Agree 60% Disagree 40%).
2. Negroes are asking for special treatment from whites to which they are not entitled (Agree 67% Disagree 33%).
3. Negroes are pushing harder and faster than they have a right to (Agree 48% Disagree 52%).

With high association among the items (Gammas ranging from .51 to .79) a trichotomized index was formed with the same cutting points as the student hostility score.

Intervening and Specification Variables.

Respect for authority was measured by four F scale items that seemed most directly related to obedience to authority and conformity. An example is the item: "The most important thing to teach children is absolute obedience to their parents" (Agree 63% Disagree 37%).

Belief in the American Dream and neglect of working man's needs required two separate measures. "Belief in the American Dream" was measured by a two item index⁶ (example: "If anyone is poor in this country today he has only himself to blame." Agree 32% Disagree 68%); while "neglect of workingman's needs" was captured by a single item ("There's more concern today for the 'welfare bum' who doesn't want to work than for the hard working person struggling to make a living." Agree 74% Disagree 26%).

Political powerlessness was measured by a score consisting of three forced choice items.⁷ Two of the items were taken from a scale developed by Neal and Seeman (1964), the third from a scale developed by Olsen (1969). Two examples are: "I don't think public officials care much what people like me think." (25%) versus "People like me can have an influence on what the government does." (75%); and "The average citizen can have an influence on government decisions." (69%) versus "This world is run by the few people in power and there's not much the little guy can do about it." (31%).

Note that these are somewhat rough indices of attitude variables with few items per index and a minimum demonstration of unidimensionality. As a result, the findings must be regarded as less than conclusive.

FINDINGS

Is the working class person more hostile toward student and black demands than those higher in the socioeconomic structure?

⁶ Gamma = .63 for the two items.

⁷ Gamma intercorrelation = .76, .71, and .52.

Table 1. Independent Variables (Occupation and Education) by Student Demonstrator Hostility, Student Power, and Black Demands Unjustified.

	Student Demonstrator Hostility %				Opposition to Student Power %			Black Demands Unjustified %			
Occupation	Low	Medium	High	N	Low	High	N	Low	Medium	High	N
Blue collar	12	37	50	(163)	34	66	(164)	12	18	70	(164)
Business white collar*	26	44	30	(159)	52	48	(162)	21	33	46	(155)
Professional white collar	34	41	25	(137)	50	50	(139)	29	31	40	(131)
	Gamma = -.32 p < .001				Gamma = -.21 p < .01			Gamma = -.34 p < .001			
<u>Education</u>											
Less than high school	3	47	50	(60)	27	73	(63)	8	15	77	(60)
High school graduate	15	40	45	(149)	42	58	(149)	17	24	59	(150)
Some college	35	36	29	(152)	49	51	(153)	22	31	47	(146)
College graduate	36	46	18	(104)	60	40	(106)	37	32	31	(99)
	Gamma = -.37 p < .001				Gamma = -.30 p < .001			Gamma = -.37 p < .001			

* refers to clerical, managerial and sales.

Note: Statistical significance determined by chi square.

Table 1 shows that this is indeed the case, with occupation moderately correlated with each dependent variable and education showing a stronger relationship. For example, 50% of the blue collar workers score high on student hostility, compared to only 25% of professionals. Education is an even more powerful independent variable, ranging from 50 to 18%. About half the blue collar workers and persons with a high school education or less agree with at least two of the very punitive student hostility items. The most striking relationship in Table 1 is the association between education and the belief that black demands are unjustified. Seventy-seven percent of those with less than high school feel blacks are pushing too hard as opposed to 31% of the college graduates.

Though it was considered essential to differentiate between occupational and educational status in this analysis, it also seemed important to assess the impact of each with the other held constant. Overall, partial correlation analysis (linearity is assumed in each relationship) reveals that education more strongly determines antagonism toward student and black militants than does occupation.⁸ However, these general correlations

may be masking important subgroup comparisons. Accordingly, Table 2 presents the joint effects of occupation and education on antagonism in cross tabular form. For ease of presentation, only the "high antagonism" end of the dependent variables are shown (the total linear relationships are captured by the gammas). Unfortunately, it was not possible to have the entire educational range for each occupation. Understandably, there were too few blue collar-college graduates

education is .49 in these data. The r between occupation and "student demonstrator hostility" is $-.20$ $p < .01$; with education controlled $r = -.08$ N.S. In contrast, the r between education and "student demonstrator hostility" is $-.31$ $p < .01$; with occupation controlled $r = -.24$ $p < .01$. The r between occupation and "opposition to student power" is $-.14$ $p < .01$; with education controlled $r = -.04$ N.S. In contrast, the r between education and "opposition to student power" is $-.24$ $p < .01$; with occupation controlled, $r = -.19$ $p < .01$. The r between occupation and "black demands unjustified" is $-.25$ $p < .01$; with education controlled $r = -.12$ $p < .05$. In comparison, the r between education and "black demands unjustified" is $-.30$ $p < .01$; with occupation controlled $r = -.19$ $p < .01$. This analysis (showing education to be the more powerful independent variable) must be viewed as suggestive rather than conclusive. For a discussion of the problems in assessing the independent effects of correlated variables, see Blalock (1964).

⁸ The Pearsonian r between occupation and edu-

and white collar-less-than-high-school persons to compute percentages for these subgroups. Nevertheless, within each occupational group are three meaningful education levels that allow for important comparisons. On the issue of "student demonstrator hostility," Table 2 shows that about half the blue collar persons are highly antagonistic regardless of educational achievement. However, for the white collar groups (business, professional) education has much more impact on hostility toward student demonstrators. One possible interpretation is that the "blue collar situation," explicated in the three theoretical statements, overrides any effects of education. Conversely, many white

collar workers are somewhat more removed from this situation (of perceived powerlessness, distributive injustice, neglect of workingman etc.) so that an individual's education may have far greater effects. A less pronounced but similar pattern—homogeneity of outlook in the blue collar category versus large differences in the white collar group—can be seen with "opposition to student power" and "black demands unjustified." The one exception is the uniquely antagonistic response toward black demands of blue collar-less-than-high-school persons. Table 2 also reveals that hostile attitudes toward students and blacks are not unique to the blue collar group. They are shared by less

Table 2. Percent Scoring "High" on Student Demonstrator Hostility, Opposition to Student Power, and Black Demands Unjustified, by Education with Occupation Controlled.

Education	% High Student Hostility N		% High Opposition to Student Power N		% High Black Demands Unjustified N	
	<u>Blue Collar</u>					
Less than high school	54	(48)	74	(50)	81	(48)
High school graduate	48	(62)	61	(61)	66	(65)
Some college	51	(37)	62	(37)	61	(36)
College graduate	--	--	--	--	--	--
	Gamma = -.15 N.S.		-.17 N.S.		-.25 p < .20	
	<u>Business White Collar</u>					
Less than high school	--	--	--	--	--	--
High school graduate	48	(56)	51	(57)	54	(56)
Some college	19	(62)	50	(62)	46	(59)
College graduate	16	(32)	33	(33)	29	(31)
	Gamma = -.38 p < .02		-.23 p < .15		-.20 p < .15	
	<u>Professional White Collar</u>					
Less than high school	--	--	--	--	--	--
High school graduate	35	(31)	65	(29)	59	(27)
Some college	27	(41)	50	(42)	40	(40)
College graduate	18	(65)	41	(66)	31	(62)
	Gamma = -.30 p < .06		-.53 p < .05		-.38 p < .02	

Note: Statistical significance determined by chi square.

well educated business and professional persons, in particular high school graduates. For example, professional persons with a high school degree show approximately the same proportion opposed to student power as blue collar workers. Similarly business white collar persons with a high school degree have the same proportion antagonistic toward student demonstrators as blue collar persons.

Our next task is to find if these relation-

ships between socioeconomic status and antagonism operate for both the males and the females in the sample. The three theories for working class anger (conformity, American Dream-worker neglect, powerlessness) seem to apply most directly to the blue collar male. However, as co-managers of the household, blue collar wives would probably share some of the same frustrations as their husbands. Though the overwhelming per-

Table 3. Percent Scoring "High" on Student Demonstrator Hostility, Opposition to Student Power, and Black Demands Unjustified, by the Independent Variables (Occupation, Education), with Sex Controlled.

	% High Student Hostility N		% High Opposition to Student Power N		% High Black Demands Unjustified N	
<u>Occupation</u>			<u>Males</u>			
Blue collar	57	(70)	67	(70)	81	(70)
Business white collar	30	(79)	54	(81)	52	(79)
Professional white collar	23	(61)	48	(62)	37	(59)
	Gamma = -.40 p < .001		Gamma = -.24 p < .20		Gamma = -.49 p < .001	
<u>Education</u>						
Less than high school	57	(28)	79	(29)	82	(27)
High school graduate	50	(52)	52	(50)	69	(52)
Some college	34	(70)	57	(70)	56	(69)
College graduate	19	(69)	45	(71)	35	(68)
	Gamma = -.39 p < .001		Gamma = -.25 p < .02		Gamma = -.44 p < .001	
<u>Occupation</u>			<u>Females</u>			
Blue collar	43.5	(85)	64	(88)	61	(87)
Business white collar	29	(80)	42	(81)	40	(76)
Professional white collar	20	(76)	51	(77)	42	(72)
	Gamma = -.25 p < .05		Gamma = -.18 p < .02		Gamma = -.23 p < .05	
<u>Education</u>						
Less than high school	44	(32)	67	(34)	73	(33)
High school graduate	42	(97)	61	(99)	54	(98)
Some college	24	(82)	46	(83)	39	(77)
College graduate	17	(35)	29	(35)	23	(31)
	Gamma = -.35 p < .001		Gamma = -.37 p < .001		Gamma = -.38 p < .001	

Note: Statistical significance determined by chi square.

Table 4. Correlation Coefficients (Pearsonian) Between Independent Variables and Intervening Variables and Between Intervening Variables and Dependent Variables.

A. Independent Variables	Intervening Variables		
	Respect for Authority	Belief in American Dream	Neglect Workingman's Needs
Occupation	-.26*	-.18*	-.21*
Education	-.37*	-.23*	-.22*

B. Intervening Variables	Dependent Variables		
	Student Demonstrator Hostility	Opposed to Student Power	Black Demands Unjustified
Respect for authority	.48*	.35*	.36*
Belief in American dream	.36*	.28*	.37*
Neglect workingman's needs	.44*	.33*	.47*

* = $p < .01$

Note: Statistical significant determined by student's T (one-tailed) test.

centage of women sampled were not employed outside the home, it seems logical to expect that they would resemble their husbands in outlook toward students and blacks. Table 3's data suggest that the association between occupation and student-black antagonism operates for both males and females, but is consistently stronger for males. This is especially true for the correlation between occupation and "black demands unjustified." The relationship is twice as strong for males as for females, with blue collar males being the most antagonistic toward black demands. We can speculate that blue collar males are unusually hostile because they are hit most directly by the work world frustrations defined in the three theories. However, when education is the independent variable, the relationships are more nearly equal in strength for males and females. Education probably reflects slightly different personal characteristics than does occupation, such as intellectual breadth and flexibility. Perhaps this dimension is more uniform for males and females regardless of employment status.

People in blue collar jobs and those with less than high school graduation are clearly more antagonistic. But are the theoretical reasons we have advanced for blue collar anger correct? To what extent do respect for authority, belief in the American Dream, and belief that worker needs are neglected, interpret the relationship between SES and

student-black antagonism? One may first note (Table 4) that occupation and education correlate with each of the three intervening variables, and they in turn, correlate with antagonism toward students and blacks.⁹ However, our theory states that blue collar anger results from the combination of respect for authority, worker neglect and belief in the American Dream. Accordingly, we need a statistical design that will handle the combined effect of all three intervening variables. Table 5 presents partial correlations (Pearsonian) between socioeconomic status and the student-black antagonism variables with the intervening variables held constant singly and in combination.¹⁰ Our

⁹ In the relationships between occupation and the intervening variables there is, again, a definite sex pattern with occupation having a stronger effect for males than females. For example, the r between occupation and authoritarianism is $-.35$ $p < .001$ for the males and $-.18$ $p < .01$ for the females. For "Belief in the American Dream" the comparable r 's are $-.27$ $p < .001$ vs. $-.10$ $p < .06$. For "Neglect workingman's needs" the r for males is $-.35$ $p < .001$ vs. $-.09$ $p < .10$ for females.

¹⁰ For the correlation and partial correlation analysis (Tables 4 and 5) all attitude variables were "opened" to include four ranks. For example, the one item measure of "worker needs neglected" contained all four Likert levels (strongly agree, agree, disagree, strongly disagree) instead of the two rank (agree, disagree) version used in the cross-tabulation tables. For a discussion of the use of the product moment correlation coefficient with ordinal data, see Labovitz (1967).

expectation is that the strength of the relationship between SES and antagonism will diminish considerably with these intervening variables held constant. In all instances, the original zero order relationship drops appreciably with all three variables controlled. For example, the original r between education and student demonstrator hostility is a moderate $-.31$, but drops to $-.12$ with the three intervening variables simultaneously controlled. Table 5 also reveals that the control for respect for authority reduces the zero order correlation slightly more than the other intervening variables. However, since the intervening variable indices did not have the same number of items, one cannot conclude that respect for authority is a more important intervening link than the belief in the openness of the American system or the belief that workingman's needs are neglected. The only surviving relationships are those between education and black antagonism (partial $r = -.16$ $p < .01$) and occupation and black antagonism (partial $r = -.16$ $p < .01$). Apparently, there are other reasons for hostility that we have not fully measured, for example, a perceived threat from black economic progress.

Specification: Control for Powerlessness. Table 6 largely supports the hypothesis that working class persons who feel politically powerless will be especially antagonistic toward the student and black movements. Consistently, SES is more strongly associated with antagonism toward rebels for the "highs" in powerlessness than for the "lows" in powerlessness (though some differences are small). Further, the blue collar group and the less-than-high-school group scoring high in powerlessness are more antagonistic toward students and blacks than anyone else in the sample. The relationship least affected by this control is one between education and black demands unjustified. Perhaps the least skilled perceive themselves threatened directly by black economic advancement, and this holds whatever their feelings of political powerlessness.

Control for Age. Are younger working class respondents more sympathetic with black and student demands? Has the youth movement invaded the socioeconomic structure to the extent that the outlooks of younger "working class" respondents will be similar to the outlooks of younger "middle class"

Table 5. Partial Correlations Between Independent Variables (Occupation and Education) and the Dependent Variables with Respect for Authority, Belief in American Dream, and Neglect of Workingman, Controlled Singly and Jointly.

Correlation Between Occupation and:	Zero Order	(1) Respect for Authority Controlled	(2) American Dream Controlled	(3) Worker Neglected Controlled	Simultaneous Control 1, 2, and 3
Student demonstrator hostility	-.20*	-.09	-.17*	-.14*	-.07
Opposed to student power	-.14*	-.06	-.12*	-.09	-.04
Black demands unjustified	-.25*	-.18*	-.23*	-.19*	-.16*
Correlation Between Education and:					
Student demonstrator hostility	-.31*	-.16*	-.25*	-.24*	-.12*
Opposed to student power	-.24*	-.12*	-.18*	-.18*	-.09
Black demands unjustified	-.32*	-.20*	-.24*	-.23*	-.16*

* = $p < .01$

Note: Statistical significance determined by student's T (one-tailed test).

Table 6. Percent Scoring "High" on Student Demonstrator Hostility, Opposition to Student Power, and Black Demands Unjustified, by the Independent Variables (Occupation, Education), with Powerlessness Controlled.

	% High Student Hostility N		% High Opposition to Student Power N		% High Black Demands Unjustified N	
<u>Occupation</u>			<u>Low Powerlessness</u>			
Blue collar	43	(79)	60	(80)	65	(79)
Business white collar	30	(101)	44	(104)	42	(100)
Professional white collar	23	(101)	53	(102)	36	(94)
	Gamma = -.29 p < .01		Gamma = -.07 N.S.		Gamma = -.29 p < .01	
<u>Education</u>						
Less than high school	36	(25)	67	(27)	75	(24)
High school graduate	41	(92)	55	(94)	56	(93)
Some college	30	(93)	50	(99)	43	(94)
College graduate	18	(77)	43	(77)	32	(72)
	Gamma = -.32 p < .001		Gamma = -.20 N.S.		Gamma = -.32 p < .01	
<u>Occupation</u>			<u>High Powerlessness</u>			
Blue collar	60	(71)	72	(71)	75	(73)
Business white collar	28	(57)	54	(57)	54	(54)
Professional white collar	30	(36)	40	(37)	43	(37)
	Gamma = -.36 p < .01		Gamma = -.41 p < .01		Gamma = -.38 p < .01	
<u>Education</u>						
Less than high school	59	(34)	77	(35)	77	(35)
High school graduate	50	(56)	61	(54)	66	(56)
Some college	28	(53)	55	(53)	57	(51)
College graduate	19	(26)	32	(28)	30	(27)
	Gamma = -.42 p < .001		Gamma = -.40 p < .01		Gamma = -.40 p < .01	

Note: Statistical significance determined by chi square.

respondents? For brevity, the data will be described rather than shown in table form. Occupation and education persist in predicting student power and black demands unjustified with three age groups held constant (21-30, 31-45, and over 45). But among the young (21-30) occupation is only weakly associated with student hostility (Gamma = -.23 p = N.S.) and education washes out completely as a predictor of student hostility (Gamma = -.05, p = N.S.). In other words, younger "working class" respondents feel

much the same as younger "middle class" respondents on the question of punitive treatment toward student protestors: both groups tend to be non-punitive. But on the question of student power and black demands, the younger blue collar workers are just as antagonistic as older blue collar workers. In fact, the greatest polarization by class occurs within the young group. For example, 76% of young blue collar workers feel black demands are unjustified vs. only 29% of young white collar professionals (Gamma = 50, p <

.02). On the issues of student power and black demands; young blue collar workers and young well educated professionals are worlds apart in outlook.

DISCUSSION AND CONCLUSIONS

The thesis that white working class people are highly antagonistic toward students and blacks demanding massive changes receives moderate support from this analysis. We stress the term "moderate support." Though there is a definite socioeconomic differential (especially for males and those scoring high in political powerlessness), fairly sizeable segments of all educational and occupation strata have reacted with anger to protest of the 1960's. Still, blue collar workers are found to be more uniformly high in antagonism toward student and black protest regardless of educational level; whereas, white collar workers display more heterogeneity of outlook according to educational achievement. This suggests that merely acquiring education will not substantially reduce the blue collar worker's level of hostility toward militants.

Not only are the major relationships between SES and antagonism supported but, given the limitations of indices with only a few items, we find some confirmation for each of the three theoretical explanations (respect for authority, American Dream-worker needs neglected, and political powerlessness.) However, the partial correlation table indicates that the explanations do not fully interpret the correlations between SES and "black demands unjustified." Further research is needed to locate other rationales for blue collar anger and to test for their relative effects.

An especially interesting finding is that for two dependent variables (student power and black demands unjustified) the greatest class polarization was found among young adults. This finding supports Lipset's contention that the generation gap is grossly overstated and that the greatest differences in political outlook and behavior are within the youth group rather than between age cohorts (Lipset, 1970:370-372).

The mass media have probably tended to overstereotype the white working man as a

narrow-minded, intolerant bigot. His angry responses to students and blacks are often viewed as the effect of a prejudiced personality. In contrast, our model shows blue collar anger to be rooted in the actual or perceived social situation. From this perspective, much of the workingman's anger is a rational response to tangible strains, independent of personal bigotry. A lack of decision making power on the job, the fact of hard earned dollars going for tax programs to aid blacks with no comparable programs for working class whites, a power structure unresponsive to the needs of the workingman—these stresses probably effect working class anger as much as personal prejudice.

In addition, white working class people who feel politically powerless are especially antagonistic toward student and black protestors. Research in the black community conducted shortly after the Watts riot (Ransford 1968) suggests a parallel finding: blacks who scored high in social powerlessness (low expectancy of gaining redress through institutional channels) were more willing to use violence to get their rights. Similarly, a violence commission report (Skolnick, 1969) notes that militant white reactions to protest (armed citizens groups) are found especially among those who are angered by excessive concern for minorities and who feel ignored by the polity. Apparently the potential exists for black as well as white militant action when individuals perceive a distributive injustice and, in addition, feel blocked in gaining redress through institutional channels.

This situation is clearly described by what Lipset (1970:510) has termed the "New American Dilemma," viz., how to square the American promise with lower class ghetto life for blacks, and at the same time not withdraw the promise from the white working class. A recent statement by Pete Hamill summarizes this dilemma well.

The working class white man is actually in revolt against taxes, joyless work, the double standards and short memories of professional politicians, hypocrisy, and what he considers the debasement of the American Dream. . . . Any politician who leaves the white man out of his political equation does so at very large risk. The next round of race riots might not be between people and property but

between people and people. And that could be the end of us (Hamill, 1969).

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ILLNESS AND THE LEGITIMATION OF FAILURE *

STEPHEN COLE

State University of New York at Stony Brook

ROBERT LEJEUNE

Herbert H. Lehman College

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In achievement oriented societies one may use illness to justify failure to fulfill socially prescribed role obligations. This hypothesis is tested on two samples of welfare mothers and one sample of working class black women. Many welfare mothers accept the dominant cultural view that being on welfare is a result of personal failure. When women accept this view and give up hope of getting off welfare, they are prone to adopt the sick role to legitimize their self-defined failure. Women who can no longer justify their dependency by the presence of pre-school children are particularly likely to make this adjustment. We conclude that the sick role may offer "substitute" status for one who lacks other socially approved statuses. Furthermore, we show that women who viewed themselves as inadequate wives and mothers were more likely to define their health as poor than those satisfied with their performance in these roles.

ILLNESS exempts individuals from their normal role obligations. Most of us are sick on occasion. Some, however, see themselves as permanently sick. Generally, we associate this adjustment with a serious physiological or psychological breakdown; rarely do we view sociological variables as prompting chronic illness. It is the central hypothesis of this paper that people who come to view themselves as unable to fulfill their normal roles will be motivated to define themselves as permanently sick to legitimize their self-defined failure.¹

Sociologically, a person is sick when he acts sick. Thus, the conditions that motivate people to view themselves as sick or healthy are important. Clearly, the "objective" physiological condition of one's body plays a major part in self-definition of health. Both medical doctors and social scientists, however, have become increasingly aware that physiological and social-psychological forces

intertwine in determining objective and subjective states of health. (Crandell and Dohrenwend, 1967; Hinkle, et al., 1956; Mechanic and Volkart, 1961; Wolff, 1958; Zola, 1966) In this paper, we shall discuss one social-psychological determinant of one's subjective health definition. Our data treat the hypothesis that inability to fulfill their role obligations is one factor leading people to define their health as poor. To explore this hypothesis, we shall examine how definition of health relates to a mother's adjustment to being on welfare. We shall also analyze the relationship between definition of health and performance of roles as wives and mothers among a sample of working-class black women.

The data derive from three studies: The first was conducted in New York City in the summer of 1966. The National Opinion Research Center interviewed a probability sample of 2,179 female heads of households receiving family public assistance in New York City in April, 1966.² The second study was conducted in New York City in the summer of 1968. National Analysts interviewed a quota sample of 412 working class black mothers living in public housing proj-

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¹ Two decades ago, Parsons, following the psychoanalytic model, noted that sick people sometimes become unconsciously motivated to retain the "privileges and exemptions of the sick role." (Parsons, 1951:437) In a subsequent examination of the problem, Parsons hypothesized that the high level of achievement demanded in America might accentuate the unconscious desire to use ill health to exempt oneself from role obligations (Parsons, 1958).

² For a detailed description of this sample see Richard Pomeroy in collaboration with Robert Lejeune and Lawrence Podell, *Studies in the Use of Health Services by Families on Welfare*, The Center for the Study of Urban Problems, Baruch College, The City University of New York, 1969; also Robert Lejeune, *Illness Behavior Among the Urban Poor*, unpublished doctoral dissertation, Columbia University, 1968.

ects.³ The final study was conducted in Camden, New Jersey, in the summer of 1969. The Center for Research on the Acts of Man, University of Pennsylvania, interviewed a quota sample of 447 women recipients of Aid to Dependent Children.⁴

The dependent variable in this study is one's subjective health definition. We asked: "In general, would you say your own health is excellent, good, fair, or poor?" A major problem in the analysis is empirically separating the physiological and social psychological components of health definition. If people fail to fulfill their role obligations because they are physiologically ill, it would make little sense to say they are defining themselves as sick to legitimize failure. In clear-cut cases of physical breakdown, such as terminal cancer, heart attacks, or tuberculosis, almost everyone will act ill. However, since practically all respondents in the three surveys are women under fifty, and most are under forty, the frequency of such illness among the respondents would not be high. In less than clear-cut cases, individuals having similar physiological conditions may behave quite differently. Most people have some health problems and some aches and pains; yet they continue to function adequately. Others magnify their aches and pains until they dominate their lives.

Even if we assume that a small minority suffered from clear-cut physiological breakdown, we are still faced with the problem of causal order. Without longitudinal data we do not know if taking the sick role proceeds or precedes a self-definition of failure. Hence, our study is exploratory rather than conclusive. The best we can do to handle the problem of causal order is to show that the relationship between self-defined failure and health persists even when we control for a few relatively more objective indicators of health such as symptoms, number of

doctor-patient contacts, and number of reported illnesses. Our analysis begins with a discussion of the meaning of being on welfare in America.

Welfare as Failure

One reason why the United States has had a weak socialist movement and a relatively conservative working class is that Americans typically see social status as a result of personal success or failure. If someone is socially mobile, it is because he is talented and works hard. If someone is not socially mobile, it is thought to be a result of his personal shortcomings. The individual's location in the social structure is not viewed as a prime determinant of his fate. It is for this reason that many Americans look down on welfare recipients. Many welfare recipients themselves believe that being on welfare signifies personal rather than societal failure.⁵

What evidence is there that welfare recipients accept the stigma attached to their status by the dominant culture? Most welfare recipients have been repeatedly exposed to the view that they are immoral and undeserving. The data indicate that a large number of AFDC mothers share this view. Seventy-one percent of the New York welfare sample agreed with the statement, "A lot of people getting money from welfare don't deserve it." Another ten percent did not know or would not reply and only 19 percent disagreed.⁶ If it is objected that some welfare mothers merely wanted to give the "expected" or "right" answer, this confirms our point: that welfare recipients know that the dominant culture defines their status as illegitimate. While we cannot tell from these replies to what extent and with what intensity welfare mothers view themselves as undeserving, they are very likely aware of the stigma attached to their status and

³ For a detailed description of this sample see Lawrence Podell and Richard Pomeroy, *Studies in the Use of Health Services by Families on Welfare: Special Population Comparisons*, The Center for the Study of Urban Problems, Baruch College, The City University of New York, 1969.

⁴ For a detailed description of this sample see Samuel Z. Klausner, et al., *The Work Incentive Program: Making Adults Economically Independent*, Center for Research on the Acts of Man, Philadelphia, Pennsylvania, 1970.

⁵ As long as a low status group like welfare recipients blame themselves for their misfortune, there will be little pressure to change society; and organizations like the welfare rights groups will fail to attract mass support.

⁶ In their responses to this question, welfare mothers may, in part, be invidiously comparing their own financial status and that of other welfare mothers. Each respondent may think that she deserves welfare more.

probably prone to question the claims of need of other welfare recipients. Thus, it is not surprising that 55 percent of the New York welfare mothers agreed that "getting money from welfare makes a person feel ashamed." And fully 87 percent agreed that "people should be grateful for the money they get from welfare."

The Camden welfare mothers expressed similar views on receiving public assistance. Fifty-seven percent said it was more often true than false that "people I know look down on welfare," and 34 percent admitted that "there are times when I have been embarrassed in front of my family or friends because of being on welfare." Another indicator of whether or not the welfare mothers think of being on welfare as a type of personal failure is provided by their opinions about the conditions under which welfare should be available. For example, 84 percent of the Camden mothers believed that welfare should not be available "if there is one parent (female) and she does not try to keep up her home." We can conclude that at least a substantial proportion of mothers on welfare accept the stigma attached to the status by the general culture. These data indicate that a majority of welfare recipients do not share the attitudes of welfare rights organizations. We wish to make explicit at this point that we do not consider welfare mothers to be failures; rather this is the view of many Americans and the data show that many welfare mothers internalize this view.

How do people who define themselves as failures cope with the knowledge of their stigma? One technique of handling this problem is to believe that things will be better in the future. Welfare clients may reduce the conflict between their need for welfare and their perception of their status as an undesirable and degrading one by believing in a welfare-free future for themselves. Only 25 percent of the New York women thought they would "surely be on welfare" and another thirty-three percent thought they would "probably be on welfare" a year from the time of the interview. Forty-two percent either did not know or thought they would not be on welfare.⁷ Sixty-one percent said

yes to the question: "Do you think you will ever (again) work for pay?" While work is not their only way to economic independence, it is certainly the most important for the majority who are husbandless or whose husbands' earnings cannot support the family. Given the poor objective chances for most welfare mothers to succeed in an economy demanding ever higher levels of training, we would expect most of them to abandon hope. That the opposite is true attests to the continued strength of the dominant goal of success. Seventy percent of both the New York and Camden welfare samples say they would rather work than remain home.⁸

The Legitimation of Dependency

A woman who sees being on welfare as a personal failure but views her own welfare status as temporary may feel no need to justify her dependency; but a woman who has abandoned hope of escape from this position of self-defined failure will probably have a strong need to justify it. Thus, we hypothesize that mothers who no longer think of welfare as a temporary status will be the most likely to use illness to justify their self-defined social failure. We do not argue that welfare mothers consciously use poor health as an excuse for remaining on welfare. Although some may do this, we do not think them typical. More probably, a woman who feels the need to justify her status to herself, gradually adopts the sick role. Over time, she comes to feel and act as if she were in fact sick.⁹ She is likely to develop a series of psychosomatic symptoms.

Margaret Olendzki (1965), who studied

be on welfare, probably not be on welfare?" As it turned out, one year from the time of the interview, 89% of the respondents were still receiving public assistance in New York City. Furthermore, it is likely that the majority who left the rolls in the one year period—judging from case histories—will be back on again.

⁸ For an analysis of the determinants of work expectations see Stephen Cole and Robert Lejeune, "Illness, Welfare Retreatism and the Legitimation of Failure," project report, Center for Research on the Acts of Man, Philadelphia, Pennsylvania. The key variables are education, previous job experience, race, intelligence, "objective health," and age.

⁹ It is also possible that the poverty which attends welfare life may contribute to physiological deterioration.

⁷ The question was: "Do you think that a year from now you will *surely be* on welfare, *probably*

1,976 applicants for public assistance in Manhattan collected data to support this view.¹⁰ As part of that study, a check-list of medical conditions was presented to each applicant. The most often mentioned health complaint was "nerves," reported by 45 percent of the women respondents who were heads of families and thus correspond most closely to the respondents in our samples. We cross-tabulated the "suffering from nerves" complaint with Olendzki's question on self-defined health. It turns out that defining one's health as poor is highly associated with the presence of emotional complaints. Sixty-nine percent who said their health was poor also said they "suffered from nerves." Sixty-one percent who viewed their health as fair made the same complaint. The proportion who said they were in good and excellent health yet suffered from nerves was smaller, though not unsubstantial: 35 percent and 27 percent respectively.¹¹ We conclude that welfare mothers who claim to be sick are probably so; however, socio-psychological factors may play at least as great a role as physiological factors in the development of their illness.

We can now present the data bearing on our main hypothesis—that women who have abandoned hope of getting off welfare will be more likely than those who maintain hope to define their health as poor. The New York survey showed that expectation not to work was correlated ($r = .22$) with definition of health.¹² Women who did not expect to

work were more likely to define their health as poor. This correlation would be spurious if it disappeared when we controlled for background variables affecting work expectations such as education, age, and a more objective indicator of health. This does not happen. When we remove the variance due to education, the partial correlation of work expectations and definition of health is $p/r = .18$. Similarly, removing the variance due to age only slightly reduced the relationship between work expectation and definition of health ($p/r = .17$). Though we have no adequate measure of objective health, we did have data on the number of times the respondent reported she was ill during the preceding year. This health indicator is also a subjective measure. It is far from perfectly correlated with definition of health ($r = .49$) and thus probably measures at least a different aspect of health conception. When the variance due to this variable was removed, the correlation between work expectation and health definition was hardly changed ($p/r = .20$). Tabular presentation of the data indicates that in each category of number of illnesses, women who do not expect to work in the future are more likely than those who do to define their health as fair or poor.

The Camden survey had similar results. Here we used two questions to measure welfare mothers' attitudes toward their health. Women not currently working were asked why. One choice was that they were "not interested in working at present time—health reason or handicap." Women who did not expect to work in the future were more likely to give this reason ($r = .27$).¹³ We also asked: "What part does your health play in your decision not to work or in the kind of work you can take?" Women who did not expect to work were more likely to say that health played a part in their decision ($r = .20$).

The Camden data allow us to test several hypotheses concerning the conditions under

¹⁰ We thank Dr. Olendzki for making her data available.

¹¹ Suffering from nerves was highly correlated with other psychosomatic symptoms such as:

"Do you have trouble sleeping?"

"Are you continually troubled by aches and pains?"

"Do you feel tired all the time for no special reason?"

"Have you recently been very depressed and blue?"

¹² The question was worded: "Do you think you will ever again work for pay?" Here, for simplicity of presentation we have used correlation coefficients rather than tables. We do this despite the fact that at times the data do not meet all the assumptions necessary for statistical analysis. None of our variables explained a great deal of variance. It is not the size of the associations but their consistency that gives us confidence in the plausibility of our conclusions. The hypotheses were

developed on the basis of the New York welfare sample and we were able to test them on the other two surveys.

¹³ The question was worded: "In the near future (whether or not you are working now) would you prefer a full time job or would you prefer to stay at home?"

which welfare mothers are likely to use poor health to legitimize their dependency. We have suggested that welfare mothers view being on welfare as a type of failure and that when they give up hope of leaving welfare they develop a need to justify their permanent dependency. Some welfare recipients, however, do not accept the stigma generally attached to their position. We would expect to find that among women who do not view welfare as a temporary status, those who accept the stigma should be more likely to take the sick role. As an indicator of attitudes towards welfare, we used the score on an index of questions concerning the conditions under which welfare should be available.¹⁴ Those who believe that welfare should not be available unless the recipient lives up to rigorous moral standards are more likely to define welfare as an illegitimate status. As Table 1 indicates, among mothers who expect to get off welfare, attitudes towards welfare have little effect on adopting the sick role. Among mothers not expecting to work, however, those who accept the dominant cultural view of welfare (in the table those who have "strict" attitudes towards the availability of welfare) are more likely than those who reject it (in the table those who have "lenient" attitudes towards the availability of welfare) to adopt the sick role. Clearly, both conditions, defining one's status as illegitimate and viewing that status as permanent, contribute to one's adopting the sick role.

A counter theory to explain the data of Table 1 would be that poor health leads a

¹⁴ The index was composed of the following questions: "Assuming jobs are available, do you think that welfare should be available to families in the following situations? a) If there are a lot of children and the parent cannot support them adequately. b) If the parent is able bodied and will only work if she can get the type of work desired. c) If the parent is able bodied and simply does not feel like working. d) If there is one parent (female) and she does not want to work (for any reason). e) If there is one parent (female) and she feels that the mother's place is at home. f) If there is one parent (female) and she does not try to keep up her home." Those women who said that welfare should *not* be available in five or six of these situations are classified as having "strict" attitudes towards its availability. Those saying that welfare should *not* be available in four or less of these situations were classified as having "lenient" attitudes toward its availability.

Table 1. Percentage Giving Health as Reason for Preferring Not to Work by Attitudes Toward Welfare and Expectation to Work in Future.
(Camden Welfare Sample)

		Availability of Welfare	
		Lenient	Strict
Expect to be working in the future	Yes	10 (106)	12 (137)
	No	15 (88)	26 (94)

woman to give up hope of working. Collapsing Table 1 we find that 42 percent of people with poor health and 60 percent of those without poor health, expect to work. While poor health may lead women to give up hope of working, this does not explain why women who accept the dominant view of welfare are more likely to view their health as poor than those who reject that view.¹⁵

Family Cycle, Age and Legitimation of Dependency

Welfare mothers can use mechanisms other than illness to legitimize failure. One condition influencing the type of mechanism adopted is the family-life cycle. AFDC's primary rationale is that women without husbands should be supported so they can stay home and care for their children. A woman with a pre-school child can easily legitimize her dependency. Indeed, 77 percent of the New York women who preferred not to work gave child care as a reason. It is initially surprising to find that only 19 percent of those who prefer not to work gave illness as a reason. We must remember, however, that the respondents were women. Their primary obligations center around expressive family rather than instrumental work roles.¹⁶ We would therefore expect

¹⁵ Another plausible explanation for Table 1 would be that those who have lenient attitudes towards welfare become casual about their continuing dependence. In contrast, those who have strict attitudes are more reluctant to rely on welfare and tend to do so only when genuinely ill. We thank the ASR reader for pointing out this possibility.

¹⁶ Much ambivalence exists about the role of the working woman in America. Though women are sometimes expected to work, they are not

them to be more likely to define their health as poor when they no longer have pre-school children.

In both the New York and Camden samples the age of a mother's youngest child was associated with citing health as a reason for preferring not to work. (See Table 2) Hardly any mother with pre-school children,

Table 2. Percentage Giving Health Reason for Preferring Not to Work by Age of Youngest Child (only those preferring not to work).

Age of youngest child	N.Y. Welfare Sample		
	0-5	6-10	11 or older
	9 (401)	20 (143)	53 (120)
Age of youngest child	Camden Welfare Sample		
	Under 6	6 or older	
	14 (145)	42 (38)	

but a substantial proportion of those with school age children gave poor health as a reason for preferring not to work. Whereas the age of the youngest child is positively associated with giving poor health as a reason for preferring not to work; it is negatively associated with giving child care as a reason. Thus, of the Camden women not expecting to work, 72 percent with children under six and 42 percent with children six or over gave child care as their reason for preferring to stay at home. As long as a mother has young children she can view her dependency as an objective necessity.

A possible source of error in our interpretation of the data of Table 2 could be the correlation between the mother's age and the age of the youngest child. Women who have no pre-school children are generally older than those who do. The results in Table 2 might be an artifact of the "self-evident" fact that older people are more likely to be sick and "therefore" less employable. Actually "employability" is a culturally defined state. There are probably

expected to achieve too much. Whereas middle class women may be urged to stay home and tend their children, welfare mothers are "lazy boondogglers" if they don't work. On the cultural ambivalence towards working women see Zoser and Rokoff (1971).

few disabilities which restrict work under all circumstances and in all social contexts. The sociologist must therefore view the system's classification of some welfare recipients as "totally and permanently disabled" as providing a socially acceptable rationale for the limited opportunities such persons have in the labor market. As in many other areas of social life the labeling process functions to tidy up the bookkeeping of a society riddled with value inconsistencies. That those excluded from participation would seek these labels is a natural outcome of the process.

As a more conclusive test of the family cycle hypothesis it is necessary to see if the relationship between age of youngest child and definition of health persists with the mother's age controlled. (See Table 3) In Table 3, the age of the youngest child is used as an indicator of the extent of child-rearing responsibilities. It turns out that even with mother's age controlled, the age of the youngest child is associated in the expected direction with giving health as a reason for preferring not to work. The association is particularly strong for those over forty. Women over forty with no children under the age of eleven are almost three times more likely than any other group to give health reasons for preferring not to work. The decline of child-rearing functions, as one aspect of aging, leads to an increasing tendency to legitimize welfare dependency by evoking the sick role. The issue here is not whether older respondents are "sicker" than younger ones. Even if they were, only among those whose children are growing up

Table 3. Percentage Giving Health Reasons for Preferring Not to Work by Age and Age of Youngest Child (only those preferring not to work - New York Welfare Sample).

Age of youngest child	Age		
	Under 30	30-39	40 and Over
0 to 5	8 (202)	9 (165)	15 (34)
6 to 10	13 (16)	14 (64)	27 (63)
11 or over	* (2)	21 (24)	63 (94)

*Too few cases.

does poor health become salient as their reason for welfare dependency. We would guess that even middle-class women would be more likely to take the sick role as they age and their child-rearing function declines in significance. A middle-class woman who has devoted her life to rearing children might find poor health an acceptable way of legitimizing to herself her failure to find another socially acceptable status when her children have grown up.

Thus it is evident that though illness plays a small part in legitimizing dependency for the total family welfare population, it becomes one of the primary bases for legitimizing a claim to welfare support among women over forty whose youngest child has reached adolescence. As these women end their child-rearing years, their chance for economic independence is highly limited. They are over forty, lack a recent job history and have limited marketable skills. If at the same time they are black or Puerto Rican (as most are) their realistic opportunities for independence are further restricted. The sick role may provide one "substitute" status for the lack of any other socially approved or positively evaluated statuses.

In Table 3 it is evident that the mother's age has a strong independent effect on adopting the sick role. Is this predominantly the result of a genuine decline in health with age, or can the finding be explained sociologically? It is necessary to separate the physiological and social components of aging. Although this is impossible given our data, we can control for some relatively more objective indicators of health. In Table 4 the relation between age and giving poor health as a reason for preferring not to work is shown, with the number of reported illnesses in the past year held constant.¹⁷ Table 4 shows once again that with increasing age, welfare mothers are more likely to give poor health as a reason for preferring not to work. This association is found even among women who had reported earlier in the interview that they had not been sick

Table 4. Percentage Giving Health Reasons for Preferring Not to Work by Age and Number of Illnesses in the Past Year (only those preferring not to work - New York Welfare Sample).

Number of Illnesses in Past Year:	Age		
	Under 30	30-39	40 and Over
None	1 (86)	3 (93)	24 (63)
1 or 2	4 (52)	9 (75)	37 (51)
3 or more	18 (80)	21 (84)	62 (76)

at all in the past year. Thus, even though the number of reported illnesses in the past year may itself have been affected by subjective considerations, older respondents who by their own account had not been ill were nonetheless more likely than younger respondents to give poor health as a reason for preferring not to work.

Chronological age and number of illnesses have both independent and interactive effects on mentioning poor health as a reason for preferring not to work. Thus, among women under thirty, the number-of-illnesses variable accounts for a 17 percentage-point difference in the first column of Table 4. There is a similar difference (18 percentage points) in the thirty to thirty-nine age group. In the forty and over group, the difference is 38 percentage points, indicating that not only illness per se but more importantly illness combined with aging increases the likelihood that illness will become a basis for legitimizing dependency. Not only does taking the sick role legitimize being on welfare, but age legitimizes taking the sick role.¹⁸ A young person who complains of aches and pains calls forth negative sanctions; an older person calls forth sympathy.

In Table 4, one cannot distinguish between "serious" and "non-serious" illnesses by simply counting the number of self-reported illnesses for the year. It was beyond the scope of the study to measure how seriously ill the women were. This can be approximated, however, by the number of doctor-patient contacts in the past year. We may assume that the sicker respondents would

¹⁷ Though "the number of reported illnesses in the past year" is not an adequate measure of the "physiological component of aging," it is the best measure we have and provides at least a tentative test.

¹⁸ For a similar analysis see Coser (1961).

Table 5. Percentage Giving Health Reasons for Preferring Not to Work by Age and Number of Doctor-Patient Contacts in the Past Year (only those preferring not to work - N.Y. Welfare Sample).

Number of Doctor-Patient Contacts in Past Year	Age		
	Under 30	30-39	40 and Over
None or 1	5 (88)	7 (118)	30 (84)
2 to 6	4 (72)	13 (79)	38 (69)
7 or more	12 (60)	18 (57)	66 (53)

on the average contact physicians more often. Table 5 shows of those women with at least seven doctor-patient contacts in the last year, only 12 percent of the youngest and fully 66 percent of the oldest gave poor health as the reason for preferring not to work. Also, 30 percent of women over forty who saw the doctor once or not at all during the year made the poor health claim.

Generalization of the Hypothesis

Thus far we have presented data illustrating how some welfare mothers use illness to legitimize self-defined failure. We believe that this practice is a widespread phenomenon in achievement-oriented societies like ours.¹⁹ Health is perceived as physiologically determined and therefore basically beyond individual control. If one fails to fulfill a socially defined role expectation, he is not to be blamed if he has poor health.

We decided to test the validity of this generalization on another set of data. We were interested in whether or not mothers and wives who defined their own role performance as below par would be more likely to define their health as poor. As part of the New York study of welfare mothers, we interviewed a "control" sample of 412 non-welfare black mothers. These working class

women lived in several public housing projects. In this questionnaire were a series of questions designed to measure the degree of self-defined success which the women had as wives and mothers. The data are presented in Table 6. For all five questions, those women who defined themselves as relative failures in their roles as wives and mothers were more likely to define their health as fair or poor than women who felt they adequately performed these roles.²⁰ For example, of those women who judged their marriage "average or unhappy," 40 percent defined their health as fair or poor; but only 22 percent of those who judged their marriage "happy" defined their health negatively. Though we have no data to measure the intervening mechanisms, we would guess that many women who view themselves as failures as mothers and wives develop a need to legitimize their failure and a substantial minority use poor health to do this.

It is, of course, possible that the associations reported in Table 6 could be spurious. It could be that women who are older, have little education, low income, and are physiologically in poor health might be more likely to see themselves as failures as wives and mothers and define their health negatively. Were we to control for these variables and eliminate the effect of self-defined role performance, then our interpretation of Table 6 would be incorrect. To analyze this possibility we combined the answers to all five questions on role performance in an index. Women scoring high on this index (defining their role performance favorably) were least likely to define their health as fair or poor. Whereas 18 percent of the high scorers defined their health as fair or poor, 45 percent of the low scorers viewed their health as fair or poor (See Column 1 of Table 7). Age, education, income and objective health were correlated with scores on the role performance index and definition of health. The association between these latter variables

¹⁹ We do not mean that most people who define themselves as failures use ill health as a means of self-legitimation; but that a substantial minority use ill health to legitimize failure in a wide variety of situations. Clearly other techniques are used to legitimize failure. Detailed investigations of these are likely to be theoretically fruitful.

²⁰ One could interpret the data in this table to mean that all responses result from a general negative self image. Women who view themselves as bad mothers are also likely to have a low evaluation of their bodies and health. To further test our hypothesis would require data to show more specifically that women who define themselves as failures take the sick role in attitude and behavior.

Table 6. Percentage Defining Health as Fair or Poor by Self-Defined Role Performance (N.Y. Working Class Sample).

Compared with your friends would you say that you are an:		
Excellent cook	20	(54)
Good cook	31	(169)
Average or below cook	33	(187)
How often do you feel that you can't control your children:		
Frequently or sometimes	38	(125)
Rarely	34	(86)
Never	24	(200)
How often do you feel that you are not as good a mother as you would like to be:		
Frequently or sometimes	38	(155)
Rarely	29	(98)
Never	24	(159)
Would you say that your marriage has been:		
Happy	22	(214)
Average or unhappy	40	(132)
Compared with your friends would you say that you and your husband get along:		
Very well	23	(188)
About average or not so well	36	(160)

could therefore be spurious. To test this possibility we have used the technique of test-factor standardization.²¹ The results presented in Table 7 are equivalent to three-variable tables in which we examine the effect of role performance on definition of health, controlling separately for age, education, income, and objective health.

Standardizing separately for age, education, and income did not substantially reduce the effect of the role-performance index on definition of health. Thus, for example, although older women were likely to score low on the role-performance index and to define their health as fair or poor, within each age category role-performance was still associated with definition of health. The results obtained when we standardized on objective health were not as clear. Objective health was measured by the number of symptoms reported. The interviewer read a list of twenty-eight symptoms ranging from "Have you lost a lot of weight recently with-

out dieting?" to "Have you recently been very depressed and blue?" Those reporting no symptoms were considered to be in excellent health, one or two symptoms good health, three or four symptoms fair health, and five or more symptoms poor health. This measure of objective health is, of course, not an indicator of the respondent's physiological condition. Many of the symptoms were psychosomatic. Also, people who define their health as poor are more likely to search for symptoms to justify their taking the sick role. Furthermore, as we observed above, it is almost impossible to separate physiological from socio-psychological determinants of many physical symptoms. As the standardized data of Table 7 indicate (see last column of Table 7), the objective health measure does reduce the relationship between role performance and self-defined health. When the data are standardized on objective health, 35 percent of mothers scoring low on the role-performance index and 22 percent scoring high called their health fair or poor. Since the objective and subjective health measures probably measure only marginally different aspects of the same phenomenon, we conclude that the data are supportive and the hypothesis merits further consideration.

Conclusion

How an individual defines his health influences many areas of his life. People who think they are unhealthy act as if they were ill and may in fact become so. In this paper we analyzed some sociological determinants of definition of health. We argued that in the United States people use poor health to legitimize a sense of failure to fulfill socially prescribed roles. In America the cultural values stress the importance of maintaining economic independence and striving to move up the social hierarchy. As Merton (1957) observed, not trying to better oneself is a form of deviance.

Welfare recipients occupy a stigmatized status in America. A substantial proportion of welfare recipients themselves define being on welfare as a type of failure; yet many have given up hope of becoming independent. When people occupy a self-defined illegitimate status and have little expectation of

²¹ For a description of the technique of test factor standardization see Morris Rosenberg, "Test Factor Standardization as a Method of Interpretation," *Social Forces*, 41 (1962-63), 53-61.

Table 7. Percentage Defining Health as Fair or Poor by Self-Defined Role Performance-Standardized by Age, Education, Income, and "Objective" Health (New York Working-Class Sample).

Role Performance				Standardized on			Objective Health
				Age	Education	Income	
Low	(4,5)	45	(109)	46	44	45	35
Med.	(2,3)	28	(193)	28	28	27	29
High	(0,1)	18	(108)	19	19	20	22

leaving this status, they feel a need to legitimize their failure. We have shown that defining one's health as poor is one way that welfare mothers have of legitimizing their status. Drawing on data from another study we showed that women who defined their performance as wives and mothers as being less than adequate were more likely than those who were satisfied with their performance to define their health as poor. We would hypothesize that wherever there are high rates of self-defined failure there will be high rates of self-defined poor health.

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LINKING FORM OF HYPOTHESIS TO TYPE OF STATISTIC: AN APPLICATION OF GOODMAN'S Z *

TAI S. KANG

State University of New York at Buffalo

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This paper suggests that types of statistics used in analysing data should be linked to logical forms of hypotheses.

It presents a proposal to include the weak conditional associations of ($p \rightarrow q$) and ($q \rightarrow p$), along with the straight biconditional monotonic form, in a basic paradigm of logical hypothesis forms.

Finally, it suggests that Goodman's flexible Z be applied in testing the validity of basic logical hypothesis forms and examines the plausibility of so doing.

GOODMAN and Kruskal (1954) suggest that the researcher should choose a measure of association which is contextually meaningful and appropriate for his particular research. This suggestion implies that the suitability of a measure depends much on its ability to assess the specific type and nature of the research hypothesis and the distinctive characteristics of the data.¹ Others, expounding further on this position, propose that a selected measure should be able to represent the form as well as the degree of association that exists among the variables in the hypothesis (Costner, 1965; and Leik and Gove, 1969). Unfortunately, in sociological inquiries, this essential link between form of hypothesis and type of statistical test to be used has been largely neglected.

Most of the statistical measures used in sociological inquiries are designed to test either the relationship of statistical independence or some type of monotonic association; whereas the sociological phenomena that we examine in our studies are, generally, types which do not fit into monotonic forms of association.

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¹ Sommers (1962:801-823), Lieberman (1964:745) and Costner (1965:350-352) all point out the analytical limits associated with Chi-square based measures and "P-R-E" related measures.

Ploch (1970) claims the biconditional relationship as the basic paradigm for a scientific hypothesis. He maintains that the hypotheses of the weak conditional types could be transformed into the biconditional form by redefining, specifying variables, improving measurement, and refining designs. However, Blalock (1961:30-35) points out that, whereas it is technically feasible to convert a conditional relationship between variables into a biconditional one by specifying and redefining the terms of an hypothesis, it is often impractical and theoretically awkward. Furthermore, as both Francis (1961:86-103) and Nowak (1960) argue, the conditional forms of relationship between variables are not only logically acceptable, but more often than not, theoretically appropriate and widely prevalent forms of sociological studies.²

This paper proposes that a measure of association to be used for research should

² This implies that we should seriously consider one of the several basic logical forms a relational proposition could take as a specific alternative to the null hypothesis. Merely rejecting independence is not enough to verify an hypothesis empirically. In this case, a hypothesis test necessarily involves verifying the logical structure of the relation indicated in the hypothesis. Moreover, in analyzing a cross-classification table, the arrangement of the table's marginal frequencies is critical—where and how many cutting points for the marginals should be set up. For example, a biconditional relation between two variables demands that the marginal distributions of a cross-classification table be alike for the column and row. To verify a hypothesis empirically one must recognize that the logical form of the hypothesis and the way empirical data are organized into a cross-classification table are inseparable (Francis, 1961:102).

be appropriate to the specific logical form of the hypothesis. In the ensuing discussion, I shall: (1) identify several basic logical forms which a hypothesis could take; and (2) investigate the feasibility of using a new measure of Goodman's Z_G (Goodman, 1969) to verify the validity of basic logical hypothesis forms.

A hypothesis is generally defined as a proposition which stipulates a specific type of relationship among the variables in the statement, the validity of which could be empirically tested. In the basic paradigm for the hypothesis structure, we could list the following types of relationships between an independent (p) and a dependent variable (q).³

Basic Logical Forms of Associations. The basic form of the hypothesis structure may be represented by simple 2×2 tables:⁴

³ Theoretically, the basic logical forms of a relationship between terms are implication, sub-implication, bi-conditionality (if and only if type), contrariety, sub-contrariety, and contradiction. The basic assumption of this paper is that a hypothesis is a relational proposition stated in terms of a probabilistic causal model. For this reason, in the discussion of an hypothesis' logical forms, only the strong relation of a "biconditional" type and the weak conditional relations of "implication" and "sub-implication" forms will be examined.

⁴ 2×2 tables are used here for presenting basic forms of the hypothesis. Of course, as we will see shortly, double polytomies could be used for these tables if the nature of substantive theory and the character of measurements warrant their use. Moreover, the extension of bivariate polytomies is straightforward.

1a. p is a necessary but not sufficient condition of q ($q \rightarrow p$). This means that $R_{pq} = 0$, i.e., p must be present, but q need not always follow p.

1b. p is a sufficient but not necessary condition for q ($p \rightarrow q$). This means that $R_{\bar{p}\bar{q}} = 0$, i.e., if p, then q; whenever p is present, q must follow, but q may also occur when p is not present.

1c. p is both a necessary and sufficient condition for q ($p \leftrightarrow q$). This means that $R_{pq} = R_{\bar{p}\bar{q}} = 0$, i.e., q if and only if p.

In analysis of sociological data, a hypothesis is generally stated in stochastic terms due to: the crude measurement devices we use in sociology, the inherent property of random variation imbedded in Nature, or the human limits of our knowledge about Nature. This means that we generally find some cases falling into a cell which, if the research hypothesis is structured in the deterministic model, is theoretically expected to have no member in it. For this reason the forms of hypotheses examined above should be stated probabilistically.

Logical Forms of Associations for Ordinal Data. The basic logical forms of association between variables for simple 2×2 table cases could easily be extended into cross-classification tables other than 2×2 tables.

Let us examine several such tables, as examples of more general $r \times c$ tables (assuming that both independent (p) and dependent variables (q) are measured in ordinal scale).

Table 1*

(a)			(b)			(c)		
		$\bar{p}$			$\bar{p}$			$\bar{p}$
q	p	R_{pq}	q	p	R_{pq}	q	p	R_{pq}
	$\bar{p}$	0		$\bar{p}$	$R_{\bar{p}\bar{q}}$		$\bar{p}$	0
$\bar{q}$	p	$R_{p\bar{q}}$	$\bar{q}$	p	0	$\bar{q}$	p	0
	$\bar{p}$	$R_{\bar{p}\bar{q}}$		$\bar{p}$	$R_{\bar{p}\bar{q}}$		$\bar{p}$	$R_{\bar{p}\bar{q}}$

* where

- p = independent variable
- q = dependent variable
- p = presence of p ; $\bar{p}$ = non-presence of p
- q = presence of q ; $\bar{q}$ = non-presence of q
- R_{pq} = the proportion of the cases that fall into the joint category of p and q
- $R_{\bar{p}\bar{q}}$ = the proportion of the cases that fall into the joint category of $\bar{p}$ and $\bar{q}$

Table 2*

(a)				(b)					
p				p					
H M L				H M L					
q	H	x	Δ	0	q	H	x	Δ	0
	M	x	x	Δ		L	x	x	x
	L	x	x	x					

* where

- p = independent variable
 q = dependent variable
 H = High
 M = Medium
 L = Low
 0 = an entry in a cell is near zero
 Δ = an entry in a cell is quite small (could be somewhat greater than the entries with 0 symbol)
 x = an entry in a cell is greater than the entries in the cells with 0 or Δ symbols

Each of the following three types could be expressed in terms of two or more basic sub-types, depending on the nature of theoretical implications associated with the table's following conditions: number of categories in each variable of the table; the way marginals are divided; and the differences indicated among categories.

1. p is a necessary but not sufficient condition of q:

Lower (or non) presence of p is not likely to be followed by the occurrence of higher q; however, higher p need not necessarily produce higher q; i.e., the lower p gets, the less likely it is for higher q to occur.

$R_{pLqH} = 0$ (or near zero) and

$R_{pMqH} = R_{pLqM} = \text{close to zero (or quite small)}$

where:

R_{pLqH} = the proportion of observations falling in the joint categories of low p and high q.

R_{pMqH} = the proportion of cases in the joint category of medium p and high q.

R_{pLqM} = the proportion of cases in the joint category of low p and medium q.

2. p is a sufficient but not necessary condition of q.

Higher p is usually followed by higher q, but higher degree of q may also occur when p is

not high; i.e., the higher p gets the less likely it is for lower q to occur.

$R_{pHqL} = 0$ (or near zero) and
 $R_{pMqL} = R_{pHqM} = \text{close to zero (or quite small)}$.

3. p is a necessary and sufficient condition of q.

In this hypothesis form, concurrent joint events of low p-high q and high p-low q are not likely to happen.

Again, depending on the theoretical characteristics associated with degree of implied category differences, we could state that:

3a. The higher p gets, the more likely q will be higher; and the items (or subjects) which differ on p are likely to differ on q. *There is a straight monotonic relationship between p and q.*

$(R_{pHqL} = R_{pLqH} = 0 \text{ or near zero, and } R_{pHqM} = R_{pMqL} = R_{pMqH} = R_{pLqM} = \text{close to zero or quite small})$.

3b. As p increases, q either stays the same order or increases.

$(R_{pHqL} = R_{pLqH} = 0 \text{ or near zero, and } R_{pMqH} = R_{pLqH} = \text{close to zero or quite small})$.

3c. As p increases, q has to increase; however, the items which do not differ on p could differ on q.

Table 3*

(a)				(b)					
P				P					
H M L				H M L					
q	H	x	x	x	q	H	x	x	x
	M	Δ	x	x		L	0	Δ	x
	L	0	Δ	x					

* where

- p = independent variable
 q = dependent variable
 H = High
 M = Medium
 L = Low
 0 = an entry in a cell is near zero
 Δ = an entry in a cell is quite small (could be somewhat greater than the entries with 0 symbol)
 x = an entry in a cell is greater than the cells with 0 or Δ symbols

Table 4*

(a)				(b)				(c)				(d)			
P				P				P				P			
H M L				H M L				H L				H M L			
H	x	Δ	0	H	x	Δ	0	H	x	0		H	x	Δ	0
q	Δ	x	Δ	q	x	x	Δ	q	x	Δ		L	0	x	x
L	0	Δ	x	L	0	x	x	L	0	x					

*where

p = independent variable

q = dependent variable

H = High

M = Medium

L = Low

0 = an entry in a cell is near zero

Δ = an entry in a cell is quite small (could be somewhat greater than the entries with 0 symbol)

x = an entry in a cell is greater than the entries in the cells with 0 or Δ symbols

($R_{pHqL} = R_{pLqH} = 0$ or near zero and $R_{pLqM} =$ close to zero or quite small).

3d. As q increases, p has to increase; however, items which do not differ on q may differ on p.

($R_{pHqL} = R_{pLqH} = 0$ or near zero and $R_{pMqH} =$ close to zero, or quite small).

Goodman's Z_G . In a recent article, Goodman (1969:1-40) proposed the use of an extremely flexible measure of association (Goodman's Z_G) for analyzing cross-classification tables.⁵ Using Goodman's notation:

Let f_{ij} : the number of observations falling into the cell on the i^{th} row and the j^{th} column.

g_{ij} : the natural logarithm of f_{ij} .

a_{ij} : an arbitrary coefficient assigned to the cell on the i^{th} row and the j^{th} column.

$$\text{Then } Z_0 = \frac{G}{S_0} \quad (1)$$

where

$$G = \sum \sum a_{ij} g_{ij} \quad (2)$$

$$\text{and } S_0 = (\sum \sum a_{ij}^2 / f_{ij})^{1/2} \quad (3)$$

⁵ In Goodman's articles, his standardized interaction of G is simply expressed as Z. Since this standardized value (Goodman, 1969:8-13) is specifically associated with Goodman's interaction G, we shall use the notation of Z_0 denoting standardized interaction G of Goodman (1969).

$$\left. \begin{array}{l} \text{and } \sum_i a_{ij} = 0 \text{ for } i = 1, 2, 3, \dots, n \\ \sum_j a_{ij} = 0 \text{ for } j = 1, 2, 3, \dots, n \end{array} \right\} \quad (4)$$

The interaction G represents either: (1) the significance of association between column and row classifications for an entry (or a set of selected entries) in a table; (2) the relative significance of association between column and row classifications for a set of selected entries in a table—compared to the association between column and row classifications for a set of other selected entries in a table; or (3) the relative significance of association between column and row classifications for a set of selected entries in a table—compared to the association between column and row classifications for a set of corresponding entries in another table.

Z_G is a standard value of the interaction G. The significance of a Z_G could easily be tested by using critical constants for significant tests suggested by Goodman (1969:8-13).

The feature of assigning arbitrary values into a_{ij} , as long as we satisfy the condition of the equation (4), makes this new association measure extremely flexible for testing a wide range of shapes and forms of possible relationships between variables, as displayed by Goodman (1969). For this reason, this statistic is particularly suitable for testing

the validity of specific logical forms of relationships stipulated for an hypothesis where variables are measured in ordinal scale.

Example 1: p is a necessary and sufficient condition for q .

Residential difference of urban and rural areas is a necessary and sufficient condition for differences in fertility rates.

This hypothesis may be stated as $p \leftrightarrow q$ where p = rural area and q = high fertility rate. In this form we observe that higher p is usually followed by higher q , and lower p by lower q . The data for the hypothesis form being tested is concentrated in the entries on the main diagonal. The entries both below and above the diagonal are likely to be small and to diminish as we move toward the lower left and upper right corners. Furthermore, entries in the non-diagonal cells are not related to each other.

To examine the validity of this biconditional hypothesis form, Goodman's Z_G should be applied in two steps. First, we must test the null hypothesis of quasi-independence (Goodman, 1969:27-29) of the column and row classifications for the two corner sets of entries. Second, after ascertaining the quasi-independence in the non-diagonal cells, we must examine the degree of data concentration in the diagonal cells.

Let us apply Goodman's Z_G to test this hypothesis by using the hypothetical data distributed as above. First, we test the quasi-independence of non-diagonal cells by blanking out the diagonal cells. According to Goodman (1969:14), any set of a_{ij} that

satisfies the set of equations (4) can be used to define an interaction index G of formula (2).

Assigning $a_{11} = a_{22} = a_{33} = 0$, $a_{12} = a_{31} = a_{23} = -1$, and $a_{21} = a_{13} = a_{32} = 1$, the interaction G for this hypothesis can be expressed as,

$$G = (g_{21} + g_{13} + g_{32}) - (g_{12} + g_{31} + g_{23}) \quad (5)$$

$$= (\ln f_{21} + \ln f_{13} + \ln f_{32}) - (\ln f_{12} + \ln f_{31} + \ln f_{23})$$

Using the equations (1) through (5) which define Z_G , we obtain the following:

$$G = (\ln 30 + \ln 10 + \ln 30) - (\ln 30 + \ln 20 + \ln 20) = .287$$

$$S_G = (\sum \sum a_{ij}^2 / f_{ij})^{1/2} = [(1/30 + 1/10 + 1/30) + (1/30 + 1/20 + 1/20)]^{1/2} = .458$$

$$Z_G = \frac{G}{S_G} = .628. \text{ As predicted } Z_G = .628 \text{ is}$$

not significantly different from zero, which implies that the non-diagonal cells are quasi-independent.

To compute Z_G for the second step of the tests, we assign $a_{11} = a_{22} = a_{33} = 1$, $a_{21} = a_{31} = a_{32} = a_{12} = a_{13} = a_{23} = -1/2$ to the equation (2).

$$G = (g_{11} + g_{22} + g_{33}) - [1/2 (g_{21} + g_{31} + g_{32} + g_{12} + g_{13} + g_{23})] \quad (6)$$

$$= (\ln f_{11} + \ln f_{22} + \ln f_{33}) - [1/2 (\ln f_{21} + \ln f_{31} + \ln f_{32} + \ln f_{12} + \ln f_{13} + \ln f_{23})]$$

$$= (\ln 80 + \ln 100 + \ln 80) - [1/2 (\ln 30 + \ln 20 + \ln 30 + \ln 30 + \ln 10 + \ln 20)]$$

$$= 4.1189$$

$$S_G = (\sum \sum a_{ij}^2 / f_{ij})^{1/2} = ((1/80 + 1/100 + 1/80) + 1/4 (1/30 + 1/20 + 1/30 + 1/30 + 1/10 + 1/20))^{1/2} = .2956$$

$$Z_G = \frac{G}{S_G} = 13.934$$

This interaction G represents the degree to which $(g_{11} + g_{22} + g_{33})$ differs from the bracketed quantity in equation (6). This difference indicates the extent of "concentration of events" in the main diagonal of f_{11} , f_{22} , f_{33} in contrast to entries in the non-diagonal cells. If this hypothesis form is valid, then Z_G computed for equation (6) should be significantly greater than zero.

This $Z_G = 13.934$ is significantly different

Table 5. Residential Areas and Fertility Rates (Hypothetical)

Fertility Rates	Area		
	Rural	Semi-Urban	Urban
High Fertility	80	30	10
Medium Fertility	30	100	20
Low Fertility	20	30	80

$$Z_{G1} = .628$$

$$Z_{G2} = 13.034$$

from zero (well beyond the significance level of 5%).

This statistically significant Z_G , together with the small Z_G obtained in equation 5, implies that the "concentration of events" in the diagonal cells relative to the entries in the nondiagonal cells is statistically significant and that the nondiagonal entries are quasi-independent. This is what we have expected for the biconditional form ($p \leftrightarrow q$).

Example II: p is a necessary but not sufficient condition for q .

A person's high social class is a necessary but not sufficient condition of his having a liberal attitude toward college students.

Symbolically, this hypothesis may be stated $q \rightarrow p$ where p = high social class and q = liberal attitudes toward college students. This means that lower p or p 's non-presence is not likely to be followed by higher q ; however, higher p need not produce higher q (Table 2a type). If the hypothesis is indeed valid, then we could expect to find the following in our cross-classification table: the lower p becomes, the less likely will higher q occur, i.e., symbolically expressed, $R_{pLqH} = 0$ (or near zero) and $R_{pLqH} = R_{pLqM} = \text{close to zero}$. Furthermore, since higher p need not produce higher q , we are likely to have quasi-independence among entries in cells which are not in the upper right-hand corner.

We apply Goodman's Z_G in two steps to test this conditional form using Table 6's hypothetical data.

First, we test quasi-independence among cells other than those where we expect to have "rareness of occurrence."

Taking $a_{11} = a_{12} = a_{13} = a_{23} = a_{33} = 0$, $a_{21} = a_{32} = 1$, and $a_{22} = a_{31} = -1$, the interaction G for testing quasi-independence assumption can be expressed as;

$$G = (g_{21} + g_{32}) - (g_{22} + g_{31}) \quad (7)$$

$$= (1n\ 90 + 1n\ 110) - (1n\ 90 + 1n\ 100)$$

$$= .0950$$

$$S_G = (\sum \sum a_{ij}/f_{ij})^{1/2} = (.0402)^{1/2} = .2004$$

$$Z_G = \frac{G}{S_G} = .4738$$

Table 6. Social Class and Liberal or Conservative Attitudes Toward College Students (Hypothetical)

Attitudes Toward College Students	Social Class		
	High	Middle	Low
Highly Liberal	80	30	20
Moderately Liberal	90	90	30
Least Liberal	100	110	100

$$Z_{G1} = .4738$$

$$Z_{G2} = -6.3715$$

The Z_G obtained is not significantly different from zero. This Z_G tests quasi-independence among cells other than those in the upper right-hand corner, a condition which may be viewed as one of those described for case II.

In the second step of the tests, we examine "rareness of occurrence of events" in the f_{12} , f_{13} , and f_{23} cells compared to other cells in the table.

Let $a_{13} = a_{12} = a_{23} = \frac{1}{3}$, $a_{11} = a_{22} = a_{33} = -\frac{2}{3}$, and $a_{21} = a_{31} = a_{32} = \frac{1}{3}$.

$$G = \frac{1}{3} (g_{13} + g_{12} + g_{23}) - [\frac{2}{3} (g_{11} + g_{22} + g_{33}) - \frac{1}{3} (g_{21} + g_{31} + g_{32})] \quad (8)$$

$$= \frac{1}{3} (1n\ f_{13} + 1n\ f_{12} + 1n\ f_{23}) - [\frac{2}{3} (1n\ f_{11} + 1n\ f_{22} + 1n\ f_{33}) - \frac{1}{3} (1n\ f_{21} + 1n\ f_{31} + 1n\ f_{32})]$$

This interaction G measures the degree to which $\frac{1}{3} (g_{13} + g_{12} + g_{23})$ differs from the bracketed quantity in (8). This bracketed quantity is a function of entries in the cells other than (f_{13} , f_{12} , and f_{23}), and could predict an average ($g_{13} + g_{12} + g_{23}$) assuming no interaction in the 2×2 tables which include the f_{13} , f_{12} , and f_{23} cells. If the Z_G that corresponds to equation (7) is not significantly different from zero, then, in the above equation (8), the extent to which the average of g_{13} , g_{12} , and g_{23} differs from the bracketed quantity may represent "rareness of occurrence of events" in the f_{13} , f_{12} , and f_{23} cells compared to other cells in the table. In other

words, if Z_G obtained in equation (7) is close to zero, this difference in (8) reflects the validity of the weak conditional form of relationship indicated in the hypothesis (in this case $q \rightarrow p$).

Using equations (1) through (4) and (3) to define Z_G for Table 6:

$$G = \frac{1}{3} (\ln 30 + \ln 20 + \ln 30) - \frac{2}{3} (\ln 80 + \ln 90 + \ln 100) + \frac{1}{3} (\ln 90 + 100 + \ln 110) = -1.123_3$$

$$S_G = .1763$$

$$Z_G = -6.3715.$$

Clearly, this Z_G is beyond the critical constant associated with the significance level of 5% (Goodman, 1969:11).

The statistically significant Z_G obtained in (8), together with a small Z in (7), indicates that the entries in the (f_{13} , f_{12} , and f_{23}) cells are significantly smaller compared to the entries in the table's other cells. This is consistent with what we have predicted for the weak conditional form ($q \rightarrow p$).

Example III: p is a sufficient but not necessary condition for q .

A person's social class is a sufficient but not necessary condition for his having a non-institutional view of education.

Symbolically, this hypothesis may be expressed as $p \rightarrow q$ where p = high social class and q = non-institutional view of education. This means that a higher p is usually followed by a higher q ; but a higher degree of q may also occur, when p is not high. If the hypothesis is indeed valid, then we expect to have the following data distribution in the cross-classification table: the entries in the cells on the lower left corner of the table are likely to be small and to diminish as we approach the corner of the joint category, higher p - lower q (Table 3b type). Moreover, since a higher degree of q may also occur when p is not high, we are likely to have quasi-independence among entries in cells not in the lower left-hand corner.

The two step method of using Goodman's Z_G in case II directly applies to the study of case III.

First, we examine quasi-independence

Table 7. Social Class and Perception of Education as Non-Instrumental and Instrumental (Hypothetical)

Perception of Education	Social Class		
	High	Middle	Low
Non-Instrumental	65	75	85
Mildly Instrumental	20	75	80
Highly Instrumental	20	25	70

$$Z_{G1} = .2707$$

$$Z_{G2} = -5.309$$

among cells other than those in the table's lower left corner.

By assigning $a_{11} = a_{21} = a_{31} = a_{32} = a_{33} = 0$, $a_{22} = a_{13} = 1$, and $a_{12} = a_{23} = -1$,

$$G = (g_{22} + g_{13}) - (g_{12} + g_{23}) = .0610$$

$$S_G = .2253$$

$$Z_G = .2707$$

This Z_G is not significantly different from zero. Hence, the quasi-independence assumption holds among entries other than those in the table's lower left corner.

In the second step, equation (8), which we used in case II, will be applied.

Let $a_{21} = a_{31} = a_{32} = \frac{1}{3}$, $a_{11} = a_{22} = a_{33} = -\frac{2}{3}$, and $a_{12} = a_{31} = a_{23} = \frac{1}{3}$, then

$$G = \frac{1}{3} (g_{21} + g_{31} + g_{32}) - [\frac{2}{3} (g_{11} + g_{22} + g_{33}) - \frac{1}{3} (g_{12} + g_{31} + g_{23})] = -1.0427$$

$$S_G = .1964$$

$$Z_G = -5.309$$

This Z_G is significantly different from zero.

As expected, we obtained a Z_G close to zero in the first step and a significantly large Z_G for the second step of tests for case III. This is what we have anticipated for the weak conditional form ($p \rightarrow q$).

SUMMARY AND CONCLUSION

Sociological hypotheses consist largely of weak conditional forms. For this reason, we proposed that the weak conditional association of ($p \rightarrow q$) and ($q \rightarrow p$) be included along with the straight biconditional mono-

tonic form in a basic paradigm of logical hypothesis forms.

This paper proposes a new approach for applying Goodman's flexible Z_G in testing the validity of the basic logical hypothesis forms. It is suggested that we could test the specific logical association form indicated in a hypothesis by appropriately managing coefficients (a_{ij}) to be assigned for the logarithm of a table's cell entries (f_{ij}).

The examples discussed in this paper illustrate some basic logical hypothesis forms. If a variant of the basic forms should be assumed for a hypothesis, either for reason of measurement or theoretical consideration, then a researcher could test the validity of this specified hypothesis form with a flexible arrangement of a_{ij} .⁶

A logical hypothesis form, stated stochastically, is a probability model. Z_G could statistically examine the validity of the probability model. To the extent that a logical form's validity is established, the logical model has a predictive ability. For this reason, the idea of applying Z_G fits rather well into Goodman and Kruskal's (1954) suggestion for selecting appropriate measures:

... it is desirable to choose a measure of association which has contextual meaning, instead of using as a matter of course one of the traditional measures ... to choose a measure of association which has meaning we propose the construction of probabilistic models of predictive activity. . . .

Describing a relation between variables, as Costner (1965:343) points out, basically refers to "form" as well as to "degree" or "closeness" of association. This new way of applying Z_G would enable us to express the relation between variables in terms of "closeness" of association to its specified "form." The proposed uses of Goodman's Z_G to test the logical hypothesis forms may

improve our efforts to link theory to statistics in sociology.

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⁶ For further details, see Goodman (1968:1126) and Goodman (1969:14). If the association between two variables is a weak conditional type, Goodman-Kruskal's Gamma is likely to be significant while both Sommers' d_{xy} and Kendall's Tau_b are not. For a biconditional association, all three measures—Gamma, d_{xy} and Tau_b are likely to be significant. This is a quick way of testing conditional implication though it is not as precise as Z_G .

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INTERNATIONAL TESTS OF THE SIZE-DENSITY HYPOTHESIS

G. EDWARD STEPHAN

Western Washington State College

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The hypothesis that the areal size of political units is inversely related to the settlement intensity of the regions in which they are located was developed and tested in an earlier work dealing with U.S. counties. Further support for this hypothesis, though tentative, is suggested in this analysis of data on the primary political divisions of ninety-eight nations.

IN a previous article in this review I showed that the areal size of counties in the United States was inversely related to the settlement intensity of their region, at least from 1790 until the 1920s when the automobile appeared. I related this finding to Durkheim's (1933) implicit theory of the growth of territorial organization and to two recent models of geopolitical structure developed by Stinchcombe (1968) and Boulding (1968). The purpose of the present research was to test the generalizability of the earlier findings, to find whether variations in size of other nations' political units might be similarly related to variations in settlement density.

If the theory of segmental growth is generally applicable, we would expect smaller units to appear in regions of higher population density; that is, we would expect to observe a negative relationship between size and density in each nation studied. Two such findings have already been reported: the geographer Peter Haggett (1965:54) found such a relationship for a sample of counties in the Brazilian state of Santa Catarina, and the anthropologist William Skinner (1964:34) reported a similar finding for market areas in rural mainland China. The data obtained for this study were drawn from a standard reference, the *Britannica World Atlas* (1967:198-272),

which gives area and population figures for the primary political divisions of the ninety-eight nations listed in Table 1. Nations for which such data were not available were generally too small to have primary political divisions; in addition, for several larger countries (e.g., Republic of Congo, Liberia, Libya) the data were incomplete. The kinds of units found in various countries differ greatly. In some cases they are nearly autonomous units of local or regional government; in others they are merely administrative districts for the central or national government. Detailed descriptions of the role of such units in each nation studied may be found in Sachs, 1967. The units studied also vary greatly in size: some average only a few hundred square kilometers, while others exceed an average of a hundred thousand square kilometers. Thus the study compares not only nations, but different types of primary political divisions. This seems appropriate since Durkheim's original theoretical statement (1933:186) seemed to apply to *any* kind of territorial organization: counties, communes, provinces, shires, departments, etc.

For comparative purposes the area data drawn from the *Atlas* were converted, where necessary, to square kilometers; and both area and density figures were expressed in common logarithms for preliminary data

plotting. They were then analyzed to determine the regression coefficient b in the equation $\log A = K + b \log D$. The hypothesis, that b would be negative, was tested using the standard one-tailed t -test with $N-2$ degrees of freedom. Results of the analysis appear in Table 1.

The hypothesized negative relationship be-

tween size and density was found in ninety-four of the ninety-eight nations in the study. In seventy-eight the relation was significant beyond the .05 level. Furthermore, none of the four positive relationships reported (Haiti, Italy, Malawi and Yugoslavia) was significant at the .05 level. Thus, for the whole set the hypothesis appears to be con-

Table 1. Number of units (N), coefficient of regression (b) of size on density, and the probability of b under H_0 .

Nation	N	b	p <	Nation	N	b	p <
Albania	27	-0.55	.0005	Jordan	9	-0.58	.0005
Algeria	15	-0.73	.0005	Kenya	8	-0.97	.0005
Angola	13	-0.71	.05	Korea, North	11	-0.81	.0005
Argentina	23	-0.58	.0005	Korea, South	11	-0.96	.0005
Australia	8	-0.74	.05	Lebanon	5	-1.05	.0005
Austria	9	-0.73	.0005	Luxemburg	4	-0.84	.05
Belgium	9	-0.13	.10	Malagasy Republic	6	-0.62	.10
Bolivia	9	-0.27	n.s.	Malawi	3	0.16	n.s.
Brazil	28	-0.64	.0005	Malaysia	13	-0.99	.0005
British Honduras	6	-0.36	n.s.	Mexico	30	-0.75	.0005
Bulgaria	28	-0.52	.0005	Mongolia	18	-0.55	.0005
Burma	7	-0.47	.05	Morocco	18	-0.71	.0005
Cambodia	17	-0.28	.0005	Netherlands	11	-0.10	n.s.
Canada	12	-0.45	.05	New Zealand	13	-0.19	n.s.
Cent. Afr. Rep.	13	-0.36	.0005	Nicaragua	16	-0.77	.0005
Ceylon	9	-0.33	.0005	Nigeria	5	-1.55	.0005
Chad	11	-0.51	.0005	Norway	20	-0.89	.0005
Chile	25	-0.59	.0005	Pakistan	16	-0.34	.0005
China	26	-0.67	.0005	Panama	9	-0.32	.10
Columbia	29	-0.44	.0005	Paraguay	17	-0.59	.0005
Congo (D.R.)	13	-1.19	.0005	Peru	23	-0.34	.01
Costa Rica	7	-0.48	.05	Philippines	57	-0.64	.0005
Cuba	6	-0.43	n.s.	Poland	21	-1.19	.0005
Czechoslovakia	11	-1.00	.0005	Portugal	18	-0.44	.0005
Denmark	24	-0.79	.0005	Rhodesia	6	-0.48	.05
Dominican Republic	26	-0.17	n.s.	Romania	17	-1.06	.0005
Ecuador	17	-0.56	.0005	Saudi Arabia	4	-1.13	.0005
El Salvador	14	-0.39	.05	Sierra Leone	4	-1.58	.0005
Finland	12	-0.58	.05	South Africa	4	-0.93	n.s.
Formosa	18	-1.00	.01	Spain	50	-0.42	.0005
France	89	-0.26	.0005	Sudan	9	-1.05	.0005
Gabon	9	-0.55	.05	Sweden	25	-0.78	.0005
Germany, East	15	-0.78	.0005	Switzerland	25	-0.61	.0005
Germany, West	10	-1.53	.0005	Syria	12	-0.76	.0005
Ghana	9	-0.95	.0005	Tanzania	17	-0.80	.0005
Greece	9	-0.17	n.s.	Thailand	4	-0.39	n.s.
Guatemala	22	-0.60	.0005	Togo	4	-0.72	.10
Haiti	5	0.76	n.s.	Tunisia	13	-0.81	.0005
Honduras	18	-0.58	.0005	Turkey	67	-0.38	.0005
Hungary	24	-1.07	.0005	Uganda	4	-0.68	.01
Iceland	5	-0.33	n.s.	U.S.S.R.	43	-0.71	.0005
India	16	-0.01	n.s.	U.A.R.	16	-0.80	.01
Indonesia	23	-0.60	.0005	United Kingdom	101	-0.09	.10
Iran	19	-0.47	.05	United States	48	-0.53	.0005
Iraq	14	-0.82	.0005	Uruguay	19	-0.48	.0005
Ireland	26	-0.36	.05	Venezuela	22	-0.62	.0005
Israel	18	-0.72	.0005	Yugoslavia	8	0.45	n.s.
Italy	19	0.52	.10	Zambia	8	-0.69	.0005
Ivory Coast	4	-0.81	.01				
Japan	46	-0.50	.0005	World	1764	-0.66	.0005

firmed: as density increases, size decreases. It is difficult to generalize about cases where the relationship was not significant, except that in most the number of political divisions was small and/or population density varied little to begin with. In several (e.g., the United Kingdom) it may be that boundaries which once reflected the population distribution have been maintained despite later population shifts.

It may be argued that the hypothesis of a negative relationship between area and density is not a substantive one since density contains area in its denominator (density = population/area). Snedecor (1946:162) has noted, however, that "one may gain information from correlating X_1 with X_2/X_1 ." As he points out:

Having observed some unwarranted interpretations of such correlations, Karl Pearson dubbed them 'spurious,' and this rather derogatory title has led people to distrust them. Of course, it is the interpretation that may be spurious. The correlations are on the same footing as any others. . . .

In order to avoid a spurious interpretation of the results reported, their relation to the theory of segmental growth must be discussed further; they do not, in fact, support the theory conclusively. The problem comes down to this: if heavily settled regions do contain small political units, then size and density will be negatively related—but the converse is not necessarily true.

We can conclude from the reported negative relationships that we have *failed to reject* the theory of segmental growth; they do not conclusively support the theory. The problem is that simple area and density relationships fail to account for the *location* of a unit in a densely settled region (e.g., the location of New Jersey in the mid-Atlantic region or Arizona in the southwest); they relate area and density only and thus are subject to criticism unless the added locational and regional information is supplied. If people in a densely settled region, like the mid-Atlantic, drew boundaries to form large political units, or if people in a lightly settled region, like the southwest, drew boundaries to form small political units, the size-density hypothesis (and theory of segmental growth) could be rejected. The results in Table 1 allow us to conclude merely that this seems not to be the case. Additional work, perhaps

using some measure of "population potential" in place of "density," (cf., Anderson, 1970), might yield more positive conclusions. Note that density *maps* for several countries (e.g., the United States, Japan, China, Brazil, Sweden and Norway) do seem to support the theory—such maps show smaller political units located in densely settled regions and larger units in more lightly settled regions.

To reduce the individual findings in Table 1 to some general form, all 1764 subdivisions were aggregated and analyzed as a set, for signs of an overall pattern. The results of this analysis appear in the last line of Table 1, from which we can obtain an expression for the "world regression line": $\log A = 5.0684 - .66 \log D$. My question then was whether the reported regression coefficients in the set of nations differ significantly from the -0.66 value reported for the aggregate. To test this hypothesis I used Lehman's procedure (1959:284-285) for testing whether a set of regression lines have equal slopes. With 1568 and 97 degrees of freedom, the value of $F (=4.11)$ was significant beyond the .01 level, so the hypothesis that the slopes did not differ had to be rejected. Still, the test's number of degrees of freedom was very large.

Individual t -tests for each country, testing whether its regression coefficient differed significantly from the -0.66 values of the aggregate, showed that seventy-six of the ninety-eight nations had no significant difference beyond the .05 level. In eight nations the regression coefficient was significantly "less negative" than the aggregate's (Ceylon, Dominican Republic, France, Italy, Spain, Turkey, United Kingdom and Yugoslavia). In fourteen nations the slope was significantly "more negative" than the aggregate's (Congo, Czechoslovakia, West Germany, Ghana, Hungary, Lebanon, Malaysia, Nigeria, Norway, Poland, Romania, Saudi Arabia, Sierra Leone and Sudan). Thus, it may be that the "world regression line" does describe some meaningful average tendency to relate size and density, but that other variable(s) may determine the actual slope in specific cases; what these variables might be is not evident from this grouping of countries which deviate from the average tendency.

Finally, regardless of the above, scatter diagrams for each nation in the study show remarkable convergence along the plotted world regression line. That is, although Italy, for example, shows a "significantly different" *positive* relationship, the point-coordinates of size and density for its political units show little spread; in fact, they cluster very nearly on the aggregate line. While it is not feasible here to reproduce all ninety-eight scatter diagrams, I can report my own general impression that the line described by the aggregate does seem to fit the scatter diagrams of the individual nations fairly well.

While inconclusive this study does suggest a relationship between a nation's population distribution and variation in size of its political units. Whether this relationship is spurious, or depends on some measure of distribution other than density, only further study will reveal. I suggest the need for such study, and conclude that these international tests of the size-density hypothesis have failed to reject the theory of segmental growth and thus eliminated one empirical possibility.

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COMMENTS

ON THE HOBBSIAN PROBLEM OF ORDER: A COMMENT

We may begin this comment on Ellis's recent paper (Ellis, 1971) on the "Hobbesian problem of order" by agreeing with two preliminary points. First, we accept Ellis's judgment that the problem of social order and solution proposed are central to the general theoretical scheme of Talcott Parsons, since Parsons' theoretical enterprise is devoted primarily to resolving this problem (Parsons, 1937; for a recent simplified presentation, see Parsons, 1971:4-28, *passim*).¹ We also recognize its importance in the writings of several historical precursors to modern social theory, particularly political theorists like Hobbes (1966).

Second, we would also concur with the more general point at stake: namely, that it is usually wise to evaluate theories in the natural or social sciences by examining the basic problematic which occasions them and which they attempt to explain and resolve. In short, it is always legitimate to ask: "Why do we need a theory in this case?" and "What is the theory for?" (On this, cf. Kuhn, 1970).

Having assented to these two points, however, we find Ellis's presentation and treatment of the problem of social order in many respects unsatisfactory and untenable. We shall focus on what we take to be the central limitation of Ellis's account.

Ellis's triad of proposed solutions to the Hobbesian problem of order—*viz.* the normative, exchange, and coercive solutions—

¹ Although we have not followed this usage consistently, note that the term "theoretical scheme" is a more apt description of Parson's conceptual writings since, whatever their validity or heuristic value, they nowhere amount to a 'theory' in the strict sense of an hypothetico-deductive system of propositions. However, since the latter is often taken as an ideal for sociology to strive for (e.g., Zetterberg, 1965) it might be observed that in the case of genuinely deductive theories the resolution of the basic problematic is expressed in the theory's axioms. Whereas, in the case of more informal sociological theories the relationship is usually less clear-cut.

although commonly presented in this form,² would appear to suffer principally from its rejection *ab initio* of a full statement of the thesis of cultural determinism, even though the latter would seem to be implicit, if not explicit, in most descriptions and applications of the distinctively sociological perspective (cf. Berger and Luckman, 1967).

Clearly, the normative approach, as Ellis (1971:693-4) describes it, is itself a cultural approach to the problem of order. It refers to the existence of shared values and norms and these are cultural phenomena. But shared values and norms describe only part of the usual domain of the concept of culture. Indeed, the inadequacy of the normative approach, which Ellis effectively perpetuates, resides in large part precisely in its failure to consider adequately the non-evaluative or 'cognitive' aspects of culture.³ For present purposes it will suffice to classify these as follows: (1) *beliefs* and substantive knowledge; (2) more formal cognitive elements, such as principles of classification and modes of reasoning; and (3) language.

Empirically, the importance of these factors for both the constitution and maintenance of social order and integration has often been emphasized in social science,⁴ despite Ellis's neglect of them in his account. However, the point at issue may be expressed more strongly; namely that a shared language (whatever its form) and certain common principles of cognitive organization

² And not only in the case of grand theory, but also in more middle-range theoretical endeavours (e.g., Etzioni, 1961).

³ The other subdivision of culture usually cited in this context is the aesthetic. We do not discuss this here, however, since it would seem logically secondary to both the cognitive and evaluative aspects of culture. But for an account emphasizing its importance, see Jaeger and Selznick, 1964.

⁴ For example, the role of the more formal cognitive elements (e.g., principles of classification and taxonomy) has been stressed in much recent anthropology as providing the underlying 'logic' of existing cultures. Such a perspective is shared, for instance, by both the Continental "anthropologie structurale" (e.g., Lévi-Strauss, 1969) and the American tradition of ethnoscientific analysis (cf. Sturtevant, 1964).

seem logically implicit in the very *concept* of society and of normal intelligible social existence (cf. Aberle *et al.*, 1950; Levy, 1952; Winch, 1958, 1960; MacIntyre, 1971: pt. II). These shared cognitive elements appear to be among the elementary logical prerequisites for any understandable form of social life.

Expressed thus, the above observation might not seem surprising, especially since, as we said earlier, assumptions of this kind pervade most descriptions and applications of the sociological perspective. However, the observation has consequences for formulating the Hobbesian problem of social order, and some of the ways it does may now be briefly examined.

First, the existence in a society of a common language and of certain shared cognitive assumptions (both formal and substantive) does not itself preclude the possibility of either social disorder or social malintegration in that society (for this last distinction, see Ellis, 1971:696-7). Rather—and this is why Ellis's paper requires a more adequate concept of culture—the relationship is more the other way round; that is, the idea and existence of human conflict in its various forms itself normally presupposes certain shared, reciprocal cognitions, albeit limited ones (regarding the definition of 'enemy,' or 'opponent' and of 'hostile' or 'threatening' action⁶ etc.). The idea of either inter-personal, inter-group, or inter-societal conflict occurring in an entirely pre-cultural context is scarcely conceivable, except perhaps in some ideal and pristine situation of 'pure' culture-contact.

Hence, as modern political philosophers have often pointed out, the Hobbesian "state of nature" far from designating a pre-social existence, necessarily contains certain vestiges of a shared culture. And so the function of the (hypothetical) social contract, which both leads to and legitimizes the emergence of the protective State, is simply to stabilize and improve upon this existing culture. It is not to create a culture *ex nihilo*: this would be a conceptual absurdity.

In short, the main limitations of Ellis's ac-

count of the problem of order may be expressed as follows:

Ellis spells out the relative merits of each of the three general 'solutions' to the problem of social order—namely, the normative, exchange, and coercive solutions. These he takes to be the basic and, one presumes, exhaustive theoretical and empirical alternatives available. But in doing so Ellis is ascribing a basic and primordial status to concepts that are logically secondary, since each is contingent upon certain minimal elements of shared, cognitive culture.

As a corollary to this, we would hold that explicating the relationship between each solution and its respective cognitive underpinnings will permit the supposed problem of social order to be presented in a more intuitively (and logically) acceptable form.

No such detailed explication will be attempted here. However, it should be noted that in addition to arguments and evidence drawn from sociology, modern analytic philosophy (and especially the philosophy of action) is likely to prove highly suggestive in this regard. Here we shall simply mention a few elementary considerations relevant to such an enterprise and suggest one key revision that might be made to the problem of order as conceived by Ellis and others. Second, we shall suggest briefly that this problem be viewed in the light of the prevailing philosophical anthropology in modern social science.

First, Ellis seems not to recognize that both the "exchange" and "coercion" accounts of social order are normally thoroughly imbued with elements of shared cognitive culture.⁶ This is most apparent in the case of the exchange solution, associated with Spencer, and, more recently Blau (1964): evidently, to take the leading terms of this approach, the substance of the formal concept of self-interest and, more complexly, the formal prudentialist strategy whereby self-interest is "regulated in the short run, so that in the long run present gains may be maintained or extended" (Ellis, 1971:695)

⁶ For the argument that there is a necessary relationship of belief to any kind of action, see MacIntyre, 1971:pt. II.

⁶ In part this omission may be due to his adopting an abstracted "Ego-Alter" relationship as the basic paradigm for his discussion, which in turn derives from his methodological commitment to the psychological reductionism of Homans (see Ellis, 1971:696).

is in any empirical instance culturally specified and related to cultural definitions of rationality. That is, we can legitimately expect societies' conceptions of self-interest and the appropriate means for attaining it to vary according to their differing beliefs about what constitutes man's nature (i.e., their philosophical anthropologies).⁷ Comparative evidence indicates some of the many possibilities here—including, for example, the possibility that self-interest may be specified in what to *our* mind are altruistic terms. Moreover, the exchange solution to the problem of order would seem to imply certain minimal agreement on moral as well as cognitive matters (regarding the need for norms of truth-telling,⁸ promise-keeping, etc.); and this makes it difficult to disentangle from its normative counterpart. To this Ellis would probably reply that such moral agreement is purely prudential and pragmatic (Ellis, 1971:696); but if it were wholly pragmatic and prudential, then it is difficult to see how any "exchange" relationships could be viably maintained over extended periods of time, as various sociological studies of the implicit moral elements in the typical contract have pointed out (e.g., Durkheim, 1933; Parsons and Smelser, 1956; and Garfinkel, 1967). Ellis, like Parsons (1937), associates the "exchange" approach with a utilitarian perspective; and utilitarianism as a social theory has often foundered in its attempt to incorporate within itself naturally intrinsic moralities such as those concerned with truth-telling or promise-keeping (cf., e.g., Foot, 1967).

A similar observation holds for the "coercive" solution: as we suggested earlier, coercion characteristically involves shared cognitions (usually together with the means for communicating these cognitions—namely, language); and the idea that the problem of social order is normally resolved by "pure coercion" without *any* pre-existing

shared cultural definitions of the situation could scarcely be less plausible.

Finally, the familiar "normative solution" which Ellis's article principally criticizes,⁹ again presupposes the existence of shared cognitive elements.¹⁰ In this sense, both values and norms are logically secondary concepts. Obviously one cannot have shared values and norms, at least as effective bases for social action, unless one presupposes a common language for communicating these values and norms. (On the distinction between implicit and explicit consensus being alluded to here, see Scheff, 1967.) But and more importantly, we would also contend that values and norms, shared or otherwise, necessarily presuppose descriptive (or explanatory) beliefs to be intelligible (cf. MacIntyre, 1971: pt. II).

Thus one might reverse the familiar Humean thesis and assert that values or prescriptions (i.e., "ought" propositions) necessarily presuppose beliefs (i.e., "is" propositions), and, further, that many cases of apparent value conflict turn out on closer inspection to be based on differences of belief,¹¹ whether the beliefs in question are empirical or not.¹²

⁷ Ellis's main criticism of the normative approach (of Parsons *et al.*), and the *leitmotiv* of his article, is to the effect that although, via an appeal to shared values and norms, it can help explain the maintenance of social order, it typically fails to account for the emergence of this order. From the above considerations, however, we would view Ellis's constant reference to the Hobbesian 'transition problem' as in a sense *the* problem of order, as being due to his phrasing the problem in too fundamental a form.

¹⁰ Parsons does of course recognise the presence of generalized beliefs at various points in his conceptual scheme. But from our point of view he fails to draw out the full implications of cognitive culture for the problem of order under review.

¹¹ This is evident, for instance, in many discussions of race in modern society and as such it points to the relevance of general education, as distinct from various forms of ideological persuasion, as a means for altering individuals' values in this area. On this cf., for instance, Rose, 1968.

¹² More precisely, the important question as to whether such conflicts are perceived as intellectually soluble or not depends on the existence of shared epistemologies, in the sense of accepted procedures for resolving differences of belief, whether the procedures in question involve inspecting legal precedents, consulting with the oracle, or using the empirical methods of modern science.

⁸ Note that even the supposedly biological needs of man—for e.g., food, shelter, and self-defense—cannot be viewed as wholly culture-free since they require the existence of at least minimally socialized individuals for their operation.

⁹ For the argument that the existence of a common language entails norms of truth-telling, see Winch, 1960.

This latter view goes against the common sociological practice of treating values as the logical termini of explanation. But this practice would itself seem largely due to an over-zealous adherence by sociologists to Hume's original and much-quoted observation on the logical impropriety of inferring evaluations, and most especially moral judgments, from mere descriptions of the world (in his *Treatise*, Book III, Part i). Following this dictum, social theorists have tended to overstate the resulting autonomy of values (and also their supposedly purely subjective status). For to claim that "*b* may not be inferred from *a*" does not rule out the possibility that "*a* may be validly inferred from *b*."¹³ And indeed if one translates "*a*" and "*b*" into "beliefs" and "values" respectively, this latter possibility (of inferring beliefs from values) does seem available to us.

On the other hand, as we noted earlier, the fact that a given society embodies certain common cognitive styles and assumptions does not itself preclude the existence of value dissensus, and of conflict over basic values, in that society, though it might be thought to militate against this in the empirical case to some extent (by providing a common framework for discussing and mediating such conflict). The point here is not to deny the existence of this type of conflict but to suggest that it does not really impinge on the allegedly universal and primordial Hobbesian problem of order, at least as this is conceived by Ellis and certain of his theoretical predecessors, since the very existence of shared cognitive elements supplies a conceptual infrastructure to the conflict.

Summary and Conclusions

In short, these shared cognitive elements are logically implicit in both the normative and exchange solutions to the problem of order in society, and in the case of the coercion solution they may be treated as at least virtually universally present.

We also question the utility (and valicity) of assuming the existence of a universal problem of social order as a suitable starting-

point for general sociological theory, since some degree of conceptual order seems implicit in the very definition of society and normal social existence, and indeed in social conflict as this is usually conceived (including those conflicts "in which force and fraud have a high constant probability in being selected" Ellis, 1971:696).

This of course still leaves open the problem of the origins of whatever conceptual schemes happen to prevail in particular societies at particular points in time. And here the perspective of the sociology of knowledge and, for example, of structural anthropology (Lévi-Strauss, 1969) may shed valuable light on individual cases, even though these approaches are likely also to encounter serious epistemological difficulties in validating their insights.

It also leaves open the need to deploy sociological theory and research toward understanding the presence of conflict and disorder in human affairs. But the above arguments suggest that the basic issue be reconceptualized. The appropriate problem for sociology is not really to explain, as Ellis and others suggest, the supposedly general problem of social order since, as we have seen, this is usually presented in an untenable and hence misleading form. The more realistic task for sociology is to explain specific instances of social disorder.¹⁴

We would maintain this latter in part from our understanding of the basic concept of society and of the role of cognitive culture within it. But we could also argue that the whole notion of a general, primordial problem of order as an appropriate problematic for general sociological theory is mistakenly premised upon a now-discredited philosophical anthropology: namely that of an Hobbesian anti-social man, who, it will be recalled, required the discipline of a coercive State to keep his self-aggrandizing and power-seeking impulses in check. Empirical sociology, and more especially social psychology, has already rejected this anthropology in favour of a bio-social conception of man's nature, whereby continuing social interaction (and so minimal social order) is an ongoing prerequisite for the most ele-

¹³ That is, simply, the falsity of " $a \rightarrow b$ " does not entail the truth of " $b \rightarrow a$."

¹⁴ We leave undiscussed the possible ideological implications of how one formulates this metatheoretic problem of order.

mentary human maturation.¹⁵ Given, this latter premise, the whole idea of a general problem of social order, perceived as inherent in human existence becomes dubious.

Indeed, probably because it chooses such an infirm starting-point, general sociological theory (i.e., grand theory) has so far been relatively unproductive and lacking in rigour. Merton's advocacy of middle-range theory seems sensible advice, at least until a new problematic can be found for general sociological theory to justify its existence.

JOHN H. BARNSELY

*University of Aston
Birmingham, England*

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REPLY TO BARNSELY

In its polemics, Barnsley's Comment may be summarized as follows: First, he opposes any sociological theory of social order which assumes that conceptual order is problematic. Conversely, he favours "cultural determinism," that is, taking conceptual order as a given and postulating that it determines other elements of culture (e.g., norms and values) as well as social interaction (social organization). Second, he opposes process explanations of morality. Conversely, he favours a "Topsy model" of moral orientations and obligations, however they developed. Third, he is against grand theory. Con-

¹⁵ Indeed the paradox is that sociological writings have been criticised for over-stating the social determination of man's nature and activities. An influential statement is: Wrong, 1961.

versely, he favours "middle range" theory.

My reaction to Barnsley's first and major point is that it is irrelevant; my thesis was not based on this assumption. As for the second point, I conclude that, as developments in modern social science do provide process explanations of the 'implicit moralities' to which he refers, Barnsley's opposition reflects a naïveté about these developments. As for the third point, Barnsley's rationale is confusing. This somewhat unkind set of conclusions is based on the following considerations.

Cultural Determinism, Conceptual Order and Social Order

According to Barnsley, the problem of order is an inappropriate problematic for developing general sociological theory because it "is mistakenly premised upon . . . an Hobbesian anti-social [conception of] man" living in a pre-social, natural state. "Since some degree of conceptual order seems implicit in the very definition of society" there exists no general problem of social order. More specifically, as all three solutions to the problem presuppose the existence of shared cognitive elements and shared cognitive elements in turn ensure social order, the development of sociological theory can only be retarded by perpetuating an approach to social theory which takes as its starting point the problematic nature of social order.

Let me begin with Barnsley's evaluation of Hobbes. My impression is that either he has not read Hobbes or not read him carefully. Hobbes was in fact writing about socialized actors, "men . . . born inapt for society and who were made fit for society, not by nature but by education" (*English Works*, Vol. II, p. 2), interacting in a market society characterized by civil war. Hobbesian men shared both cognitive and evaluative elements of culture. However, in relation to the evaluative (normative) element, the role of sanctions in regulating social behaviour was clearly recognized—"covenants without the sword are but words and of no strength to secure a man at all" ((1651) 1958:128). It is instructive to compare Hobbes' position here with that taken by Blake and Davis (1964).

Certainly, Hobbes' analysis is crude by present standards; however, he was less interested in presenting a sophisticated axiomatic basis for a general theory of society, than in exploring how the ever-present possibility, inter-personal conflict, could be regulated in a situation in which, "reason was frustrated, custom broken down, education corrupt and self-interest imprudent" (Lamprecht 1966:39). In short, Hobbes was primarily interested in a transition (planned social change) problem. In this transition, cognitive and evaluative elements of culture are shared by men in both the *from* (natural) condition and the *to* (civil) condition. In short, while conceptual order obtains in the natural condition, social order remains problematic.

It seems to me that Barnsley has confused two types of problems—a de-randomization problem (predictability in behaviour) and an Hobbesian conflict regulation problem. While solutions to the former are neutral with respect to distributing the interaction outcomes (gratifications and deprivations), the latter process is not. Thus, it is possible to have predictable, mutually depriving interactional sequences. Barnsley's focus, like that of Simmel's (1963:73-92) is on the former (derandomization or conceptual order) problem. The cultural deterministic perspective is coterminous only with *conceptual order*, i.e., it assumes predictability in behaviour based on shared reciprocal cognitions. The (Hobbesian) problem of social order however is created by men who already share reciprocal cognitions. Now, since conceptual order (a given) can be associated with varying degrees of social conflict, as Barnsley himself admits, in what sense is the cultural deterministic perspective, "consequential for the Hobbesian problem of order"? As used in this specific context, Barnsley's notion of consequential is emptied of substance.

According to Barnsley the cultural deterministic perspective is consequential for the Hobbesian problem for at least two sets of reasons. One, because "conflict presupposes shared reciprocal cognitions." Whereas the first set of reasons deals with the formulation of the problem, the second set focuses on solutions to the Hobbesian problem. Now, just as Barnsley failed to demon-

strate precisely how his perspective was consequential in the context of discussing the Hobbesian problem, he fails to show how acceptance of the second set of assumptions invalidates the conclusions I reached in my original article. Solutions to problems arrived at by men who share reciprocal cognitions will indeed "logically imply" the fact of shared cognitions. Surely consequential means more than the acceptance of this statement. Apparently it does. To discover how it does, we move to what Barnsley (rightly) contends I rejected; "a full statement of cultural determinism." Barnsley believes that cognitive elements of culture determine other cultural elements (e.g., norms and values) and social organizational (interactional) elements. In the beginning was the mind, not the deed nor mind and deed in interaction. Certainly the matter is not as settled as Barnsley imagines. Mead, for example, conceptualized the causal sequence in society, mind, self terms, while Dewey maintains that, far from values and norms being logically secondary to cognitive elements, cognition itself is grounded in feeling and behaving. More generally, the status of cultural determinism as an explanatory paradigm is far from being generally accepted even by those who are as committed as Barnsley is to the "sociological perspective." (Blake and Davis 1964).¹

Third Barnsley's version of this perspective diverts attention from the analysis of a relevant and highly strategic set of problems, for example, the interrelationships between symbolic (cognitive), moral (evaluative) and social organizational (behavioural) realms (see Jaeger and Selznick 1964). The thesis of cultural determinism as formulated by Barnsley (and Parsons) solves Hobbes' problem of order by simply avoiding it.

Moral Obligation and Exchange Theory

According to Barnsley, exchange-utilitarian theory has failed to "incorporate within itself naturally 'intrinsic' moralities such as those concerned with truth telling and prom-

ise keeping." Now, by "naturally intrinsic" Barnsley could mean that moral behaviour is not learned behaviour. But, given his commitment to a bio-social conception of man, Barnsley would probably reject this meaning of his term. If we grant that moral behaviour is *acquired* via the process of learning I would then ask, what operative principles of learning does social exchange theory fail to embrace? Barnsley's Comment does not answer this question.

Second, by "naturally intrinsic" Barnsley could mean that regardless of how moral behaviour was learned, once learned its *maintenance* is not a function of any consequences that might be paired with the behaviour of "external" socialization agents, i.e., the individual rewards and punishes himself for complying or not complying with moral rules. If this is what Barnsley means, then "naturally intrinsic" moralities can, with relative ease, be incorporated into exchange theory. The application of conditioning (operant and classical) principles to this problem are presented by Bandura and Walters (1963:162-223) and Scott (1971) among others.

Grand vs. Middle Range Theory

According to Barnsley, the current condition of general sociological theory—he calls it "unproductive and lacking in rigour"—is caused by "its choice of an infirm starting point, i.e., the Hobbesian problem of order." Why is this starting point "infirm"? Because "given a bio-social conception of man, the whole idea of a *general* problem of order becomes dubious." In my view, holding this conception and the premise it implies (social interaction) has little to do with the Hobbesian problem, for the outcome referred to by Hobbes is an "ever-present possibility" among socialized actors. This point is clearly recognized by scholars whose "basic problematic" is not the Hobbesian problem of order, and who like Barnsley and I maintain a bio-social conception of man. Thus, Blake and Davis, following their critical treatment of a "cultural determinist" theoretical stance, contend that "societies . . . are . . . filled with conflict, striving, deceit, cunning" (1964:464). In short, accepting the bio-social man postulate (or

¹ For a theoretical statement of the relation between the Hobbesian problem and "cultural complexity" as a dependent variable, see Sahlins (1968). For an excellent demonstration of the interaction between cultural (Waiteri complex) and social organizational elements see Chagnon (1968).

the social interaction premise) in no way invalidates the idea of conflict as an ever present possibility. To the extent that this statement is acceptable, it seriously undermines Barnsley's rationale in support of middle range theory. It may well be that the unproductive, unrigorous condition of sociological theory is due not so much to its "infirm" starting point, and the level of theorizing engendered but to the fact that too few "large minded" sociologist-philosophers have tackled the problem. In any case, middle range theorizing guarantees neither productivity nor rigour.

Secondly, I find it difficult to reconcile Barnsley's critique against grand theory and reductionism with his positive reference to the work of Levi-Strauss (*The Raw and The Cooked*). Is this critique general or is it directed against particular grand theories and types of psychological reductionism? If it is meant to be general, and I think it is, then how does one explain his support of Levi-Strauss, perhaps the greatest "grand" theoretician and most committed cognitive reductionist among contemporary social scientists.

In conclusion, let me say that even where I have closely followed Barnsley's advice to engage in, "the more realistic task . . . [of] explaining specific instances of social disorder," the deficiencies of grand theory have not been as patently obvious as Barnsley suggests. In fact, quite the reverse is true. Such theoretical integration as my "realistic" research on the problem of social order in prisons achieves will be due less to middle range theorizing than to my reaction to the starting point of Parson's grand theory—the Hobbesian problem of order.²

DESMOND ELLIS

York University

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A COMMENT ON "THE SCIENTIFIC LEGITIMIZATION OF FALLACY"

As the author whose name appears in the first sentence of the paper by Lauer "The Scientific Legitimization of Fallacy: Neutralizing Social Change Theory," (1971), I feel constrained to comment on the fallacy of inventing spurious fallacies and particularly on the appearances of sketchy scholarship displayed in an article published in our most prestigious journal.

Lauer seems ignorant of a book of mine (Moore, 1963), the first chapter of which is entitled "The Normality of Change." Therein and subsequently in a rather concise exposition I attend to intrinsic sources of change in *any* society, and explicitly reject any social-system model so tightly interdependent and self-equilibrating that any change would be either "trivial or tragic." The book cited is scarcely an obscure source: some 70,000 copies have been sold, though of course I have no way of determining how many actually have been read. I refrain from further extensive citations of publications by my favorite author over the last decade or so for fear of the successful charge of being

self-serving. I must, however, refer to another short and widely known volume (Moore, 1965) which does not argue for a unidirectional course in modernization, (Lauer, 1971:884-886) and does attend to "convergence theory" by noting those structures and institutions likely to converge among "modernized" societies, and those for which no such convergence is to be expected in any future that matters.

At the risk of trivializing a complex subject by reducing it to twenty-five words or less (and a facsimile box-top), the essential point is that social phenomena are to be understood, analyzed, and predicted as components of systems, but that interdependence is sequential as well as synchronic. It seems to me not very important at this point in contemporary theory to worry about whether all this is true or false to some supposed doctrine of structural-functionalism.

Lauer's references comprise no fewer than sixty titles, but if he wants to pursue the theory of social change he would be well advised to attend more closely the literature that is truly relevant to his expressed concerns.

WILBERT E. MOORE

University of Denver

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A COMMENT ON "THE SCIENTIFIC LEGITIMATION OF FALLACY"

Frankly, I am rather tired of reading articles like Robert H. Lauer's ("The Scientific Legitimation of Fallacy: Neutralizing Social Change Theory," *ASR*, October 1971:881-889), that take functional sociology to task for its supposed antipathy to social change. Furthermore, I have yet to read one that is convincing. Professor Lauer's is distinctive only in that he levies the charge not merely at functionalism but even more broadly at "much sociological thought" (Lauer: 881).

However, weariness gives way to irritation when I read a statement such as this, which is quite characteristic of the *genre*: "The theorist who works in the context of a 'nonchange' theory provides—willingly or not—ideological

basis for the status quo" (Lauer: 887). Oddly enough, that is itself a functional proposition, and one which could be asserted, with equal logic, for change theory as well.

Labeling the assumptions of nonchange theory as fallacies, Professor Lauer calls for a different set of theoretical premises—viz., "that change is normal; that change carries with it no intrinsic trauma; that diverse patterns of change and a range of future alternatives are open to any society; and that whether one assumes change or persistence as the basic reality has both theoretical and practical consequences of import" (Lauer: 887).

Now, suppose that just such premises *did* predominate in contemporary sociological thought. It would be easy to imagine the counterpart to Professor Lauer then writing an essay that concluded with a paragraph along these lines:

"The theorist who works in the context of a 'change' theory provides—willingly or not—ideological basis for the status quo. Incessant change is, of course, the *sine qua non* of modern commercial society. No traditional attachments, no desire for stability or continuity, no longing for respite from the constant demand to re-adjust, can be allowed to stand in the way of technological innovation, of the mobility of the factors of production, of the decline of old occupations and organizations and the rise of new ones. Obsolescence must be built into roles and institutions as well as into products if commercial forces are to hold sway as they do at present. Thus, the actor in this society must be immunized against future shock; he must be socialized into the beliefs that change is normal and not traumatic and that the future holds open a range of alternatives for more satisfying conditions than those he now enjoys. That is what change theory contributes to the maintenance of the status quo. Such a theory has many consequences of practical import; perhaps the epitome of these is the advertising industry, which seeks continually to create new consumer needs on the basis of premises that are no sounder than those which underlie change theory itself."

I would not care to argue that the connection between ideology and change theory expressed in that paragraph actually exists. But isn't it just as plausible as the one expressed in Professor Lauer's article? And if it is, can he offer any better evidence for the existence of *his* connection than that Chinese society changed at an "exceedingly slow rate . . . for hundreds of years prior to the twentieth century," while one Chinese author, writing in 1897 (!), did indeed say that change seldom

occurs (Lauer: 887). That is, the only evidence he offers, and it is somewhat weakened by the fairly substantial changes that did take place in Chinese society in the twentieth century.

ROBERT A. FELDMESSER

Educational Testing Service

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ON CLAMS AND CROWBARS: A REPLY TO MOORE AND FELDMESSER

Professor Moore's comment seems to me to focus on two complaints: (1) I did not read one of his books, and (2) on that basis I unjustly criticized or misrepresented him. He is wrong on both counts. I read and profited from his book some years ago. And the paper was not written vis-a-vis his own general position, but was directed at the assumptions that underlie much work on social change (including both non- and neo-functionalists). Whether Professor Moore believes that change is normal and ubiquitous is not the point. The question is, can the functionalist model—even with modifications—analyze social change? And does the particular modification offered by Moore, the "tension-management" model (Moore, 1963: 10-11), solve the problem?

This raises the question of the logical link between some of Professor Moore's assertions on change and the model which he briefly delineates. The model, incidentally, is not wholly new. As Smelser (1968:268f) pointed out, the position taken by Malinowski over fifteen years earlier could be called an "equilibrium-in-tension" model. This is an advance over the more common approach to social systems as systems in equilibrium; nevertheless, I have seen no theoretical developments over the past decades that persuade me of the utility of the model. But because the paper was not directed to the specifics of Professor Moore's thought, I did not raise such points in it.

Professor Feldmesser's comment is an interesting illustration of some of the points made in the paper. His hypothetical writer (who, I presume, reflects his own mode of thinking) assumes the intrinsic trauma of change. I have argued that no such intrinsic trauma exists. But this is not to say that there is no relationship at all between change and

trauma; as I pointed out in the paper, the *rate* of change does seem related to experienced stress.

Moreover, to assert that change is normal is not to legitimate or applaud every kind of change. The argument that whatever exists has some useful purpose is a kind of vulgar functionalism, but it is not my view. The frenzy of modern commercial society which apparently both Feldmesser and I find abhorrent is the kind of elaboration of an existing system which is quite in accord with a functionalist view of process. To assert the normality of change is to give hope of the negation of the contemporary frenzy.

Further, to assert the normality of change is not to deny continuity or stability. As Whitehead (1925:179) argued, the principles of change and conservation both inhere in reality: "Mere change without conservation is a passage from nothing to nothing," while "mere conservation without change cannot conserve." "Being" is not, therefore, illusory. Rather, being is defined in terms of becoming: "How an actual entity becomes constitutes *what* that actual entity is" (Whitehead, 1929:28).

With respect to the Hsueh Fu-ch'eng quote, it obviously was not meant to offer adequate support for my position. It is merely an apt expression of a mode of thinking that pervaded Chinese society. Chinese expectations regarding change—the societal past and future—may be found readily in a number of works (e.g., de Bary, Chan and Watson, 1960; Bodde, 1953; Marcus, 1961; and the American observer, Parsons, 1900). And the substantial changes of the twentieth century were effected by people who took pains to reject traditional views. Far from weakening my case, twentieth century changes strengthen it. New views about change emerged in China out of the despair generated by the tragic failures of the nineteenth century. An "expectation of progress" and a "linear conception of time" developed (Marcus, 1961: 138); this was given concrete content by Mao, and China broke the shackles of the past.

Finally, Professor Feldmesser notes his weariness with papers that assault nonchange theories. Indeed, mine was not the first, nor, I suspect, shall it be the last. How long shall they continue to be written? I would suggest an answer in the context of a saying offered by a Chinese thinker: don't try to open clams with a crowbar. The crowbar may be useful for some purposes, but it is obviously unwieldy and inappropriate for opening clams. Likewise, the assumptions which have often underlain the study of social change are simply not suited for the task at hand. Papers will be written,

therefore, until we have discarded the crowbars for more useful tools.

ROBERT H. LAUER

Southern Illinois University

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A COMMENT ON "POLITICAL ORIENTATION AND RIOT PARTICIPATION"

Jeffery M. Paige uses his Newark data to support the thesis that "radical political action depends on a combination of a strong sense of personal political competence (i.e., "efficacy") combined with a deep distrust of the political system" ("Political Orientation and Riot Participation," *ASR*, October 1971:810). Paige's interpretation of "trust" and "political system" as measured by responses to the question, "do you think you can trust the government . . . to do what is right?" needs modification (815). There may be low trust among the "high efficacy-low trust" activist group in Paige's paradigm, but the low trust may be in government officials rather than government itself. The same may be true of the other efficacy-trust types as well.

By not making this distinction, Paige overlooks a major finding of the Kerner Commission's analysis of black "counter-rioters" in all the major riots (Kerner, 1968:127-135). The Newark sample might well have included a

proportion of activists trying to quell the rioting. The Kerner Report described the typical counter-rioter as "an active supporter of existing institutions" (129). Several communities experiencing disorders gave evidence that counter-rioters were high in efficacy and political competence yet low in their trust of the government, thereby falling in the group Paige designates most prone to riot (819). Data on black counter-rioters from Cleveland, Columbus and Springfield, Ohio and Indianapolis, Indiana suggest varied counter-rioter types—opportunistic, moral, functionary and political (Anderson *et al.*, 1972). However, in none was high trust in local or national government to be found. Similarly, Warren's extensive sampling of low and middle income blacks and middle income whites in Detroit, revealed a high positive correlation between black groups' perception of the government's contribution to riots (e.g. "police brutality," "poor housing," "poverty"). The alienation of even middle income blacks from middle income whites was extreme, the Spearman coefficient being $-.865$, a significant inverse relationship (Warren, 1971:131).

These findings suggest that "trust" in government as Paige puts it does not sufficiently distinguish the "government" from those "governing" for a proportion of his sample. Hence, Paige's conclusion that "there is evidence that rioting can profitably be considered a form of disorganized political protest engaged in by those who have become highly distrustful of existing political institutions" needs to be modified (819). Government distrust is a universal phenomenon among blacks. Protest among the non-rioting proportion of Paige's four efficacy-trust types often assumes non-rioting forms. This suggests confidence in government institutions but not in government officials.

LEONARD GORDON

Arizona State University

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"REPLY TO GORDON"

The distinction between trust in the political system and trust in its functionaries is important in understanding many forms of political behavior, and similar distinctions have been made by Mitchell, Easton and Gamson. Gordon's use of the distinction, however, seems more directly applicable to his work on counter-rioters than it does to my article on rioters. Throughout the article and in fact in the passages quoted by Gordon I refer to the rioters' rejection of political institutions or the political system. Rejection of the political system implies rejection of the functionaries of that system since the former concept logically includes the latter. It is not likely that respondents retained their faith in government functionaries while rejecting the government itself. It is of course possible, as Gordon points out, that some respondents rejected the current government functionaries while retaining their faith in the political system. If, however, there were substantial numbers of these respondents in the high efficacy, low trust condition that I identified as most likely to riot, this would have *decreased* not *increased* the empirical association between distrust of government and riot participation. In fact Gordon argues that respondents who distrusted the government functionaries while trusting the political system would have collaborated with the authorities and become counter-rioters and certainly not actively participated in the riot. This group therefore could not have contributed to the empirical association between distrust of the government and active riot participation. In fact the only group who could have contributed to this association were those who rejected the political system including the system's functionaries. This is precisely what I argued and what the distrust of government item was intended to measure.

Gordon may well be right about the importance of distinguishing between system and functionaries in explaining the behavior of counter-rioters. I did not, in fact, discuss this group in my article on rioters, but not because I "overlooked a major finding of the Kerner Commission." This hardly seems likely since the analysis in question (Kerner, 1968:127-135) was conducted for the Commission by Nathan Caplan and myself, as Gordon would have discovered had he read the footnotes. The data reported there indicates that counter-rioters in Detroit were more likely to support the political system than either the rioters or the uninvolved. Thirty-nine percent of the rioters, sixteen percent of the uninvolved, but only three percent of the counter-rioters reported that they

did not think the country was worth fighting for in the event of a major war. (Kerner, 1968:178). Previously unpublished data from the same Detroit survey indicate that counter-rioters distrust white government functionaries as much as rioters. Forty-four percent of the Detroit rioters and 42 percent of the Detroit counter-rioters reported that they thought white officials had a great deal to do with causing the riot. Only 29 percent of the uninvolved expressed the same opinion. The Detroit data support Gordon's predictions for the attitudes of counter-rioters. I look forward to reading his own analysis of the problem.

JEFFERY M. PAIGE

University of California, Berkeley

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COMMENT ON "URBAN STRUCTURE AND THE DIFFERENTIATION BETWEEN BLACKS AND WHITES"

The recent research report by Jiobu and Marshall (1971) on Black assimilation into urban structures is a step forward in many ways. Yet I would like to point out a few problems that may lead readers to erroneous conclusions.

1. The path analysis may imply a causal sequence which in fact is not unidirectional. Though the variables in question were measured at dates which imply a time sequence, we know that most sociological variables do not vary randomly in time or space. The relative positions for cities on assimilation measures may have been rather constant over time, so that in prior periods the now dependent variables could have influenced the proposed independent variables. For instance, the relationship most at odds with Jiobu and Marshall's original hypotheses and the one the authors have most difficulty explaining is the negative relationship between black population increase and educational, oc-

cupational and income differentiation. This relationship is not difficult to understand if the cities' relative standing on educational, occupational and income differentiation have been stable over time. Assuming a rational model of human behavior, we would expect black migrants (higher in educational achievement than non-migrants on the average) to gravitate toward those urban areas which are, in fact, more equalitarian in opportunities relative to whites. In this case, the researchers used a time span from 1950 to 1969 to measure black population increase, so that even the 1960 assimilation measures could have influenced black immigration through 1969.

2. The method of operationalizing "black population increase" is also open to question. Usually population increase is related to a population's initial size. Jiobu and Marshall, however, subtracted the proportion of black urban population in 1950 from that proportion in 1969. A city whose black population increased from four to eight percent would be scored the same as one which increased from 34 to 38 percent (the mean increase was 4.19), though in one case the proportion doubled and in the other the proportion increased only 12 percent. It would be interesting to know how the correlations might differ with the second system of operationalizing black population increase.

3. The conclusion that ghettoization is a relatively minor factor in black assimilation may be misleading. The index of dissimilarity is so high (mean=38.48) and varies so little from one city to another (standard deviation=4.81) that we have *uniformly* high residential segregation. When high population density in limited geographical areas combines with rapid population increase, an explosive situation may result regardless of the high equality in education, occupation and income. Note that in the Kerner Commission Report (1968:158, 248) most cities classified as having "major" civil disturbances had had a doubling or even tripling in the proportion of black population between 1950 and 1965. Galtung's theory of aggression (1954) suggests that when groups are relatively high on one or more status dimensions (i.e., education, occupation and/or income) but low on others (i.e., housing opportunity) aggression is a likely outcome. Near equality on objective measures of education, occupation and income may make other subjectively felt inequities more intolerable. Thus, I question the conclusion that:

both in terms of its absolute contribution and in comparison with other variables, it (ghettoization) is a relatively minor factor. Consequently, we conclude that the emphasis given to area location as a determinant of black assimilation should be reconsidered. (p. 648)

On the contrary, ghettoization is not a minor factor, but we may consider whether it is an independent variable or itself indicates a lack of assimilation. Gordon (1964) emphasized that assimilation has many dimensions which do not necessarily change simultaneously. The dissimilarity index is useful precisely in its capacity to specify the *degree* of assimilation for a variety of dimensions. It can also indicate direction over time. I am hopeful that Jiobu and Marshall will expand the time dimension where possible to earlier census material and 1970 data, as well.

4. Though we have witnessed an advance in data on some structural properties of black assimilation, unless assimilation in education, occupation and income opens opportunities for increased social and cultural assimilation and better housing, we probably should not use the general term "assimilation," but rather report the degrees of equality or dissimilarity in specific areas: education, occupation, income, and housing.

5. Furthermore, these measures should be combined with a recognition that the expectations people bring to an objective situation are important to their satisfaction or frustration, and to subsequent action. Not that we require simultaneous opinion research and demographic analysis of the authors, just that they specifically recognize the importance of subjective feelings. Thus, if blacks expect greater equality in northern cities than in the South the inequalities felt in the North may be more frustrating.

I look forward to future research reports from Jiobu and Marshall in this important subject area.

SHIRLEY HARTLEY

California State College

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REPLY TO HARTLEY

Professor Hartley raises a number of important issues, and we welcome this opportunity

to clarify certain parts of our analysis. Her first comment is directed toward our use of "lagging" and the causal ordering of certain variables. She notes that the rank of cities on various measures of differentiation may not have changed significantly since 1950. If this is the case then little, if anything, is gained by the lagging technique. At the same time, we argue little is lost, although the theoretical basis for appropriate causal ordering becomes more critical.

She raises these questions in commenting on our interpretation of the unanticipated negative path coefficient between black increase and the three measures of differentiation. While her interpretation of this negative coefficient may be correct, we point out that in our ordering of variables black population increase was assumed to cause educational, occupational, and income differentiation. Consequently, the path coefficients presented in Figure 1 represent the impact of black population increase on these measures of differentiation. Since the values of partial regression coefficients will change if the dependent variable is transposed with an independent variable, it is really not correct to interpret them symmetrically as Professor Hartley has done.

Nonetheless, the general point is well taken and illustrates the always thorny problems involved in any causal analysis—ordering of variables and possible reciprocal influences. These problems are especially acute when a clear temporal sequence is lacking. Consider ghettoization; it may be the case that income, educational and occupational differentiation affect ghettoization as well as the reverse. We chose the order that appears in the Figure because the literature suggests that the predominant influence is from ghettoization to other dimensions of assimilation. In other words, we assumed the causal order implicit in the literature was correct and then examined those implications. Our entire causal structure, including the impact of black population increase, was predicated on this idea of predominant influence—we did not mean to imply that reciprocal influences are totally impossible. (An excellent treatment of this question can be found in Rosenberg 1968:9-13).

Moreover, this order was never intended to be definitive. Path analysis is not meant to "prove" that one variable does, or does not, cause another. Rather, it shows the types (direct, indirect, joint, spurious) and amount of effect that independent variables have on dependent variables, given the correctness of a particular causal structure. Indeed, our analysis questions the validity of regarding ghettoization as a major independent variable *vis-à-vis* other differentiation measures. As Duncan (1969:89)

observes, a major function of such models is to "... order the available information as well as it can be ordered in the present state of knowledge. The presumption is that improvements in knowledge will result in modifications and complications of the models."

As for Professor Hartley's criticism of our operational definition of rate of black population increase, it is true that a black population increase in one city of from 4 to 8 percent is different from an increase from 34 to 38 percent in another. It is equally true that with Professor Hartley's suggested operational definition, a city whose black population increased from 4 to 8 percent would be scored the same as one whose increase was from 50 to 100 percent, since in both cases the black proportion doubled. Yet, we may not wish to view these cities as identical.

The real question is which operational definition is relevant to the hypothesis. Our hypothesis was based on the presumed threat associated with a growing black population and the problem of absorbing relatively large numbers of newcomers. Though the point is moot, our measure appears more appropriate for this framework.

Our rationale is that the threat and other problems are the same since the relative racial balance would have shifted four percentage points in both cases. Thus, both would face the same magnitude of problems reallocating blacks and whites in the occupational structure, and in both, the white population may experience some threat. Of course, our analysis showed effects opposite to those anticipated, and Professor Hartley's *ex post facto* interpretation may be as valid as ours, and perhaps more interesting theoretically. In any case, our results cast doubt on the theoretical structure which yielded the hypothesis, and we may need to reconceptualize the theory and/or the operational definition.

In her third point Professor Hartley raises additional questions about ghettoization. She notes that the standard deviation of the Taeubers' index is small relative to its mean. This is certainly the case, but we are uncertain about how to interpret her subsequent remarks. She may be suggesting that the negligible impact of ghettoization is simply a statistical artifact resulting from the low coefficient of variability. If so we point to the relatively large (compared with the partial regression coefficients) zero order correlation coefficients between ghettoization and other variables in the model (Table 1 in Jiobu and Marshall, 1971:644). These range from $-.22$ to $.36$ and indicate that the small coefficient of variability does not prevent the Taeubers' index from systematically covarying

with other variables. Consequently, within the path model, we are still inclined to attribute the comparatively minor impact of ghettoization to the simultaneous control of other variables.

Or Professor Hartley may mean that we neglected to consider the causal import of uniformly high ghettoization levels on urban racial violence. Specifically, she suggests that ghettoization may affect various social and psychological states which lead to civil disturbances. While we might agree with her, the data do not bear on this hypothesis. Our findings are based on urban places and refer to the impact of ghettoization on other aspects of urban structure. The discussion of ghettoization is cast in the context of a structural model, whose data indicate that ghettoization is relatively unimportant.

Professor Hartley also asks whether ghettoization should be considered an independent variable or indicator of assimilation. This may be an arbitrary distinction, since a major premise of the study was that various assimilation dimensions are causally interrelated. From our viewpoint an assimilation dimension may be both an independent variable and an indicator of assimilation levels. Ghettoization can certainly be considered an assimilation dimension, though we did not use this term in the paper.

In her fourth point Professor Hartley suggests that the term "assimilation" not be used until social and cultural opportunities are more widely available to blacks. Though we would not object to this usage, until sociologists come to a greater consensus, researchers have a wide latitude in choosing terms. They are constrained only not to violate flagrantly the broad range of meanings associated with a term, and to make each definition clear and consistent. We took care to define "differentiation" and "assimilation" as clearly as possible, and to use the terms consistently.

Finally, Professor Hartley urges us to give greater emphasis to individual expectations, definitions of the situation, and feelings of satisfaction or frustration. Actually, the reverse may be argued with equal justification. That is, research based on individual perspectives should give greater emphasis to structural causes. The point we tried to emphasize (Jiobu and Marshall, 1971:638-639) is that concepts such as norms, values, prejudice and discrimination, may ignore the potential consequences of macro-level variables. We make the same point concerning the subjective states mentioned by Professor Hartley. We do not deny the importance of such variables, but merely suggest that explanations of many social phenomena are enriched by considering a variety of approaches.

In conclusion, we feel Professor Hartley's

comments are important and illuminate a number of issues which must be dealt with in research of this type.

HARVEY H. MARSHALL, JR.

Purdue University

ROBERT M. JIOBU

University of Wisconsin, Green Bay

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ERRATUM

In Professors Jiobu and Marshall's research report, "Urban Structure and the Differentiation Between Blacks and Whites," published in the August, 1971, issue of *ASR*, a typesetting error occurred on p. 643. The operational definition of rate of black population increase ought to be the difference between percent black in 1960 and 1950—not between 1969 and 1950.

ERRATUM

A number of errors in the tables and the text appeared in Terry N. Clark, "Community Structure, Decision-Making, Budget Expenditures, and Urban Renewal in 51 American Communities," 33 (August, 1968: 576-593). Most errors have been corrected in a revised and expanded version which appears in Charles M. Bonjean, Terry N. Clark and Robert L. Lineberry, editors, *Community Politics* (Free Press, 1971: 293-313). Additional corrections: In Table 3 the score for Cambridge, Massachusetts should be 5.00. In Table 5 $X_1 \cdot X_4 = .516$. Table 7: variance explained equals 40%. Table 9: variance explained equals 40%. Figure 4: Z_1 should read Z_2 ; X_1Z_2 path coefficient should be .341. Figure 5: Z_2 should read Z_1 . Path diagrams omitted from the *ASR* article also appear in the *Community Politics* version.

Items (*Continued*)

■ **H. Edward Ransford** is Assistant Professor of Sociology, University of Southern California. His current research interests deal with the interplay of race, class and alienation variables on political attitudes and behavior. He is currently writing a book on racial stratification.

■ **Stephen Cole** is Associate Professor of Sociology at SUNY-Stony Brook. His main area of research is the sociology of science. His most recent book is *Social Stratification in Science*, with J. R. Cole. **Robert Lejeune** is Assistant Professor of Sociology at Lehman College-CUNY. He is currently conducting research on people who have been mugged. He is editor of *Class and Conflict in American Society*.

■ **Tai Shiek Kang** is Assistant Professor of Sociology at the SUNY-Buffalo. His principal interests are in social psychology and mathematical models applicable to sociology. He will be taking advanced training in mathematics and statistics for

the next two years (1972-74) under an NIH post-doctoral fellowship. Currently he is completing a social psychological study of smiles.

■ **G. Edward Stephan** teaches theory and human ecology at Western Washington State College. His research interests are in the social determinants of boundaries of administrative units, and in certain problems associated with population distribution.

■ To the best of my knowledge, the three Indiana University authors in this issue did not withhold their articles from *ASR* until the editorship left Bloomington, Indiana; nor, for that matter, did my immediate predecessor make the editorial decisions which brought their articles to publication. We welcome them and all who have contributed to this issue, with thanks. Have a good summer, one and all. But keep researching and writing. We need your good service and your best work.

J.F.S.



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Meyer	Size and Organization	Willer Willer	Comment on Freese; Reply
Other Comments			

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5. Include an abstract of 100–150 words.

Format of References in Text

All references to monographs, articles and statistical sources are to be identified at an appropriate point in the text by last name of author, year of publication, and pagination where appropriate, all within parentheses. Footnotes are to be used only for substantive observations, and not for purpose of citation. There is no need for "*Ibid.*," "*op. cit.*," or "*loc. cit.*"; specify subsequent citations of the same source in the same way as the first citation. Examples follow:

1. If author's name is in the text, follow it with year in parentheses. [". . . Duncan (1959) has proven that . . ."] If author's name is not in the text, insert at an appropriate point the last name and year, separated by comma. [". . . some have claimed (cf. Gouldner, 1963) that . . ."]
2. Pagination (without "p." or "pp.") follows year of publication, separated by colon. [". . . it has been noted (Lipset, 1964:61–64) that . . ."] Incorporate within parentheses any brief phrase associated with reference. [". . . have claimed that this is so (but see Jones, 1952:99 for a conflicting view.)"]
3. With dual authorship, give both last names; for more than two, use "et al." For institutional authorship, supply minimum identification from the beginning of the complete citation. [". . . occupational data (U.S. Bureau of the Census, 1963:117) reveal . . ."]
4. If there is more than one reference to the same author and year, distinguish them by use of letters (a, b) attached to year of publication, in text and in reference appendix. [". . . as was previously suggested (Levy, 1965a:331) . . ."]
5. Enclose a series of references within a single pair of parentheses and separate by semicolons. [". . . as many have noted (Johnson, 1942; Perry, 1947; Lindquist, 1948) . . ."]

Format of References in Appendix

List all items alphabetically by author and, within author, by year of publication, in an appendix, titled "REFERENCES." Use no italics and no abbreviations. For typing format, see the following examples:

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THE SIGNALS OF '68: ALIENATION IN
PRE-CRISIS FRANCE

MELVIN SEEMAN

*University of California, Los Angeles*¹*American Sociological Review* 1972, Vol. 37 (August):385-402

Interviews with four hundred employed males in France, and with a parallel sample of American workers, provide the basis for characterizing the alienation of French workers in the spring which preceded the "events of May" (1968). Various forms of alienation (e.g., powerlessness; social isolation, etc.) are examined, along with their likely correlates (e.g. political knowledge; organization membership; left-right political stance). Neither direct participation in the rebellion nor changes in the level of alienation could be monitored, hence the argument rests on (1) a depiction of the prior pattern of alienation, and (2) the plausibility that this pattern lends to various interpretations of the May events.

The data suggest that "powerlessness" was the primary problem: (1) French workers, both manual and white collar, were conspicuously low in sensed control; (2) this powerlessness was expressed in a discriminating way (i.e., did not reflect a uniform French pessimism); (3) powerlessness, rather than work estrangement (or other alienations), correlated with the politically relevant variables (e.g., knowledge and membership); and (4) powerlessness (but not work alienation) was especially notable in the problematic political "center." The findings are interpreted in the light of propositions about the importance of alienation in collective behavior, and questions are raised about the significance often imputed to work alienation.

La France s'ennuie." The general boredom expressed so crisply in the pages of *Le Monde* in the winter of '67 erupted into a general rebellion in May of '68. Could anyone have predicted that political disengagement would so rapidly be translated into the passion and revolutionary threat of "the events of May?" Probably not, given the complexities of time, place, and manner in such explosive events, and given also the fact that the inattention of both social scientists and the public has regularly bred surprise: Berkeley, after all, was something of a bomb-shell; and later, there were few warnings about Watts.

Where prediction is so difficult, reconstruction is helpful. We have seen various forms of it for all these rebellions, ranging from journalistic accounts (Seale and McConville, 1968) and reports of investigating commissions (McCone, 1965), to post-factum surveys (Cohen, 1969) and retro-

spective interpretation (Touraine, 1968). What is presented here is still another kind of reconstruction—an attempt to discern the portents of the May rebellion in a pre-crisis study of attitudes among French workers in Paris.

The study was planned in late 1966 and carried out in the spring of 1967, just before the demonstrations that ultimately led to the May climax. Though the interviews were a year removed from May '68, it is well to remember that June of 1967 (im-

¹ This study was carried out under a grant from the National Institutes of Health (MH10460) for comparative work on alienation. The reception accorded me by Yves Delamotte and his colleagues at the Institut des Sciences Sociales du Travail was an indispensable help throughout a year's residence in Paris in 1966-1967. I want to acknowledge also the hospitality, intellectual and otherwise, of three persons who have contributed much to this work (though they might wish to disown that responsibility): Claude Faucheux, Serge Moscovici, and Alain Touraine.

mediately following our field work) brought a major demonstration in Berlin (where a student was killed); and the beginning of the French affair can be dated from the student strike in November of the same year on the outskirts of Paris at Nanterre.

What we have, then, is a "pre-test" of the temper of the French worker, a pre-test which can be read with the hindsight of what happened in May and in the light of prevailing interpretations of that happening. The French pre-test can also be read in a cross-national comparative way, the same basic study having been conducted with a sample of American workers at approximately the same time in Los Angeles. Since the study focused upon various forms of alienation, it provides some basis for examining in an empirical way one of the leading ideas regularly invoked to explain social movements like the May episode.

What is at issue is whether, and in what way, the idea of alienation provides some insight by "reading back" from the May revolt to the prior state of the society that produced it. We are interested not only in asking whether the level of French alienation was relatively high, but also (1) what kinds of alienation appear to have been most problematic, (2) how thoroughgoing the alienation was (in the sense of its penetration across class lines, or its comprehensiveness for a given individual), (3) what the level of alienation implied for political engagement or disengagement, (4) how it bore upon the worker's integration into organizational life, and (5) whether alienation was involved in the fundamental left-right polarization that characterizes French politics generally and the May events dramatically.

It should be evident that such an analysis can promise only to bring some firmness and clarity to the portrait of French alienation, and some understanding of its political correlates, immediately before the crisis. But the intrusion of that crisis makes these data all the more interesting, since they provide some basis for assessing the plausibility of various post-hoc interpretations of the events of May.

"De nouvelles aliénations." The interpretive importance of alienation can be seen in Touraine's view that the "movement of

May" represented a new form of class struggle arising out of the "programmed" post-industrial society and founded upon the new alienations in that society—especially among the teachers, technicians, students, young workers, journalists, and the like (Touraine, 1968; esp. pp. 14–26). Despite appearances, he argues, the principal actors were not the working class but the professionals; and the principal problem was not exploitation but alienation. His phrase "the new alienations" implies that these are distinctly plural (how "new" is another matter), and this is precisely the perspective that was embodied in the interviews obtained in Paris and in Los Angeles.

In both countries, there were some four hundred interviews with employed male workers, covering the full range of occupations. In the French case, the interviews were obtained by a quota sampling procedure designed to reproduce the occupational and age distribution known to be characteristic of the Paris circumscription. In Los Angeles, the interviews were based on random sampling within neighborhood blocks which had been selected so as to represent the range of socio-economic status and to establish controls for ethnic membership (avoiding Black, Oriental, Mexican-American, and Jewish concentrations).² Obviously, these two sampling procedures are not directly comparable, nor is it known how well they represent their respective countrymen, though (as will be seen below) there are grounds for suspecting that the French sample in particular is probably not distorting the overall French sentiment. The interviews were basically of the pre-coded variety; but in France, there were fifteen additional open-ended interviews (using stimuli taken from the pre-coded interview guide) to provide more fullness in

² The procedure for block selection in the Los Angeles case was substantially that described in greater detail in the report on the Watts study (Cohen, 1970; esp. pp. 480–481). The Paris interviews were conducted by a well-known French polling agency, SOFRES (Société Française d'Enquêtes par Sondage), whose personnel assisted in translation and in the training of interviewers. I am especially indebted to Pierre Weill at SOFRES for his knowledgeable assistance in fielding the study.

the materials and some check on the respondents' interpretation of the question.

The interview was designed to do three things: (1) derive indices of six kinds of alienation identified in the literature (Seeman, 1972)—e.g., feelings of powerlessness, self-estrangement, social isolation; (2) obtain more detailed measures of two of these forms of alienation—namely, powerlessness and alienation in work; and (3) isolate certain aspects of belief and behavior commonly thought to be related to alienation—e.g., organizational membership, political knowledge and interest in the political process, voting behavior, and ethnic hostility.

For the first of these three tasks, where considerable economy of measurement was required, the Middleton (1963) alienation scale was translated for use with the French sample. It is composed of six items (all presented in a "strongly agree" to "strongly disagree" format) intended to capture the meaning of the several brands of alienation:

1. (Powerlessness)—There is not much that I can do about most of the important problems that we face today.

2. (Meaninglessness)—Things have become so complicated in the world today that I really don't understand just what is going on.

3. (Normlessness)—In order to get ahead in the world today, you are almost forced to do some things which are not right.³

4. (Cultural estrangement)—I am not much interested in the TV programs, movies, and magazines that most people seem to like.

5. (Social isolation)—I often feel lonely.

6. (Self-estrangement in work)—I really don't enjoy most of the work that I do, but I feel that I must do it in order to have other things that I need and want.

For the second task, measuring powerlessness and work alienation in greater detail, two scales that have been tested in other studies were employed: namely, a thirteen-item variant of the Internal-External Control (powerlessness). Scale developed originally by Rotter (1966) and his co-workers; and a seven-item index of work

alienation (Seeman, 1967b) developed from a larger set of items presented in Blauner's study (1964) of industrial work. The powerlessness items used a forced-choice format (for example a choice between: "Many times I feel that I have little influence over the things that happen to me"; or, "I do not believe that chance and luck are very important in my life"). The work alienation items required Yes-No responses, each item carrying the implication that the respondent finds (or does not find) his work engaging and rewarding in itself (example: Can you do the work on your job and keep your mind on other things most of the time, or not?). Finally, for its potential comparative value, a French version of the well-known anomia scale developed by Srole (1956) was included. The scale has been widely used in the United States, and is intended as a measure of the individual's sense for a dependable social order characterized by interpersonal trust and clarity of normative demands (the items, however, are negative in phrasing—reflecting what Srole calls "self-to-other alienation").

Thus, in various forms and in various degrees of depth, we had ample measures of those alienations that might well have been significant in the events of May. Still, neither this extensive coverage of highly relevant attitudes, nor the fortunate pre-crisis timing, can rescue the fact that the causal inference (that these alienations were contributing symptoms of May) remains an inference. We do not know directly how the alienation of these French workers was related to their action or attitudes in May; and we do not know the secular trend of the attitude pattern revealed here—whether, in fact, the same level and pattern of alienation has been typical in France (or elsewhere) for decades.⁴

³ The French translations were achieved with relative ease and agreement among multiple translators. The normlessness item posed some difficulty in direct translation and emerged as follows: Pour réussir de nos jours, il ne faut pas être trop à cheval sur les principes.

⁴ Two provisos on these difficulties: First, they are hardly unique, the causal interpretation being not unlike the method in historical sociology or in much survey work (note, for example, the significance imputed to "relative deprivation" in numerous community studies; and the doubts that have been expressed about such imputations—e.g., McPhail, 1971). Second, the likelihood is great that the alienations documented here have, indeed, characterized France for some time; cf., for example, Cantril's *The Politics of Despair* (1958), and Touraine's (1966) evidence on French workers'

"*L'homme de la rue n'y peut rien.*" This conviction, expressed by a 45-year-old auto mechanic, that the man in the street can do nothing to influence the decisions of the government, expresses well the sense of low control that dominated the French scene in the spring of '67. How dominant this feeling of powerlessness was can be seen in the data of Table 1, where the responses to the six alienations in the Middleton scale are presented, controlling both for occupation (manual and non-manual workers) and for education.⁵

There are three striking features in these data:

1. In comparison with his American counterpart, the French worker feels notably more powerless, more doubtful about the standards of behavior in the society, and more constrained by his work. The differences on these matters are regularly on the order of 20-25 percent more alienated in the French sample. Among the less educated, almost 80 percent express the conviction that there is not much they can do about their problems!
2. This sense of low control is not very respectful of customary class lines (i.e., occupation-education): in France, the majority (roughly 65 percent or more) express the attitude of low control regardless of status level.
3. These differences might be attributed simply to response style, reflections of the

attitudes toward the role of chance in their lives (esp. pp. 145-147, 255-256).

Its long presence, however, is no argument against the significance of alienation in May, since (in my view) alienation alone can hardly be the full explanation of that event (or any other). The alienations in question always operate in a social context that includes (for example) political structures and histories, value considerations and economic circumstances, informal leadership, conjunctures of events (including the effect of Berkeley and its aftermath), etc. Thus, the level of alienation cannot in itself be a diagnostic or predictive social thermometer (though, unfortunately, some seek to use alienation in that oversimplified way). For a related view of the complexity that produces collective behavior, cf. Smelser's (1963) "value-added" approach, in which (as I see it) alienation is both an important indicator of "structural strain" and is embodied in various forms in all four of Smelser's major "components" of social action (values, norms, mobilization and facilities).

⁵ Middleton (1963) reports that these items form a Guttman scale (with the cultural estrangement item omitted); but that is not the case for the French sample (either taken as a whole, or for the manual and non-manual workers separately), nor is such a scale obtained in Los Angeles.

generalized French unhappiness and negativism that others have commented upon (e.g., Crespi, 1966). But the responses are *not* generalized; they are discriminating—which is all the more impressive given the fact that these six items appeared sequentially in the interview, thus by the timing and similarity of response demanded (the Likert format) one could expect a strong instrument effect. Table 1 shows that the French respondents are not uniform "negativists" and are quite discriminating in what they have to say. They do not find the complications of understanding the world today (item 2, Table 1) more frustrating than do the Americans (indeed, somewhat less so, especially among the relatively well educated); they are not more "turned off" by the media (item 4); and they do not especially complain about social isolation (item 5). That we are not dealing with a supposed cultural style of unhappiness is further suggested by the fact that better than 80 percent of the French sample express themselves as being "fairly satisfied" or "very satisfied" on a question concerning their overall job satisfaction. That figure is not very different from many U. S. samples, including the present one in Los Angeles; and not very different across the occupation and education categories in Table 1.

The force, the specificity, and the relative independence of social class that I have attributed to some of these alienations, and especially to the sense of powerlessness, can be seen in another set of responses within the interview (hence, in some degree, the reliability of powerlessness can be seen as well). Table 2 presents in full both the English and French versions of the Srole anomia scale, and the percentage of respondents who agreed with each item.

The French tend to agree more often with these pessimistic items (though they are not always more alienated than the American sample, and sometimes they do not differ by very much). But the striking difference—in its size and its consistency across class and education—occurs on the item that is most clearly an expression of political powerlessness (item 5, Table 2). In every instance, regardless of education or occupation, the majority in France expresses the powerless view vis-à-vis the public officials. The difference, in comparison with the U.S., is regularly better than 30 percent more in France choosing the alienated answer. Even among the well-educated white-collar workers, 52 percent of the French workers say that they find the public officials unresponsive to their

Table 1. Per Cent High in Six Forms of Alienation, Among French and American Workers, by Occupation and Education.

	<u>Manual</u>		<u>Non-manual</u>	
	U.S.	France	U.S.	France
<u>Low education</u>	(N=61)	(N=136)	(N=132)	(N=125)
1. Powerlessness (Not much I can do)	53	[76]	52	[78]
2. Meaninglessness (don't understand)	57	61	39	42
3. Normlessness (must do things not right)	30	[82]	31	[63]
4. Cultural estrangement (don't like TV, etc.)	36	30	45	31
5. Social isolation (feel lonely)	10	21	11	12
6. Work alienation (don't really enjoy work)	26	[68]	12	[50]
<u>High education</u>	(N=95)	(N=76)	(N=107)	(N=151)
1. Powerlessness	46	[64]	44	[64]
2. Meaninglessness	[50]	38	[28]	17
3. Normlessness	33	[60]	20	[59]
4. Cultural estrangement	34	29	50	45
5. Social isolation	10	15	9	18
6. Work alienation	30	[53]	6	[31]

Note: The percentages include those who responded "agree" or "strongly agree" for each item. The exact wording of the six items is given in the text. Differences between France and the U.S. of more than 10 per cent in the alienated direction (consistently so for both manual and non-manual workers) are enclosed in brackets.

problems (compared to 18 percent of the comparable group of workers in the U.S.).

The only other sentiment (besides powerlessness) in Table 2 which shows something like the same consistency and degree of alienation is found in the relatively widespread French conviction that "the lot of the average man is getting worse" (though here, educational level is a considerable moderating influence: less than one-third of the well-educated white-collar workers agree and "only" 37 percent of the well-educated manual workers, compared with 63 percent and 50 percent among the low-education workers). Like the feeling of powerlessness, this jaundiced view of the day-to-day prospect for the French worker (which embodies, one might guess, some element of powerlessness as well), can be counted on to appear in a reliable way. Thus, near the close of the interview the respondent was asked a question which was intended to re-test this prognosis concerning the worker's domestic welfare: "Do you have the impression that if the wealth of France increased, the worker's standard of living would also increase, or wouldn't that change very much?" The conviction

was widespread that the French worker would not share in any projected economic progress: among the less educated, 65 percent of the manual workers and 55 percent of the non-manuals said that an increase in French wealth wouldn't change their own economic situation very much; among the better educated, the pessimistic choice was made by 54 percent and 39 percent of the manuals and non-manuals respectively.⁶ These results—reflecting a fairly dismal assessment, but with more optimism among the better educated white-collars—are quite similar to those already presented concerning the "lot of the average man" in France (item 1, Table 2).

Perhaps it ought to be re-emphasized that both Tables 1 and 2 speak not to some generalized pall of discontent but to what we might call "selective alienation"—

⁶ Claude Durand (1971), from whose then ongoing study of French unionists this item was borrowed, both for its intrinsic interest and as a marker for comparison, reports a comparable level of pessimism. Among his CGT (Communist-affiliated) workers, 87 percent chose the pessimistic alternative on this question; in the next largest union group (the C. F. D. T.), the figure was 63 percent (cf., Table 34, p. 122).

Table 2. Per Cent Alienated (Agree or Strongly Agree) for the Five Items of the Srole Anomia Scale, Among French and American Workers, by Occupation and Education.

English and French Item Versions	Manual		Non-manual	
	U.S.	France	U.S.	France
<u>Low education</u>	(N=61)	(N=136)	(N=132)	(N=125)
1. In spite of what some people say, the lot of the average man is getting worse.	35	[63]	23	[50]
2. It's hardly fair to bring children into the world with the way things look for the future.	20	30	21	12
3. Nowadays a person has to live pretty much for today and let tomorrow take care of itself.	23	37	18	20
4. These days a person doesn't really know who he can count on.	49	65	38	47
5. There's little use writing to public officials because often they aren't interested in the problems of the average man.	38	[71]	20	[53]
<u>High education</u>	(N=95)	(N=76)	(N=107)	(N=151)
1. On a beau dire, le sort du français moyen est de pire en pire.	22	[37]	17	[31]
2. Vue la manière dont l'avenir se présente, cela ne vaut presque pas la peine d'avoir des enfants.	11	16	4	12
3. L'avenir est si incertain que la meilleure chose que l'on puisse faire est de prendre chaque jour comme il vient, et de ne pas se soucier de ce que arrivera le lendemain.	18	26	18	28
4. De nos jours, on ne sait plus sur qui on peut compter en cas de besoin.	33	46	24	31
5. Cela ne sert pas à grand chose de s'adresser aux responsables officiels parce que, le plus souvent, ils ne s'intéressent pas aux problèmes de l'homme de la rue.	27	[60]	18	[52]

Note: The French items are presented to show concretely the kinds of changes that were required (e. g., in item 5 --powerlessness before officialdom--the idea of "writing" to public officials does not fit the French pattern, hence the translation "to address," appeal, or direct oneself to the officials). Differences of more than 10 per cent in the alienated direction are enclosed in brackets.

mainly, extensive powerlessness tinged with a not very sanguine view of the chances for participation in the good life in France. And it also might be emphasized that though there are expectable differences across the socio-economic groups (the poorly-educated manuals do tend to be more alienated in some ways), neither education nor white-collar status moderates the feeling of powerlessness in any important degree. Even among the well-educated and the white-collar groups, the *majority* of the French workers feel powerless (differing notably from their American counterparts in this respect). We need to examine this issue of sensed low control in greater detail.

"*Le rôle international de la France.*" The

name of De Gaulle is almost synonymous with the idea of French grandeur; but that, too, was selective. To the old French centralism and bureaucratic ritualism, writes Touraine (1968; p. 35), De Gaulle added a purportedly "enlightened despotism" more concerned with the international role of France than with French society itself. Can this selective thrust, with all that it implies for the state of French internal affairs subject to such controlled neglect, be seen in the public attitudes about control that characterized Gaullist France in 1967?

The question implies that we should expect to find not an undifferentiated sense of powerlessness, but a discriminating set

of attitudes that matches the distinguishable internal and external realities of French politics at that time. The best place to look for evidence on the matter is in the replies our respondents gave to the forced-choice items of the I-E scale (Internal vs. External Control) since these items were intended to probe the person's sense of powerlessness or mastery in various domains of his experience (e.g., work, personal life, international affairs, etc.). Table 3 presents the I-E items for which comparative data are available, along with the endorsement frequencies (percent choosing the powerless alternative) for the U.S. and French samples.⁷

The classification in Table 3 (segregating the items that deal with foreign, internal, and personal affairs) is *ad hoc*, but it is also rather revealing. The differences between the U.S. and French samples (highlighted by the bracketed percentages in Table 3, where consistent differences of more than 10 percent in the alienated direction have been isolated) are systematic and discriminating:

1. On foreign affairs, the French have apparently been convinced by De Gaulle's attention outward. Both the manual workers and the non-manuals, in about equal degree, are more sanguine than the Americans about controlling the world situation and its main problem of peace and war.

2. On internal affairs—protecting one's own interests against organized pressure groups; influencing the government's decisions; getting a good job—it is quite the other way around: the French feel notably more powerless than the Americans and are much more convinced that the government

is not responsive. Apparently the relatively high powerlessness displayed earlier (in Tables 1 and 2) is, again, not simply a generalized French discontent or negativism; on that point, the discriminating pattern shown in Table 3 should be instructive. Only 20 percent talk the language of low control (powerlessness) when the question is whether wars between countries can be avoided; but 60 percent speak that language when it is a question of coping with pressure groups in France (item 4) and better than 50 percent when it is a question of whether one can really have an influence in the society (item 6).

3. It is also instructive (especially on the issue of response discrimination) to find that for some of these powerlessness items there is *no difference* between the French and American samples. On the more general items—i.e., those that involve fatalism about one's personal life being governed by chance, luck, or accident—the French and American workers are very similar (see, for example, item 9 in Table 3, where approximately 45 percent across the four groups affirm that they often feel they have little influence over the things that happen to them, which means that some 60 percent are rejecting the notion that chance and luck are "very important" in their lives).

In sum, the data thus far presented argue in a reasonable way about the character of the alienation in pre-crisis France. Far from reflecting a "one-dimensional man" (Marcuse, 1964), the data depict a French worker whose alienations seem to be remarkably distinguishable for him. He is quite aware of his own political powerlessness and is reliable in his report on it (it appears despite varying question format, and it appears early and late in the interview). But he is not thereby expressing a thoroughgoing alienation (nor even generalized negativity). His sense of social isolation, of cultural estrangement, of incomprehensible events in the world, of job satisfaction, or of generalized despair for the future are not particularly high—which is to say both that the percentage who affirm these alienations is not very great, and the difference between the French and U.S. samples is minimal.

The only other domain of alienation (besides powerlessness) that contends for a prominent place in the French perspective is work alienation (see especially item 6 in Table 1). Even here, however, it is not work alienation in general, but lack of con-

⁷ These items (a selected set from Rotter's I-E scale) have been used in various other studies (e.g., Ransford, 1968; Seeman, 1971). Though the total score customarily shows moderate reliability as an overall index of powerlessness, and the search for distinguishable sub-domains within the scale was not originally successful (cf., Rotter, Seeman, and Liverant, 1962; Rotter, 1966), there are clear signs that the person's sense of control can vary considerably as the domain at issue varies—cf., for example, the distinction between "personal" and "social" control (Seeman, 1967a), and the distinction by Gurin *et al.* (1969) between external control and "system blame." The treatment here (in Table 3 and elsewhere) reemphasizes the need for attention to these sub-domains of powerlessness, as well as better conceptualization and measurement of them.

Table 3. Per Cent Choosing the Alienated (Powerless) Alternative on Items of the Internal-External Control Scale, Among French and American Workers, by Occupation.

Item and Domain of Control (Powerless choice circled)	Manual		Non-manual	
	U.S. (N=156)	France (N=209)	U.S. (N=239)	France (N=274)
<u>Foreign (international) affairs</u>				
1. (A) Wars between countries seem inevitable despite the efforts of men to prevent them.	[66]	20	[65]	29
B. Wars between countries can be avoided.				
2. A. A lasting world peace can be achieved by those of us who work for it.				
(B) There's very little we can do to bring about a permanent world peace.	[43]	30	[45]	32
3. A. By studying the world situation, one can greatly improve his political effectiveness.				
(B) Whether one likes it or not, chance plays an awfully large part in world events.	[32]	21	[29]	18
<u>Internal (French) affairs</u>				
4. (A) Persons like myself have little chance of protecting our personal interests when they conflict with those of strong pressure groups.	45	[62]	42	[60]
B. I feel that we have adequate ways of coping with pressure groups.				
5. (A) It is only wishful thinking to believe that one can really influence what happens in society at large.				
B. People like me can have an influence in the world if we make ourselves heard.	23	[52]	25	[54]
6. A. The average citizen can have an influence on government decisions.				
(B) This world is run by the few people in power, and there is not much the little guy can do about it.	26	[41]	26	[42]
7. A. Becoming a success is a matter of hard work; luck has little or nothing to do with it.	23	[42]	25	[40]
(B) Getting a good job depends on being in the right place at the right time.				
<u>Personal (general) affairs</u>				
8. (A) Most people don't realize how much their lives are the result of accidental happenings.	61	52	65	60
B. There is no such thing as "luck."				
9. (A) Many times I feel that I have little influence over the things that happen to me.	45	44	39	37
B. I do not believe that chance and luck are very important in my life.				

Note: The education breakdown in Tables 1 and 2 has been omitted here for simplicity of presentation; it does not alter the pattern of relationships shown here. Differences of more than 10 per cent in the alienated direction are shown in brackets.

trol in the work sphere that seems to be the main issue. This is made evident in Table 4, where responses to the items of the "work alienation" scale are displayed. These items allow us to examine the specifics of the complaint about alienated labor. The data of Table 4 make clear that the special complaint in France is not that the work fails to use the worker's skill, or that it is too routinized, or allows no opportunity for

expressing himself in it. The trouble is that he has little control over the work tempo (cf. items 1 and 8 in Table 4)—which is to say again that the feeling of powerlessness is the prominent problem.⁸

⁸ The first seven items in Table 4 constitute a "work alienation" scale that was developed through factor analysis of Blauner's (1964) work items. For further data on this scale, and on the "control" question included as item 8 in Table 4, see

Table 4. Per Cent Choosing the Alienated Alternative on Items of the Work Alienation Scale, Among French and American Workers, by Occupation.

	Manual		Non-manual	
	U.S.	France	U.S.	France
1. Would you say that your job makes you work too fast most of the time, or not? (% "too fast")	25	[48]	22	[42]
2. Is your job too simple to bring out your best abilities, or not? (% "too simple")	26	27	17	21
3. Does your work really give you a chance to try out your own ideas, or not? (% "no")	26	31	11	13
4. Can you do the work on the job and keep your mind on other things, or not? (% "yes; other things")	40	45	32	37
5. Is your job interesting nearly all the time, interesting most of the time but with some dull stretches, or pretty dull and monotonous most of the time? (% "mostly dull")	18	27	6	15
6. If you had the opportunity to retire right now, would you prefer to do that, or would you prefer to go on working at your present job? (% "retire")	38	47	31	39
7. On an ordinary workday, do you have the chance to make independent decisions in your work, or is it rather routine work? (% "routine")	30	37	11	24
8. How much control would you say you have personally over the work pace--a great deal of control, some control, or very little control? (% "very little")	15	[34]	7	[42]

Note: The education breakdown is omitted for simplicity in the presentation. The pattern of substantial differences on items 1 and 8, and similarity across countries on the other items, holds for the high and low education groups. Differences of more than 10 per cent in the alienated direction are enclosed in brackets.

the results of an investigation in Malmö, Sweden (Seeman, 1967b).

Three points need to be noted without elaboration. First, it would be more precise to say that, comparatively speaking, control at work is the problem, since Table 4 emphasizes the French-American comparison. In an absolute sense, more intrinsic concerns at work (the "mindlessness" of it, for example; item 4) get substantial notice, too—and, of course, in both countries the manual workers regularly experience these alienative features at work more than the non-manuals.

Second, the unhappiness about work alienation (in both Tables 1 and 4) compared with the much more positive replies concerning "job satisfaction" conforms with the view that these two concepts are not equivalent (and with similar relationships reported for other samples; cf., Seeman, 1971).

Third, it is possible to argue that 30% of the population undergoing work-disaffection is potentially more explosive than 75% experiencing powerlessness. But that is a dubious speculation, indeed (given the thrust and the limitations of the available comparative data), and it is a speculation made even more dubious by showing (as we proceed to do next) how these alienations respectively correlate with politically relevant aspects of French society.

"Soit à gauche soit à droite, il faut faire de la politique." The 35-year-old engraver who urged engagement in political life, whether on the political left or right, establishes another dimension with which our analysis must deal. To show, as we have, that alienation in France was sharply focussed, chiefly on feelings of powerlessness within French society itself, and that this powerlessness was widely felt among French workers of high and low estate does not establish the potential significance of such alienation in May of '67 (much less '68).

It is necessary to show that these feelings of alienation have political and quasi-political consequences or correlates that make sense as conditions for the political explosion that followed. The young engraver quoted above, who made clear that he is not among the plurality who feel powerless, is a partial guide to the correlates or consequences that must be examined: he has been a member of a labor union for ten years; he is interested in and knowledgeable

able about politics; he is a regular voter; he is not doubtful or centrist politically, having made his own choice "à gauche" (he calls himself a moderate leftist); he is more or less integrated into an occupational community of friends who share similar work interests; and he finds his work rewarding in itself. The question is whether over the sample as a whole, the unalienated workers differ in these ways from those who feel powerless—and, equally important, on what features they do *not* differ.

Table 5 helps to validate the fact that the engraver typifies an important political pattern. The six variables shown in Table 5 are all predictable correlates of the widespread powerlessness that was so clearly the norm in France. A word about each of these measures may help in the interpretation:

1. The interview opened with a series of questions about the respondent's degree of

interest in various activities (e.g., discussing politics; talking with others about one's work; etc.), and the percentage shown here indicates those who chose "not interested" (over "rather" interested or "very" interested) in political discussion.

2. Immediately preceding the field work, there had been a legislative election (in March of 1967), and the respondent simply indicated whether he had participated in that election or not.

3. On a modified Bogardus scale, three gradations of social distance toward Jews could be expressed—acceptance (or not) as a work colleague, as a school friend of one's children, and as a marriage relative. The respondent had four response alternatives to indicate his degree of acceptance of the relation (he could be "pleased" to have such a relation with a minority member; "neither for nor against it"; "rather unhappy" with it; or "very unhappy"). The percentage shown in Table 5 draws the line quite sharply: any expression of discontent on any of the three possible social relations with Jews was scored as anti-Semitism (which

Table 5. Some Correlates of Felt Powerlessness Among French Workers, by Occupation and Education.

	Manual			Non-manual		
	Powerlessness High	Powerlessness Low	Gamma	Powerlessness High	Powerlessness Low	Gamma
<u>Low education</u>	(N=72)	(N=60)		(N=74)	(N=48)	
1. Express no interest in political discussion	64%	50%	.32*	51%	21%	.62*
2. Report not voting in last election	25%	10%	.50*	18%	13%	.20
3. Score high in anti-Semitism	81%	73%	.20	78%	71%	.19
4. Score high in total prejudice	61%	48%	.24	69%	50%	.37*
5. Score low in political knowledge	78%	65%	.31*	53%	46%	.14
6. Report no work organization membership	74%	63%	.24	64%	56%	.15
<u>High education</u>	(N=35)	(N=38)		(N=76)	(N=68)	
1. Express no interest in political discussion	40%	21%	.40*	33%	19%	.38*
2. Report not voting in last election	15%	11%	.19	24%	14%	.29
3. Score high in anti-Semitism	89%	66%	.60*	77%	76%	.02
4. Score high in total prejudice	51%	43%	.16	52%	39%	.26
5. Score low in political knowledge	38%	49%	-.21	38%	17%	.48*
6. Report no work organization membership	63%	37%	.49*	66%	44%	.43*

* Indicates that the associated chi square was significant at the .05 level of significance (or beyond). The positive gammas show the degree of association that obtains between high powerlessness scores and high political alienation of one kind or another (e.g., non-voting; expressed disinterest; ethnic hostility; etc.).

explains, of course, the high percentages shown there).

4. The sum of the respondent's social distance toward four foreign groups (Americans, Negroes, Jews and Algerians) constituted the "total prejudice" score (the high-low breakdown for this score having been made at the median of the distribution).

5. Twelve factual true-false questions about international and French politics comprised the political knowledge test (see Table 6 for the content of these questions).

6. Respondents were asked whether they belonged to an organization associated with their work ("Faites-vous partie d'un organisme professionnel: syndicat, ou groupement de défense de votre profession?"). As Table 5 suggests (line 6), the majority (as in the U. S.) are not members—the overall figure for the present sample was 39 percent. That figure may well be somewhat inflated, partly because the very idea of what constitutes membership is more difficult to determine in France where the union's role (politically, structurally, and in relation to bargaining processes) is very different from the American one.⁹

Table 5 shows quite clearly that the extensive sense of powerlessness in France was no free-floating or innocuous attitude. It was an element in the political order and in the social process. The data indicate that the sense of low control was associated with disinterest in political talk, absenteeism in voting, and objective lack of knowledge about political affairs; and powerlessness was associated as well with anti-Semitism, general ethnic hostility, and isolation from organizational ties in the work sphere.

It is fair to ask whether my emphasis on the role of powerlessness as against the other alienations (e.g., work alienation) is not, in fact, an over-emphasis. But how would one know? Some clues can be provided by applying the logic of Campbell and Fiske's (1959) multi-trait measurement. If powerlessness is not simply generalized discontent but the relatively specific and basically political phenomenon it is represented to be through the interpretation of Table 5, it ought to be possible to show that powerlessness does not correlate with some things it should not be es-

pecially tied to—e.g., some kinds of discontent, and some kinds of social engagement. Further, one should also show that other alienations which are prominent candidates as predictors of political action—especially work alienation, in the present case—are *not* correlated with the socio-political variables in Table 5. In short, we need to know that the correlations in Table 5 are discriminating (just as we needed earlier to know that French opinion about the several alienations was discriminating).

Though a full account is not possible here, the necessary tables have been constructed, and they reveal that the powerlessness variable is discriminating in the way it should be. Powerlessness does not correlate, for example, with the person's degree of interest in discussing his work (the gamma of .62 for non-manuals in the present Table 5, between powerlessness and interest in discussing politics, drops to .11 when work interest is substituted as the dependent variable); nor does it correlate with job satisfaction, status-mindedness, or the like. Further, when work alienation is substituted for powerlessness as the independent variable, the significant correlations with organization membership (.49 and .43 in Table 5) disappear; and in the same way, political knowledge is not correlated with work alienation—nor should it be, if political knowledge is a matter of mastery (powerlessness) rather than intrinsic engagement in work.

In sum, the evidence indicates that the high level of French powerlessness was channeled in essentially political ways: at least, it was apparently attuned to politically-relevant interest, action (voting), attitudes, knowledge and organization (to follow the order in Table 5); and the same cannot be said for the person's sense of alienation in work.¹⁰

⁹ For a similar, and not unrelated, point about the variety of meanings that are given to membership in the Communist Party, cf. Kriegl (1968; esp. Chapter VII: Les degrés d'adhésion, pp. 117-126).

¹⁰ Contrary to what some might expect—those who think that the integrative, unalienated (i.e., supposedly positive) experiences are "packaged" together—membership in an occupational community (like work alienation) has little to do with either the sense of powerlessness among these French workers, or with its politically relevant correlates (e.g., political knowledge or ethnic prejudice). These data on the independence of occupational community suggest again (as do the results on work alienation itself) how removed

"*Le Marais, arbitre du pouvoir.*" If the feeling of powerlessness was prevailing and politically embedded (i.e., had political correlates of some consequence), as has been demonstrated, it remains to be shown that it was also embedded in the operating structure of French politics. The clearest dimension of that structure is the left-right polarization, but with a substantial centrist influence as well. The centrist has been the object of careful scrutiny quite recently—Deutsch, Linden and Weill (1966) call them the "floating electorate." They argue that the center is not a true member of the "political families" in France, insofar as it largely consists of politically isolated and disinterested people with no identifiable or consistent ideological stance. Deutsch *et al.*, attach the name "marais" (literally a swamp or morass) to this group of fluid and inconstant voters: as a stable center they are an illusion; as a shifting contributor to the majority, they are the arbiters of power.¹¹

On such a self-classification of political tendency, (ranging from "extreme left" to "extreme right"), a remarkably stable 30 percent of the French electorate have classified themselves as "center" in six separate polls between June 1964 and May 1966. Deutsch *et al.*, report 31 percent of a national sample classifying themselves as centrists in May of '66; and one year later, in the present investigation where the same self-classification question was asked, parallel figures were obtained: 30 percent of the manual workers and 31 percent of the non-manuals called themselves centrists.

Deutsch and his colleagues speak of the "ill humor" and "diffuse discontent" of the

marais (pp. 64–69), and they write: "... because it is not informed and not politically structured, it is dangerously unstable" (p. 87). How badly informed the political center was in May of '67 can be readily seen in the data of Table 6, where the twelve items of the political information test are presented (those concerning French politics having been separated from those dealing with international affairs, for ease of examination). The lack of political sophistication among the centrists is rather remarkable. These questions were designed and pre-tested with the thought that the test should not be too difficult, yet in numerous instances the majority (even 75 percent!) of the "center" cannot answer correctly. As one might expect, the white collar workers are better informed (being better educated as well), but even there the discrimination between the knowledgeable "left" and the relatively uninformed "center" is consistent (and not dependent on educational differences between them, those differences being minimal in any case).

The uninformed center is also, relatively speaking, the non-participating and unintegrated center. Where only 9 percent of the manual "left" report themselves as non-voters in the previous election, 25 percent of the manual "center" admit (or should one say "claim"?) non-voting—and the figure is even higher (33 percent) in the true "marais" (the smaller sample which designates itself both "center" and "disinterested" in politics; $N = 39$ among the manual workers, and 34 among the non-manuals). The lack of integration shows clearly in the membership figures: 50 percent of the manual "left" are not members of a worker's organization; but 78 percent of the manual "center" are unorganized (and the figure rises to 85 percent in the "marais"). Even among those centrists who are members, the organization is less meaningful (64 percent of the left say that their membership is personally quite important for them, and 43 percent say that the members have considerable influence in the organization; the comparable figures for the center are 43 percent and 21 percent respectively). And if one asks, as we did, who *ought* to make the decisions in public affairs ("in fact, the

work experience may be from the center of the problem. The data are congruent with what has been found in both Sweden and the United States (cf., Seeman, Bishop and Grigsby, 1971).

¹¹ The great bulk of the centrists are in the "marais," which is defined by (a) self-designation as a political centrist, and (b) answering "very little" or "not at all" when asked how much interest one has in politics. Almost 70 percent of the centrists generally qualify for the "marais" (cf., Deutsch *et al.*, 1966; esp. pp. 15–19). In the present sample, the same is true: Among the manual workers, for example, 66 percent of those who called themselves "center" expressed little interest in politics; for the "left," the comparable figure was 38 percent.

Table 6. Percentage Answering Incorrectly on Political Knowledge Items Concerning French Politics and International Affairs, by Political Self-Designation ("Left" or "Center") and by Occupational Category.

Item	Manual		Non-manual	
	Left (N=118)	Center (N=58)	Left (N=120)	Center (N=78)
<u>French politics</u>				
1. The present Minister of Education in France is Edgar Faure. (False)	39	78*	29	41
3. France is presently engaged in its 5th Plan. (True)	13	27*	11	6
5. In France, the Premier Minister is elected by the Chamber of Deputies. (False)	50	71*	25	40*
7. The CGT is the French worker's union having the largest membership. (True)	7	23*	5	17*
9. It was Mendes-France who put an end to the French war in Indochina in 1954. (True)	33	44	21	42*
11. The current Minister of Finance is Giscard d'Estaing. (False)	36	56*	17	22
<u>International affairs</u>				
2. The present president of the U.S. (Johnson) belongs to the Democratic party. (True)	29	48*	27	43*
4. The Labor Party is currently in power in Great Britain. (True)	18	44*	13	19
6. In Greece, the communists have just now taken governmental power. (False)	29	53*	12	28*
8. Switzerland is one of the six countries included in the Common Market. (False)	29	44*	12	14
10. The five permanent members of the Security Council in the U.N. are: the U.S., Russia, France, Great Britain, and Nationalist China. (True)	62	61	55	71*
12. Communist China is the only power that has refused to sign the accord halting atomic tests. (False)	44	70*	38	44

* Indicates that the associated chi square was significant at the .05 level or beyond. The item numbers are those used in the interview proper, the French and international items being alternated. The self-proclaimed "right" has been omitted, since the number was quite small (N=24 among the manual workers).

important social and political decisions ought to be made by the specialists and experts"), almost everyone in the "center" opts for letting the experts do it (87 percent of the manual "center" are expert-oriented; 89 percent of the non-manual "center").¹²

These are the signs of behavioral rootlessness in the political center—their lack of political knowledge; their disproportionate non-voting; their non-membership; their

readiness to escape political responsibility. But what of their alienation? Was it, indeed, a matter of diffuse discontent; or was it, as the thrust of the earlier argument would suggest, considerably more focused than such an image of thoroughgoing alienation would imply?

Most important, in light of what has gone before, the center is where the sense of powerlessness is sharpest. On the crucial questions that deal with the sense of control in French life, the center is notably higher in powerlessness than the left—and, often enough, the percentages reach overwhelming proportions. This concentrated powerlessness can be illustrated using an item from each of the first three tables presented above (which is to say, using powerlessness items in various formats deriving from various portions of the interview):

¹² Holding education and occupation constant, the Los Angeles sample was regularly much less inclined to accept the dominance of expert decision than the Paris sample. Among the well-educated white collar workers, for example, the percentage who agreed with the item was 37 percent in Los Angeles and 61 percent in Paris; among the poorly educated manuals, the respective percentages were 46 percent and 78 percent.

1. (From Table 1, the Middleton scale)—To the statement, "There is not much I can do about most of the important problems we face today," 88 percent of the manual centrists agree; on the left, 69 percent agree.

2. (From Table 2, the Srole scale)—To the statement, "There's little use writing to public officials . . .", 59 percent of the non-manual centrists agree, and in the marais proper 82 percent agree that contacting the officials is useless; on the white-collar left, 51 percent agree.

3. (From Table 3, the I-E powerlessness scale)—The alternative, "It is only wishful thinking to believe that one can really influence what happens in society at large," is chosen by 65 percent of the manual centrists, against 46 percent for the left.

Was this attenuated sense of control in the political center part of a syndrome of alienations that would signal a deep and diffuse disaffection? The answer is that no such syndrome appears. The center is not distinctively alienated or disaffected in the many other ways that it could be:

1. The center does not differ from the left in their sense of alienation in work.

2. Concerning loneliness and social isolation, there is no difference between the center and the left (across all of the relevant sub-groups—manual and non-manual; center and left—the percent who complain on this score ranges only between 18 percent and 21 percent).

3. To the extent that cultural estrangement is tapped by the item intended to do so (item 4, Table 1), the center and the marais are, if anything, *less* estranged: 35 percent of the center, but 57 percent of the left, reject the going media fare.

4. There is no difference in the level of job satisfaction among these groups (81 percent of both the centrists and the leftists express satisfaction).

5. Even on some kinds of powerlessness, the center and the left agree—for example, on the question of war and peace (item 1, Table 3) the difference is minimal (18 percent are fatalists among the manual left; 19 percent in the center); and on the importance of "luck" in general (item 8, Table 3), the fatalists are evenly distributed, too (52 percent and 58 percent in the manual left and center).

But when we return to the specifically political scene, the malaise is obvious enough. On the question of whether the French worker would be likely to participate in any future gains made by the French economy, 74 percent of the manuals in the "marais" say that they do not think the

worker would share in the improvement, and 70 percent of the non-manuals take that dim view as well. Asked whether the Plan in France is important to their welfare, 63 percent say that it is not (the figure is 44 percent on the left); and when asked whether the Plan provides them with some degree of influence in French affairs, 81 percent of the "marais" say "No" against 61 percent on the left). Thus, the malaise—among the powerless, and especially in the center—extends, as we have seen, from knowing very little, to wanting very little, to expecting very little, and feeling able to do very little in the political arena.

"Conclusion: *ce n'est qu'un début*." This theme ("It's only the beginning") was the slogan of the rebellion in the streets of Paris. It also introduces and ends Touraine's analysis (1968; pp. 12, 274), since he views the May rebellion as the opening of a new period of class conflict in which those who work for the "techno-bureaucracy"—the engineers, journalists, teachers, researchers, technicians, radio-T.V. men, students, bureau staff, and assorted other white-collar personnel—will play a commanding role.

In a more limited sense, the theme is appropriate here, too, since the particulars that have been presented, despite the calculated detail, are only a beginning in seeking an explanation of the May event. They are only a beginning for various reasons, not least of all because the focus here is so heavily on the social-psychological temper of the time, rather than on the historical circumstances that led to May, the institutional arrangements that contributed their part, the strategies of leadership and counter-response that fed the crisis, or the complex of political interests and agents that were involved.

Nevertheless, the analysis allows some very explicit gains in clarity, and advances substantial evidence concerning inferences (and counter-inferences) that are regularly made about the French episode. There is, first, the clear evidence of the reservoir of alienation that existed in France at that time—but, most especially, evidence concerning both the force and specificity of that alienation. The evidence suggests that Touraine is probably right to argue that in

the emerging society "the awareness of exploitation is replaced by the awareness of repression and alienation" (1966, p. 22), but probably wrong to argue that the sentiment of alienation is "more diffuse" (p. 43). The sentiment of alienation depicted here is remarkable for its lack of diffusion. It is the sense of powerlessness that emerges as the key form of alienation in pre-crisis France; and this powerlessness was *not* diffuse in two senses: (1) it was not highly correlated with a variety of other alienations; and (2) it was discriminating, in that it focussed upon French society itself as the arena of low control.

Though it hardly constitutes proof, this kind of evidence concerning the force and form of powerlessness is consistent with Touraine's contention that the key feature of the movement of May was not the cultural agitation, or the university rebellion, or the moves for modernization in France, but "the appearance in the heart of the society of a new contest for power" (1966, p. 12). It is also consistent, in its emphasis on powerlessness, with Seale and McConville's interpretation of the unrest as it gained headway in the professional classes: "... it was a revolt against ruling bureaucracies, administrative machines, professional apparatuses. It found expression in an urge to run one's own affairs..." (1968, p. 226).

Second, the data raise some very serious questions about the prevalent attributions about how crucial alienation in work was in the genesis of May (and in similar events). Thus, Singer (1970)—whose emphasis on the importance of control over one's destiny generally conforms with the view elaborated here—writes: "Work is a crucial part of (man's) life and the French spring of discontent revealed how much dissatisfaction there was with the organization of work in the apparently privileged sections of society" (p. 24); and what struck a chord with French workers was the wider message of the students that "artificial" leisure could not "compensate him for the frustrations of alienated labor" (p. 88). The present data (especially when taken in conjunction with the parallel data from other countries) suggest that alienated work is not the primary issue—or better,

that *control* at work rather than *self-fulfillment* at work may be closer to the issue.

Third, the data show an unusual degree of cross-class similarity in the level of alienation (powerlessness) in France. To be explicit, where 76 percent of the manual workers say that there is "not much they can do," 78 percent of the white-collar workers agree; and even among the better educated 64 percent of both groups agree (Table 1). Where 60 percent of the better-educated manual workers say there's no use seeking redress from public officials, 52 percent of the white collar workers hold the same opinion (Table 2). Where 62 percent of the manual workers say they have little hope of protecting their own interests against pressure groups, 60 percent of the white collar workers agree (Table 3). Full-scale participation of the white-collar segment was one of the notable features of the action in May ("the working class was not the principal actor"; Touraine, 1968, p. 25), and though the present data would hardly allow one to predict "... the remarkable penetration and intensity of protest feeling and activity" (Converse and Pierce, 1971, p. 17) it makes it retrospectively less surprising. A popular thesis, of course, is that increasing white collar discontent follows upon their increasing alienation in work—they become, in this regard, more like the proletariat; but here, be it noted, attention is focussed on the cross-class similarity (high powerlessness) outside the work sphere.

Fourth, the data show that this powerlessness was, so to speak, politically engaged and consequential. And not only that: it shows that the other alienations that have been named as culprits in affairs of this kind were not engaged with anything like the same consistency as was the sense of powerlessness. To say that powerlessness was "engaged," is to say that it was correlated with things that make a difference politically: knowledge; interest; membership; voting; prejudice; and other politically potent attitudes (e.g., attitudes of retreat before experts and or resentment of their restriction of equal participation in socio-economic gain).

Fifth, the "dangerously unstable" political center was uniquely vulnerable in the

extremity of its sense of powerlessness (and the correlates thereof—e.g., political ignorance). If the center is the arbiter of power, it would take their participation to produce the scenario that developed in May of 1968—including both their initial participation, and their eventual retreat from the precipice in the pro-Gaullist electoral landslide that sealed the affair in late June. That the center was active in this way is not entirely a matter of conjecture based merely on their known instability and their proven sense of powerlessness. In their excellent study of the cleavages in French politics before and after the May-June disorder, Converse and Pierce (1970) remark:

... the *marais* was remarkably likely to participate, or remarkably unlikely to, according to whether or not demonstrations were occurring on their door-steps. In the 'hottest' districts, demonstration participation by the *marais* was substantial, not lagging too far behind that of avowed leftist partisans themselves. It is clear that although hardly politicized, they were quite willing to join the excitement sweeping by them, and the crowds in the streets contained large proportions of people whose normal involvement in politics was thin to non-existent (p. 23).

The *marais*, or some sizeable portion of it, presumably participated and fell back, having lived with their powerlessness before and with other movements and defections before (Petain in 1940 and Poujade in 1956, perhaps; vacillation between De Gaulle on the one hand, and Lecanuet and Mitterand on the other in 1965).¹³

Finally, these data establish the relevance of organizational membership. Though union structure, role and membership in France are different in many ways from unions elsewhere (cf., for example, Crozier, 1964), the French organizations are not

different in one critical way: membership breeds and/or attracts people who think they *can* exercise control. Not simply control in general, but control over the decisions about French society. If we look exclusively at one of the occupational groups known to have been more active than most in the demonstrations and strikes in May, the *cadres moyens* (middle-level white collar workers), we find that they were predominantly unorganized (58 percent were non-members; N=91); but, more interesting, the overwhelming proportion of those who were organized express attitudes of mastery not powerlessness: fully 78 percent of the organized *cadres* say that "the average citizen *can* have an influence on government decisions" (see item 6, Table 3); among the unorganized, 55 percent choose this mastery alternative (i.e., 45 percent choose the powerless alternative).

How this relative sense of mastery among the integrated workers, and powerlessness among the unorganized, contributed to May and June can be surmised but not documented. It would be no surprise if both attitudes, mastery and alienation, played their part in the complex events that unfolded. The student initiatives sparked strikes which the integrated, unionized and less alienated workers (among the highly educated *cadres* in particular) were ready to lead, with the public majority—relatively unorganized and highly alienated—expressing great sympathy and joining the fray as the early success of the rebellion developed. That it was not all so simple in its reality is attested to by the fact that the union leadership, and many of the "militants" closest to them in the shops, forcefully resisted the engagement of the workers (see, for example, the commentary on the Communist-led C. G. T.'s conservative role by one who left that organization's leadership body; Barjonet, 1968). Yet union members (and leaders), often enough of white-collar status, did play a significant, perhaps essential, role in the unfolding of the crisis—including the workers' rejection of the "Grenelle Agreements" negotiated by union leaders in late May (cf., Touraine, 1968, esp. pp. 163-179; and Reynaud *et al.*, 1971, esp. pp. 81-83).

The dual contribution of both mastery

¹³ On the falling back to De Gaulle in 1968, cf. the remark by Converse and Pierce (1970): "If we were to describe the center of the June reaction at the polls in political terms, we would have to point in particular to the citizens of the *marais*" (p. 25). On the flirtation to the left of De Gaulle in 1965, see Deutsch *et al.* (1966; Chapter 4). For a description of similar political shifting in the local elections of 1959, where the interpretation again emphasizes the sense of control (the shrug "c'est comme ça," vs. the sense of freedom to act in one's own welfare), cf. Wylie (1964; pp. 353 ff.).

and powerlessness would be no surprise because evidence for a similar duality has been advanced concerning black protest in the United States. The evidence suggests that the sense of mastery (i.e., low powerlessness: "personal efficacy" or "fate control" as it is sometimes called; Caplan, 1970) is associated with black militancy (Gore and Rotter, 1963; Forward and Williams, 1970), but that readiness to approve of, or participate in, mass disturbances like Watts or some recent campus action is associated with high powerlessness (Ransford, 1968; Crawford and Naditch, 1970).

The complexity in this portrait of how powerlessness operates should not be surprising. If present evidence is any guide, at least two major distinctions are vital in understanding that complexity. First, there is a distinction concerning the domains of powerlessness, especially the difference between *personal* versus *societal* control. The militants and early activists (who hold an instrumental view of their action) are likely to be low in powerlessness when this refers to their sense of personal efficacy, but high in powerlessness in the sense of being aware of (and fighting against) blocked social control (e.g., discrimination and bureaucratic arrangements). And when the movement is on the upswing, the activists are joined, both actually and spiritually, by those who feel powerless in both senses (who "join the excitement," as Converse and Pierce put it, to make their sheer protest plain).

Second, there is the related distinction between two forms of alienation that are often blurred; namely, between powerlessness and distrust. It is a distinction that has recently been insisted upon by various writers (e.g., Gamson, 1968; Seeman, 1972). If the activists are not "powerless," they are customarily "distrustful" of the political system—which is a way of saying that, at a given point in time and in particular movements, activism is likely to be found among those who are high in their sense of efficacy but low in political trust (cf., Paige, 1971). Obviously, it takes particular circumstances to convert a substantial segment of the population into "participants"—to bring these different social types into the arena of action.

If all this is only a beginning, the answer to the question about May raised at the outset—could anyone really have predicted it?—would seem to be "No." But I think that a better answer would be "Yes and No." The combinations of historical circumstances, group interests, momentary opportunities, and political strategies are inordinately complex for such prediction. Obviously, the present study, which was designed simply as a comparative investigation of the forms and correlates of alienation in two Western countries, can hardly be properly conceived as the ground for such complex prediction. But if the evidence and the interpretation presented here concerning the level, specificity and significance of powerlessness are correct, then we can predict that where there is neither a successful revolution nor successful reform (as seems to be the case in France, and in the university and community upheavals in the U.S.), the disturbances will recur despite surface appearance of calm and reconsideration.

The recurrence, when it happens, will not be the result of vague unrest that we can hardly identify, or diffuse and thoroughgoing alienation which undermines the whole social fabric, or disaffection about alienated labor that necessitates utopian remedies. More likely, the symptoms and the scenes of May will recur because there remains a substantial sense of powerlessness to control one's destiny, at work and elsewhere; and because there is corporate resistance to the major changes in power and social structure that are required by way of remedy.

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THE MANUFACTURE OF DEVIANCE: THE CASE OF THE SOVIET PURGE, 1936-1938

WALTER D. CONNOR

University of Michigan

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This paper examines the Stalinist purge of 1936-1938—as a modern case of a massive deviant-recruitment campaign. It tests, in general, the applicability of tentative generalizations made about the nature and the limits of such campaigns. These generalizations derive from investigations of American, English and Continental witch-crazes, and appear mainly in Erikson's Wayward Puritans (1966). Examination of the purge, its development and outcome, suggests that (1) under certain conditions, the amount of deviance a society can create, recognize, and process is not, as Erikson suggests, likely to remain "fairly constant over time," but can be radically and swiftly increased; (2) the major characteristic enabling elasticity in the amount of deviance recognized is a control system of a special, repressive type; and (3) elasticity is amplified when the deployment pattern used by the society with respect to the deviants is one which, as in the Soviet case, at least appears to minimize the economic costs of isolating large numbers of persons from society.

RECENT sociological and historical studies of forms of religious deviance (notably witchcraft) endemic in many areas in the sixteenth and seventeenth centuries (Erikson, 1966; Trevor-Roper, 1967; Currie, 1968) illustrate instances in which social control agencies largely shaped and sustained the epidemics of deviance they purportedly strove to suppress. The specific deviant acts they deal with—pacts with the devil, bewitching, and others—are frequently bizarre, and most often the inventions of popular beliefs and of the control agencies themselves. While some of the accused probably did believe themselves to be witches, most of this deviance was manufactured.

This study concerns itself with a modern instance of the manufacture of deviance—the Stalinist purge, the Soviet "Great Terror" of 1936-1938. The lessons derived from it may point to a need to rethink some contemporary ideas about deviance. Centuries, and deep historical, cultural and ideological differences separate the purge from the campaigns against witches and heretics in England, Europe and Puritan Massachusetts; yet important similarities argue the value of examining the purge, its mechanics and outcome, within a comparative framework which includes the earlier anti-deviance campaigns. Each of these campaigns recruited masses of persons to

deviant status, the majority of whom were in fact innocent of the acts with which they were charged. And in each, deviance-invention had more or less traumatic effects on the society in which it took place.

The purge has hitherto been of interest mainly to political scientists and historians (Conquest, 1968; Beck and Godin, 1951; Brzezinski, 1956) and to those of its victims who survived to write their memoirs. On the surface, perhaps, it appears too much a political move, too planned and orchestrated, to fit readily into that class of events which may profitably be examined by the sociology of deviance. Yet, as sociological attention increasingly encompasses control as something other than simple reaction to deviance, questions about planning and orchestration in detecting and prosecuting deviance are of growing significance. Deviance, as manifested in defining certain acts as deviant, and identifying persons as deviant by connecting them with deviant acts, comes to be seen as at least partially an effect of control. However the purge has been viewed in the past, as an example of a case in which control produced deviance on an enormous scale it is a topic worthy of sociological investigation.

More directly, studies of witch-crazes and religious controversies have suggested generalizations about processes of large-scale deviance whose fit to the facts of the purge

remains to be tested. As we shall see, the fit is quite good at some points; at others, however, the case of the purge suggests a need for reformulation.

Erikson develops three theoretical themes on the basis of three deviance epidemics in Puritan society—the Antinomian religious controversy, the “invasion” of Massachusetts Bay by the Quakers, and the Salem witchcraft episode. The three themes concern the style or type of deviance characteristic of a community, the volume of deviance a community can recognize, and the way a community handles the problem of the deviants within it—its deployment pattern (1966:19–29).

The first suggests that “every community . . . has its own characteristic styles of deviant behavior,” styles conditioned by the community’s emphasis on particular types of values—political, economic, religious etc. Thus, deviance seems to appear “at exactly those points where it is most feared” (see Erikson, 1966:19–21). The dominant values of the Puritan community were religious: around the orthodoxy they formed, the group’s identity was built. Hence, the deviance that appeared was of a religious variety. This proposition can easily be demonstrated in the Soviet case. The U.S.S.R. has been, and is, a politicized society, attaching political relevance to a whole range of behaviors not so regarded in many other nations, placing a high premium on political orthodoxy, and fearing subversion and disloyalty. As such, it has tended to beget political deviance.

The second of these themes, which concerns the volume of deviance, is of greater interest here, since it is one that the purge calls in question. Erikson suggests that “the amount of deviation a community encounters is apt to remain fairly constant over time” (1966:23). Encountering (perceiving, recognizing) deviance takes place within limits imposed by enforcement capacities. Generally, communities are limited to recognizing the number of deviants that can be handled in jails, hospitals, etc.

When the community tries to assess the size of its deviant population, then, it is usually measuring the capacity of its own social control apparatus and not the inclinations toward deviance found among its members (1966:25).

To some degree, then, constancy over time in the amount of deviance encountered is a function of stability in the capacity of the social control apparatus. Beyond noting that generally “we invoke emergency measures when the volume of deviance threatens to grow beyond some level we have learned to consider “normal” (1966:24), Erikson does not discuss factors that might effect change in the capacity of the social control apparatus.

The constancy proposition receives empirical support from Essex County crime statistics during the “Quaker invasion.” While the rate of recorded offenses climbed sharply, the number of convicted persons rose scarcely at all during this period.

A crime wave lashed across Essex County which almost doubled the number of offenses handled by the local court, yet the size of the deviant population itself did not increase to any appreciable degree (1966:175).

The rise of deviance without a concomitant increase in deviants suggests that during the thirty-year period, other, non-Quaker, non-religious deviants, were displaced from judicial attention and that displacement was a product of Essex County’s relatively “fixed” capacities for handling deviants.

From this capsule summary of a complex argument one point emerges clearly: in the period of the “Quaker invasion,” a major “crime wave,” the number of recognized deviants remained roughly the same as in the period before its onset. Erikson’s constancy notion fits the experience of Essex County: as we shall see, it does not fit a modern instance of a “crime wave” on a larger scale—the Soviet purge of 1936–1938. Those years saw the recognition of massive numbers of new deviants, and there is little evidence that the old were displaced.

Critical to any analysis of the handling of “deviance epidemics” is an examination of control systems, their differences, and the impact of these differences on deviance itself. Currie (1968:28–39) distinguishes two types of control systems—“repressive” and “restrained.” He examines the difference between the nature and scale of witch-control activities in Europe under the Inquisition, and in England, (where the Inquisition did not exist), and attributes the differences in

scope between the English control effort ("witchcraft as racket") and the continental operations ("witchcraft as industry") to differences in the control systems. The Inquisition approximated, in its structure and operations, a repressive control system, whose marks were "invulnerability to restraint from other social institutions," "systematically established extraordinary powers for suppressing deviance and a lack of internal restraints," and "a high degree of structured interest in the apprehension and processing of deviants" (1968:16). The legal restraints of continental trial procedure (weaker in any case, given the inquisitorial nature of that procedure, than those in English accusatorial proceedings) bound the Inquisitors little, if at all. Torture, indefinite imprisonment and other special powers were at their disposal, and motive for recruiting as many, and as prosperous, witches as possible was supplied by the practice of the Inquisition's confiscating the accused witches' property.

Witchcraft in England assumed very different dimensions, given the English control system's closer approximation of a restrained system, distinguished by accountability and restraint, a lack of extraordinary powers, and a low degree of interest in deviant-apprehension. Procedural safeguards were greater, and the confiscation of witches' property never became the rule (Currie, 1968:20). The markedly smaller number of witches detected tended, *contra* continental experience, to be drawn from socially marginal populations, rather than the wealthy and important. If processing witches in Europe assumed the face of industrial bureaucracy, in England it had the look of petty entrepreneurship. The control system of Massachusetts Bay, while open to abuses, was for obvious historical and cultural reasons closer to the restrained English system than to the continental Inquisition (1968:8, n.3), and behaved, within limits, as a restrained system would be predicted to. That the Soviet control system of the purge years approximated the ideal of a repressive system, as we shall argue below, is crucial in explaining why the purge does not fit Erikson's proposition regarding "constancy over time" in the amount of deviance detected.

(Erikson's third theme, concerning "deployment patterns" and their cultural conditioning (1966:27-29, 185-205), is an attempt to explain aspects of the American experience. Some of the issues raised are, however, relevant to the Soviet case, and shall be considered in the concluding sections.)

The remaining sections of this paper will describe and analyze a single case of "epidemic deviance"—the purge—in an attempt to support the following propositions:

(1) that the limits on the amount of deviance a society can "afford to recognize" rather than being constant over time are, in fact, quite elastic under certain circumstances, and allow for rapid increase in the number of deviants recognized;

(2) that the critical circumstance under (1) is the existence of a repressive control system, as exemplified by the Inquisition and the Soviet system in the 1930's (Erikson focuses on a set of cases involving a relatively restrained control system—a focus which precludes examining the behavior of repressive systems.);

(3) that the elasticity of a repressive control system may be (as in the Soviet case) amplified by a deployment pattern which appears to reduce the costs of recruiting and maintaining a deviant population, through setting up camps rather than prisons, removing deviants to geographically remote areas, and trying to maintain their contribution to the economy through forced labor, all of which allow rapid expansion in the intake of deviants.

Finally, two additional themes run through what is to follow: first, the tendency of the deviant-detection activities of repressive control systems to snowball, almost randomly recruiting deviants, and second, the irrelevance, under certain circumstances, of prior community support, or the lack thereof, for the progress of the purge. As we shall see, even its victims, once in the interrogation phase, often found it in their interest, as they saw it, to cooperate in expanding the purge.

The Victims

Many authors have tried to estimate the scope of the purge, to number its victims. Their figures—admittedly loose and tentative approximations—have one thing in common: they reflect a social upheaval

probably unmatched in modern peacetime history.¹

Since this study is a discussion of sociological questions raised by the purge (and since space constraints preclude it), no lengthy description of the victim population is offered here. Extended treatments of it are available in other sources (Beck and Godin, 1951:86-170; Conquest, 1968: 276 ff.). For present purposes, it may suffice to note that the victim population was diverse, reflecting many subgroups in Soviet society, and at the same time, special. Beyond the most visible victims—the high party and state officials who appeared in the dock at the Moscow trials—party and state bureaucrats at the provincial and district levels were hard hit. Rank-and file old Bolsheviks, as well as members of other defunct revolutionary parties, perished in large numbers. Industrial managers, engineers, transport officials and others with responsible posts in large organizations contributed to the victim group in numbers greatly disproportionate to their share of the population, as did the Soviet officer corps, from marshals on down. Citizens with relatives abroad, or other foreign contacts (many if not most of them legal, in trade, tourist, or cultural organizations), as well as foreign Communists who had emigrated to the USSR, were arrested in large numbers. Other target groups included relatives of those arrested, and members of Soviet ethnic minorities residing in cities outside their own areas.

While diverse, this population, heavily composed of better-educated urban groups, was less diverse than Soviet society itself. It included in large numbers members of the new Soviet elite, persons whose careers had been made primarily in the years since the revolution. The purge fell less heavily on those at the bottom of Soviet society—the lower working strata, the collective farm peasantry. All victims were, however, united

in facing situations of presumed and non-rebuttable guilt after their arrest.

Processing and Confession

Where offenses do not lend themselves easily to proof by eyewitness testimony (as in the witch's pact with the devil, the conspiracies of counterrevolutionaries, and any offense which has not actually been committed), confession must play a major role in establishing guilt. Victims of the Soviet purge were in fact required to confess not only their own guilt, but also the guilt of others, to their interrogators. Confessions were critical to the purge, for many reasons.

First, the Stalin regime encountered a large public relations problem in the purge. The Soviet Union was, by the mid-1930's, held to have left behind the Red terror, the "class justice" of its earlier years. Thus, to admit that a prophylactic purge, and not the uncovering of a vast network of political deviants, was taking place would have compromised the Soviet Union too much in the eyes of both enemies and admirers. Confessions at the Moscow trials convinced many foreigners and Soviet citizens that, however implausible, old Bolsheviks were in truth selling out the nation, and that the Soviet Union was indeed infested with active plotters and saboteurs. The accusations were untrue, but given the commitment made to ferreting out deviants, they had to be proven nonetheless. Where evidence cannot be convincing (since it does not exist), confessions may fill the gap.

Second, the success of individual interrogators of the NKVD in extracting confessions indicated their vigilance, and in part guaranteed their own safety. Accusations of insufficient vigilance changed many an interrogator into an accused, while a high "productivity index" was some disproof of leniency. As the purge intensified, the spectacle of interrogators following those they processed to the camps was not uncommon. (see Beck and Godin, 1951:238; Ginzburg, 1967:139, 338-390; Weissberg, 1951:298).

Third, each confessing deviant, in naming his recruiters, and those whom he had recruited to anti-state activity, supplied an important input. The more persons, and the more important the persons, who could thus

¹ Relatively thorough reviews of the available data and discussions of earlier estimates of the number of victims, are presented in two recent sources (Conquest, 1968:525-535; Swianiewicz, 1965:25-40). Conquest, for example, estimates that seven million persons were arrested in 1937-1938. Any discussion of these matters here would require many pages; the interested reader is therefore referred to the sources above.

be detected, the more credible the vigilance and effectiveness of the entire NKVD.

Many accused, reluctant to implicate other innocents, filled their recruit quota with those beyond reach. Experienced prisoners often provided new arrivals with names of the dead or arrested who could be recruited with impunity. But others were convinced that the key to ending the purge, and to their own release, lay in reducing the process to complete absurdity. These sought to drive the purge to its limits by implicating all those in responsible positions still at large. Military officers implicated their whole units, officials denounced everyone in their organizations, academics their colleagues (see Weissberg, 1951:314-317; Beck and Godin, 1951:188, 194-200).

Confessions, of course, were seldom voluntary. They were the product of interrogation, and the speed and fullness of confession depended on the mechanics of the interrogation process. Except in the cases of some special victims such as military and NKVD officers, early in the purge little physical violence seems to have been used on the accused. More often, the first stage of interrogation was to persuade the prisoner that the NKVD already knew everything, that only one's official confession, identification of recruits, and minor details were lacking.² In return for cooperation, light sentences were often promised. Persistence in denying guilt led to threats of heavier sentences or death penalties as well as arrest or exile for relatives still at large.

Those who refused to cooperate, were subjected to the "conveyor"—a lengthy, sleepless interrogation by a succession of well-rested interrogators (see, e.g., Beck and Godin, 1951:53-54; Weissberg, 1951:53-54, 238 ff.; Ginzberg, 1967:83 ff.).

But the conveyor was time-consuming. A speedier, more efficient method of processing was needed. (The amount of time between arrest and formal dispatch to a camp varied. For workers and peasants, whose confessions were expected to be simple, only

a few months generally elapsed. Members of the intelligentsia typically took longer) (see Beck and Godin, 1951:75). The "breakthrough" for permission to use systematic beatings and other torture, came in August, 1937 (Conquest, 1968:307). Thereafter, the purge moved toward its peak.

In essence, then, one became a deviant upon arrest. Practically speaking, one could not prove his innocence.

[A] person whose work had always been above reproach was undoubtedly covering up counterrevolutionary activities. But should there have been any instance of trouble or any accident in the defendant's place of work, that was proof of his sabotaging activities. If the defendant admitted having observed anything suspicious in the private or professional life of a superior who was also under indictment, he was guilty of a lack of revolutionary alertness. If a woman repudiated her husband once he was arrested, she thereby admitted that she thought him a counterrevolutionary and was therefore guilty for having lived with him. If she did not renounce him, she was guilty of questioning the infallibility of the NKVD. (Lipper, 1951:38).

Such guides for behavior interpretation are strikingly similar to those used to detect witches centuries earlier.

If the accused was found to be in good repute among the populace, he or she was clearly a witch, since witches invariably sought to be highly thought of; if in bad repute, then he or she was also clearly a witch, since no one approves of witches. If the accused was especially regular in worship or morals, it was argued that the worst witches made the greatest show of piety. (Currie, 1968:15).

The Vigilance Industry

Explaining the growth and acceleration of the purge involves examining the purge machinery—the Soviet secret police or NKVD—which, in the period that concerns us, amply satisfied Currie's three criteria of repressive control systems.

Invulnerability to restraint. As a practical matter, the NKVD was invulnerable to all external restraints, save those imposed by the top leadership: essentially, Stalin himself.³ Beyond this, the NKVD was un-

² However, one was often not told what one's offense was. This had to be invented by the accused himself. As the prison culture became more aware of what the interrogators sought, experienced prisoners could often help newcomers with inventing their legends. (See Conquest, 1968:304-305).

³ Indeed, in running its course, the purge consumed the two chief purgers—NKVD heads Yagoda and Yezhov.

restrained, as the toll of victims among the top Party, government, economic and military leadership shows. By and large, the leadership of other social institutions found cooperation with the NKVD in rooting out deviants in their midst the only course that offered a modicum of safety. Attempts at restraint were *prima facie* evidence of counterrevolutionary or subversive intent.

Extraordinary powers. The powers granted the NKVD in coping with deviants vastly exceeded the normal Soviet criminal law, substantive and procedural. First, a contamination principle operated, whereby not only the presumptive deviant, but his friends and acquaintances, and *their* friends, might also be arrested on an assumption of, at worst, their complicity and at best, their "lack of vigilance" (see Conquest, 1968:260, 300). Second, physical beating, while probably used in many criminal interrogations, was not officially sanctioned: after August, 1937, its application to "politicals" received official sanction. Third the trial mode of political deviants diverged markedly from that of regular criminals: trials *in absentia*, trials by special three-man boards rather than people's courts, the lack of any provisions for a defense—all these gave the NKVD extraordinary processing powers over those marked for removal.

Structured interests. The Soviet seige mentality of the 1920's and 1930's, coupled with the impact of Stalin's own personality, gave the secret police a large role to play, and conferred a privileged status on its members. As a control institution, the NKVD had interests in maintaining itself and acquiring powers and resources—interest it could promote by detecting (or inventing) growing members of deviants. In this sense, at least, there were positive rewards to be gained.

Yet such a view minimizes the degree to which, in 1937–1938, the NKVD was a mechanism whose motive force Stalin largely supplied. Within this mechanism, individual workers had another sort of interest: sparing themselves from the fate of deviants. Some, of course, pursued promotion, etc., within the NKVD. But it seems doubtful that such rewards offered as much stimulus as enrichment from confiscating

witches' property offered the Renaissance inquisitors (Currie, 1968:21–24).

The NKVD was, simultaneously, both a source and target of the pressures for ever-increasing vigilance, pressures with which early in the purge many officers over-complied, creating the momentum that would characterize it at its peak. Thus, it appears, the terror extended beyond any targets Stalin himself might have seen as "rational." As an organization, the NKVD could not, pull back from its activities. While many interrogators may have doubted the deviants' guilt, "they were caught in their own system. It was impossible for them not to arrest a man who had been denounced as an agent of Hitler" (Conquest, 1968:465). Nor could NKVD administrations at the provincial and district level safely restrict their output of arrests: many, indeed, received orders from Moscow specifying "target figures," indicating that so many nameless thousands of wreckers or saboteurs lived in their jurisdictions who should be identified and apprehended (see Petrov and Petrov, 1956: 73; Medvedev, 1971:284).

Opportunities arose for self-aggrandizement by the unscrupulous, who denounced others out of enmity, ambition, or covetousness. Some sought their superior's job, others more room in a crowded communal apartment. For some, denunciations became a quasi-racket. Two men in Kiev denounced over 169 people. A party member in Odessa denounced 230, and another in Poltava his entire party organization. One industrious denouncer applied for a free vacation, claiming he had spent his strength struggling with the enemy (Conquest, 1968:280).

Added to these amateurs were the "secret collaborators" of the NKVD, or *seksots*. *Seksots* were both voluntary and involuntary. The voluntary were those who enjoyed the work of denunciation, or felt they were serving the cause. The latter delivered denunciations for fear of their own arrest or in hopes of improving the lot of arrested relatives. These soon found themselves entangled in a system interested only in incriminating information. When none existed, self-interest dictated its manufacture: "the *seksot* who failed to produce information was himself automatically suspect"

(Conquest, 1968:281; see also Beck and Godin, 1951:164-166).

The Purge in Review

Many questions about the purge are, and will probably remain unanswered. As Dallin and Breslauer (1970:5) note, both "the purposes and functions of political terror are many." The latter may be, however, somewhat clearer than the former. The purge's results, at least, are observable. As to the purposes of the purge as a species of political terror, the present author agrees with Dallin and Breslauer that "we can do little more than guess." Purpose, however clear it may have been in Stalin's mind initially, cannot well account for the purge's scale, cannot easily explain its reaching a point at which the "NKVD . . . had files proving that almost every leading official everywhere was a spy" (Conquest, 1968: 316). As the purge developed from an operationally rational desire by Stalin to eliminate all potential sources of opposition and potential support for that opposition, it reached a point where a "spillover of victimization" occurred (see Dallin and Breslauer, 1970:27-28). Arrests by quota, even when ordered from Moscow, probably partly resulted from NKVD over-compliance, rather than clear political purpose. But arrests by quota, and other elements contributing to the spillover, did reflect the repressive character of the NKVD—a system whose very characteristics made for constant growth in the volume of deviance it encountered. Eventually, the purge reached its limits, but they were the limits characteristic of a repressive, rather than a restrained control system.

The first substantial question an interrogator asked was "Who are your accomplices?" So from each arrest several other arrests more or less automatically followed. But if this had gone on for a few more months and each new victim named only two or three accomplices, the next wave would have struck at 10 to 15 percent of the population, and soon after that at 30 to 45 percent . . . we can see that the extreme limits had been reached. To have gone on would have been impossible economically, politically—and even physically, in that interrogators, prisons and camps, already gro-

tesquely overloaded, could not have managed it. (See Conquest, 1968:316-317).

Though it had reached its limits, the purge did not "collapse under its own weight." It came to an end by a decision which reflected the realities: not a formal decree, but a decision communicated in cues too obvious to be ignored. In late 1938, selective prosecutions of regional NKVD heads (See Conquest, 1968:466-467) for "abuse of authority," "unjustified arrest" and similar charges, signalled the change, and were further confirmed by the dismissal of the national NKVD head, Yezhov, in December, 1938. He disappeared in February, 1939. The purge had run its course.

The Elasticity of Limits

The purge, as we have seen, was marked by rapid growth, rather than stability, in the number of deviants detected. That such rapid growth occurred, a product of the repetitive cycle of arrests, confessions and denunciations, does not refute the claim that such processes have limits, nor contravene the observation that societies can afford to recognize only so much deviance: rather, the purge illustrates how far such processes may go, how many can be removed from society before the costs are recognized, the limits reached. The rapid growth indicates an elasticity, rather than constancy, in the amount of deviance that can be afforded over the short term at least.⁴

To return to the issues raised earlier: what factors present in the purge could account for the elasticity of its limits? First, the very nature of repressive control systems involves a strain toward detecting and processing deviants in increasing numbers,

⁴ In support of his constancy argument, Erikson reviews data from the "Quaker invasion" (1966: 107-136, 163-181). The Salem witchcraft episode, however, would seem to suggest a certain elasticity (though only suggest, since comparable statistics for the short time-span involved are not available) even in a relatively restrained control system. The Witchcraft hysteria spread, multiplying accusations and filling the jails (*Ibid.*: 145-153) until, when it could no longer be afforded, its growth was checked. Whether, during the period, common criminals who might otherwise have inhabited the jails were displaced from judicial attention, one can only speculate.

including a broadening of the definition of deviance, and the recruitment of even the innocent to the status of "deviants." The concern of the NKVD rank and file for their safety guaranteed their vigilance. At higher levels, the NKVD leadership could well understand that its place in the system depended on continuing combat against the opposition: the more dangerous the opposition, the more secure the place of the secret police, and hence the strain toward exaggerating (and inventing) that opposition (see Brzezinski, 1956:20-23). In its demand for more and more evidence of political deviation the NKVD was open to exploitation by those who sought job advancement, more living space or other goods through denunciations. The NKVD's mode of operation was supported by the top and bottom of Soviet society: Stalin's suspicion, which gave the initial push, was borne out by the deviance the NKVD uncovered, and encouraged an intensified hunt, while at the bottom *seksois* and racketeering denouncers responded to the atmosphere, supplying more denunciations. Finally, those already arrested, who sensed that the purge could be pushed to its limits and hopefully reversed thereby, contributed still more denunciations.

Of interest is the rapid growth of the purge in the absence of any substantial prior community support for an operation of the scope it was to assume. While the mobilization of previously-weak anti-deviance sentiments among the broader public by interested parties may be observed in many types of societies, and millions of Soviet citizens may have come to believe that the victims were, indeed, "enemies of the people" (Medvedev, 1971:365-367), the purge case seems extreme in this regard. Here we may be dealing with a corollary of those modern social systems in which repressive control can flourish: a radical asymmetry of power, and, by implication, an extremely narrow monopoly on decisions about what constitutes deviance, its proofs, and how society will proceed against it. The idea that societies do not define deviance, but that groups or persons with specific interests and the requisite resources do construct such definitions and make them stick, is not new (Becker, 1963:15,

17 and ff.; Merton, 1966:784-785). Characteristic of the Soviet Union was a radical monopoly of such powers—their concentration in the hands of Stalin and a handful close to him. His own personality, rather than the collective interests and apprehensions of the Soviet upper elite (many of whom were purge victims), shaped the purge to a large degree. In such situations, over the short run at least, community sentiment counts little, and leadership decisions more.

Continental witch persecutions likewise seem to have halted only when monarchs or officials who were well-insulated themselves from charges of witchcraft applied restrictions on torture and confiscation of property (Currie, 1968:22, 24). As Dallin and Breslauer (1970:83) argue, "terror as an instrument of purposive policy does not erode or wither 'on its own' . . . The decline in its use requires active or passive, explicit or tacit, decision-making."⁵ One might add that, as the purge demonstrates, even when the terror launched by a repressive control system *outgrows* purposive policy, its halt still seems to require such decision-making. The purge went beyond any limits community sentiment could have imposed. As Brzezinski (1956:31), observes, the ability to recruit deviants on the scale of the purge already demonstrates a radical monopoly of power. "The purge is an expression of the regime's power, not an effort to achieve it."

Much like the Inquisition, which shared its characteristics of repressive control, the NKVD created its own opportunities, invented the deviance it was to repress. In a relatively short period, it "recognized" millions of previously-hidden political deviants. The total number of deviants increased, with no evident displacement of non-political deviants.⁶ The degree of elas-

⁵ The Puritan control system, in its most extreme crisis—the Salem witch hysteria—wound down due to the incredulosity of the leading men which emerged rapidly as community leaders themselves came to be numbered among the accused. (Erikson, 1966:149-153). While a decision thus curtailed the terror here as well, it is interesting to note that, in the purge, leading men were among the foremost victims.

⁶ In fact, some persons apparently sought definition as non-political deviants (through commission

ticity in the limits, then, seems strongly related to the nature of the control system: restrained and repressive systems behave differently, with different results. The distinction is critical, and one cannot generalize to the behavior of all control systems by investigating the behavior of one type.

A final factor—the Soviet deployment pattern⁷—seems critical in two ways: first, it amplified the recruitment and containment capabilities of an already repressive control system, and second, it represents an alternative to the model of a relatively fixed-capacity containment system, on which the notion of limits on the recognizable quantity of deviance seems to depend.

The Soviet deployment pattern was one in which some of the problems of expense in controlling and containing deviant populations—recruiting personnel, constructing detention facilities, losing the labor capacities of the confined—all seemed, at least, less pressing than in other systems.

The secret police were, despite (or perhaps because of) the nature of their work, a favored group in Soviet society. The privileges and high salary of an NKVD officer were, for some, enough incentive to minimize recruitment problems. Even before the purge, the NKVD was a large, quasi-military body, with thousands of troops—a pool from which convoy detachments, guards, etc. could be drawn (see Medvedev, 1971: 392).

Perhaps more important was the question of “brick and mortar”; for while these are expensive, little of either was used for long-term containment of purge victims. The prisons of the large cities were grotesquely crowded in 1938, cells holding ten times the number for which they had been intended. To construct new stone or brick prisons to hold the millions arrested would, indeed, have been a task awesome enough to create new limits. Such limits were not inherent

in the course actually taken: recommissioning disused buildings—churches, monasteries, hotels, etc.—as prisons, and, more important, containing prisoners in camps—wood buildings, barbed wire “walls.” Such a solution was less capital-intensive by far than the alternatives. When, as was often the case, a train containing thousands of prisoners ended its eastward journey in a desolate area of Siberia or Central Asia, and the prisoners, under armed guard, constructed most of the camp facilities themselves, considerable short-term savings resulted. It was in such camps that the vast majority of prisoners were held: the simplicity of construction and materials, the ready labor supply guaranteed, for a considerable time, that the containment capacity of the Soviet system was not fixed, but indefinitely expandable. Sending the prisoners to remote, relatively unpopulated areas enabled the regime to avoid the problems that might arise from containing massive numbers of convicts in or near population centers.

Another critical element of the Soviet deployment pattern (and one which distinguishes it from the other historical examples noted here) was convict involvement in forced labor. This is not the place for a full discussion of the forced labor theory,⁸ but the opportunities inherent in convict labor perhaps contributed to the scope of the purges. The costs of maintaining large numbers in confinement can be reduced by retaining the confined as part of the active labor force. Even in the pre-Soviet period, Russia had a tradition of using convict labor in jobs unattractive to free labor. In the earlier Soviet period, forced labor was used heavily in construction, lumbering, and extractive industries. Such was the fate of purge victims. They were not retired from

of, and jailing for, minor criminal offenses) in order to preempt identification as “politicals” (see, e.g., Beck and Godin, 1951:96–97). Residence in a local jail on a nonpolitical charge was one way of sitting out the peak of the purge.

⁷ This term is used here in a somewhat different sense than that employed by Erikson (1966:27–29). At issue here are the physical, geographical, and economic aspects of deployment.

⁸ Essentially, this theory saw the *raison d'être* for the purge in the need to supply cheap labor to arduous and remote industries. It was popular with the prisoners themselves (Beck and Godin, 1951:225–226), though some raised the obvious objection that recruiting trained bureaucrats, technicians and managers to such unskilled work made no sense (Weissberg, 1951:292–293). The most comprehensive and sophisticated treatment of the theory (and also of pre-purge forced labor policies in the USSR) is by an economist who himself was a forced laborer. See Swianiewicz, 1965.

economic production, nor was their labor "make-work."

This does not mean that it was economically rational. Removing millions of citizens, many highly skilled and educated, from their normal work clearly bespeaks irrationality. But using their labor power may, given deviant recruitment, have been seen as more economical than confining them in prisons (see Medvedev, 1971:394). In any case, to many the whole enterprise probably appeared rational. In retrospect the purges make little economic sense. The critical break-through in industrialization had been reached by 1935; the extractive industries in remote sections of the country could probably have been maintained by positive incentives; the arrest and disappearance of skilled technicians and managers harmed the economy. Whether all this was understood in the 1930's, we cannot tell. But to some, at least, forced labor in unattractive industries in remote areas seemed a rational way to deploy the millions of deviants arrested in 1936-1938.

Thus, it seems that the idea that all societies experience a relative stability in the number of deviants they encounter, a stability rooted in relatively stable containment and control capacities, needs modifying. As with all single cases, the Soviet experience is, presumably, not to be duplicated completely. But some of its elements may be (or may have been), and thereby deserve consideration when one examines the generalizations about social control activities discussed here. The experience of the purge suggests that the limits on deviant-recruitment vary greatly in elasticity with the type of control system involved, and that they can be broad indeed. It also indicates the sort of economic, physical and geographic circumstances under which repressive control systems can recruit rapidly increasing numbers of deviants to permanent service at the borders of society, without assessing the effects as too expensive to be borne.

Beyond these issues, the purge is of more general interest to students who view deviance through "conflict" or "functionalist" perspectives. Many purge characteristics support a conflict interpretation. The mode

of securing confessions and denunciations, the quotas, the size of the enterprise, all point to the importance of the radical disparity in power between rulers and ruled (the latter including most of the elite itself) in the Soviet case. It is hard to argue the validity of a functionalist interpretation, in a strict sense: that the purge showed the Soviet social order generating the deviance it needed to define its boundaries. Neither Soviet society, nor the Soviet elite, needed the purge. The deviance was "generated," but for the most part in a very special sense: it was invented. The victims' crimes were not acts engaged in, *then* defined as deviance: they were acts that had not taken place. The purge, in its general traumatic impact on Soviet life (especially among the military) was, in fact, more evidently dysfunctional in light of the next large challenge the Soviet system would face—the German invasion of 1941. Indeed, as the record of wartime collaboration between some nationalities and the Nazis demonstrates, it had not managed, despite its massive intake, to remove all potential "traitors" from circulation.

Yet, viewed from another angle, the functionalist perspective may be useful. It is arguable, at least, that the purge in fact resulted in a redefinition of Soviet society's behavioral boundaries (though not a necessary one), by making clear to those who remained the degree of political dissent that would be regarded as tolerable (nil), and the degree of personal security one might expect (relatively little) even if one were performing a responsible job to the best of one's abilities. The purge defined boundaries which held to the end of the Stalin era. In retrospect, they seem unnecessary, indeed inimical, to societal integration and the adaptive capabilities of Soviet society; but they were real, and characteristically Soviet. In light of Stalin's radical monopoly of power, one wonders if he, and those close to him did not, at least early in the purge, have some notion that boundary-defining, as well as preemptively crushing potential opposition, was what they were about. Given the (probably permanent) poverty of the historical record here, all one can do is wonder.

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EXCHANGE AS SYMBOLIC INTERACTION: CONVERGENCES BETWEEN TWO THEORETICAL PERSPECTIVES *

PETER SINGELMANN

University of Missouri, Kansas City

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This paper explores convergences between symbolic interactionism and exchange theory in four major areas: (1) both theoretical orientations assume the operation of constructive mental processes when actors act toward their environment; this assumption is explicitly stated by symbolic interactionists and implied in exchange-theoretical propositions dealing with valuation, decision-making or justice; (2) exchange theory implies processes akin to G. H. Mead's "self" and "generalized other" in the sense that interaction in exchange requires persons to imaginatively assume the roles of others and view themselves in terms of the conceptions of others; (3) in both perspectives social organization is viewed as emerging from constructed individual acts "fitted" to one another; such "elementary" interactions give rise to institutional modes of behavior which, once established, exist as a reality sui generis over and against the individual actors; (4) in both perspectives social dynamics is conceived in dialectic terms, arising out of contradictions between micro- and macro processes and inherent tendencies in social organization toward inconsistency, conflict and change. It is proposed that a possible synthesis between exchange theory and symbolic interactionism can begin by postulating a dialectical process in which objective realities become subjectified by actors and subjective meanings become objectified in social institutions. A synthesized theory based on such general postulates can be empirically tested when (a) the concrete "subjective" and "objective" contingencies which make acts meaningful for the actors are posited and empirically indicated and (b) longitudinal observations show changes in some of these contingencies so that predictions about behavioral changes can be made.

EXCHANGE theory is one of the most stimulating current sociological theories because it provides a general rationale for explaining human interaction while at the same time generating specific propositions for predicting concrete behaviors. While all proponents of exchange theory explicitly reject the assumption that human behavior is guided mainly by the deliberate assessment of available means for attaining given ends, the postulate that human behavior is to a significant degree reward-oriented has been useful in generating a large body of testable generalizations.

On the other hand, exchange theory has shed little light on why many behavior patterns in human groups appear manifestly "unrewarding" to an observer. As Abrahamsen (1970:283-284) has pointed out in a recent critique of Homans' exchange theory, understanding and predicting human behavior requires considering the experiences and interpretations of the actors. Especially

Homans' behaviorist approach does not consider that human behavior is subjectively meaningful, and that there are multiple dimensions along which behaviors can be meaningful for the actors. These dimensions have, in Pareto's terms, "logical" as well as "non-logical" aspects, but they all provide the framework in which behavior is motivated.

A theoretical perspective which foremost postulates the subjective meaningfulness of human action is symbolic interactionism. This paper will explore the convergences and possibilities for a synthesis between the basic assumptions of that perspective and exchange theory.

SYMBOLIC INTERACTIONISM AND EXCHANGE THEORY: AN OVERVIEW

Neither symbolic interactionism nor exchange theory is represented by a unified body of systematically interrelated propositions, partly because so many authors have added their particular styles to the respective approaches. George Herbert Mead, while not coining the term, may be considered the father of symbolic interactionism.

*I am indebted to Russell Curtis, Jack Gibbs, John Higley, and Joseph Lopreato (University of Texas at Austin) for their helpful comments on earlier versions of this paper.

But such later writers as Blumer or Berger and Luckmann have modified his perspective in significant ways. Exchange theory also appears in different manifestations; the ones to be considered here can be found in the writings of Homans, Blau, and Thibaut and Kelley. For this reason, and also because both approaches have been discussed and summarized repeatedly elsewhere,¹ the following outline will be confined to select aspects of symbolic interactionism and exchange theory.

Symbolic Interactionism. The point of departure for symbolic interaction theory is the dialectic interdependence between the human organism and his natural and social environments. While the environment has its own reality *vis-a-vis* the individual, it is selectively perceived and reacted to by the organism. Human action is thus at the same time a creative response to a subjectively meaningful environment as it is constrained by the objective nature of natural and social phenomena. The human "mind" represents the organism's capacity to respond subjectively to given objective stimuli through conceptualizing, defining, symbolizing, aspiring, valuing and reflecting. It is the mind that makes human actions "rational," "pragmatic," or "meaningful." In this sense, action or behavior is not a mechanical response to external stimuli, but a thing "constructed" creatively and selectively.

Interaction between humans is "symbolic" in the sense that actors respond to the behaviors of others not for some inherent quality in them, but for the significance imputed to them by the actors; a "conversa-

tion of gestures" becomes "meaningful" for the participants when given gestures arouse the same response in the actor as in others. Meanings shared in this way form the basis for human social organization.

In this process of symbolic interaction man develops his self. Parts of his self become "lodged" in significant dimensions of social structure when he identifies with individuals in similar positions and contrasts himself with others in different positions. His mind enables him to "take the role of others" with whom he interacts and thus to view himself as an "object" through their eyes. His "looking-glass" self-idea is thus of social origin and varies with his involvement in different aspects of social structure. But at the same time, the self becomes a motivating force in its own right and dialectically acts back on and changes the social environment from which it derived.

Society consists essentially of overlapping networks of symbolic interaction. Paradoxically, it only exists in the constructed interactions of individuals and can thus be viewed as a corollary of these interactions (cf. Blumer, 1969:18, 75; Shibutani, 1961:175); but at the same time, society is historically prior to the individual member:² the individual is born into an already existing society which sets the institutional parameters for his self-development. Self and society thus develop together and because of each other in a dialectical process of mutual transformation.³

Exchange Theory. While symbolic interactionism is essentially a grand-theoretical attempt to clarify the nature of the interrelationships between individual and society, exchange theory has been phrased in terms of variables and provides a more specific plan to investigate concretely why given actors act as they do in given social situations.

Exchange theory assumes that men have needs and that fulfilling these needs constitutes a reward. Behavior becomes positively

¹ For original formulations of exchange theory, see Homans (1961, 1958), Blau (1964a), and Thibaut and Kelley (1967). Emerson (1962) had developed the power dimension in exchange before the extensive treatment by Blau. For a discussion and critique of Homans' work, see the special issue of *Social Inquiry*, 24 (Spring, 1964), notably the papers by Alexander and Simpson, and Blau. The first comprehensive formulations of the interactionist perspective are by Mead (1932, 1934, 1938). The tradition has been continued and partially modified by authors such as Blumer (1969:11-89), Shibutani (1961), and Berger and Luckmann (1967). For summary presentations and critiques of Mead's social philosophy, see Natanson (1956) and Meltzer (1964). For an overview of the developments in symbolic interactionism after Mead, see Kuhn, (1964b).

² This theme is touched by Mead throughout *Mind, Self, and Society*.

³ This dialectical synthesis is elaborated in Mead's general work, and it has received further systematic analysis in Berger and Luckmann's (1967) brilliant book which integrates symbolic interactionism with Schütz's phenomenology.

reinforced when it is associated with the experience of a reward and negatively reinforced when associated with a punishment. Behavior therefore is directed toward maximizing rewards and minimizing punishment. Actors may or may not be aware of this reinforcement process in any given situation, and their behavior may or may not be directed toward explicitly recognized goals: the crucial assumption is that given actions are more likely to occur when they are associated with past or anticipated future rewards.⁴ Social interaction results from the fact that others often provide a person's rewards. In order to induce Other to reward him Person has to provide rewards to Other. Social interaction thus becomes a complex exchange of mutually rewarding activities in which the reception of a needed benefit is contingent on the delivery of a returned favor. Needs, anticipations, and predictions of others' behaviors thus become important ingredients in a person's decision-making.

Exchange relations are power relations.⁵ If Person is in a position to provide Other with a benefit he needs and cannot obtain elsewhere, he can make Other provide benefits to him as an inducement, i.e., he can modify and influence Other's actions. A balanced exchange relationship is one in which Person needs Other's services as much as Other needs his; in an unbalanced relationship the benefits exchanged are of unequal value, which gives "power" to the one who can provide the superior or more needed benefits. The dynamics of social interaction then consists in the continuous balancing and rebalancing of power which takes the form of reducing needs, acquiring by force, providing inducements, or seeking out alternative sources for rewards.⁶ Thus, exchange has a larger dimension in the social structure that provides the opportunities and constraints within which the bargaining takes place.

⁴ On this point, Blau appears to disagree with the more strictly "behavioristic" approach taken by Homans.

⁵ The following discussion is based mainly on Blau (1964a) and Emerson (1952).

⁶ The problem of unilateral dependence and alternatives to compliance with the power-holder's wishes has been discussed in detail by Blau (1964a:118-125). It has already been anticipated by Albert Chavannes in 1884 (cf. Knox, 1963:344).

CONVERGENCES

Mind

"Mind," according to George Herbert Mead, "as constructive or reflective or problem-solving thinking, is the socially acquired means or mechanism or apparatus whereby the human individual solves the various problems of environmental adjustment. . . ." It is the mind that gives the capacity to "turn back critically . . . upon the organized social structure of society . . . and to reorganize or reconstruct or modify that social structure . . ." (1934:308). The mind reflects the human capacity to *conceive* what the organism *perceives*, define situations, evaluate phenomena, convert gestures into symbols, and exhibit pragmatic and goal-directed behavior.

The mind-related aspects of human interaction have been explicitly recognized in the writings of exchange theorists. Perceptions are seen as more than passive receptions of stimuli. They entail cognitive, expressive, and evaluative dimensions. Interactants are aware of possibilities and alternatives (cf. Homans, 1961:57-60, 102; Blau, 1964a:96, 119-120), and they have aspirations and expectations (e.g., Homans, 1961:58; Blau, 1964a:95-97). A specific form of mind-oriented behavior, abundantly discussed in the exchange literature, is the formation of value judgments about actions and objects.

The process of valuation becomes especially apparent in Homans' concept of "distributive justice" (1961:74-78) or Blau's related concept of "fair exchange" (1964a:154-160; 1964b). Both principles stipulate that in exchange relationships men will expect the rewards for an activity to be proportional to its cost, or that profits be proportional to investments (Homans, 1961:75). The problem, however, is that when qualitatively different goods and services are exchanged, men "differ in their ideas of what legitimately constitutes investment, reward, and cost, and how these things are to be ranked" (Homans, 1961:246). Accordingly, valuation is a subjectively meaningful activity (cf. Homans, 1961:247) which determines what constitutes a "just" reward for given services. It is worth noting here that in exchange theory "value" is conceived not as something that people "have"

or something that is only culturally imposed on them. Rather, valuing is an *activity* that has a distinctively subjective and situation-specific dimension.⁷ Such a conception is quite compatible with the symbolic interactionist notion that "culture" is not a phenomenon external to the individual but exists only insofar as it is continuously reconstructed and interpreted in the actions of men (cf. Blumer, 1969:18, 75; Shibutani, 1961:175).

The subjective qualities of the human mind are also apparent in Thibaut and Kelley's (1967) highly instructive approach to the study of exchange in small groups, which recognized that human choices cannot be accurately predicted from knowledge of the matrix of "objectively available outcomes" (1967:25) alone. Rather, the matrix which becomes behaviorally relevant "describes the *subjective* understandings and anticipations of the possible interactions and outcomes, however inadequately these may represent the actual universe of possibilities" (1967:24, *italics original*). The recognition of the mind as intervening between "stimulus" and "response" is also manifested in Thibaut and Kelley's key concepts of "comparison level" and "comparison level for alternatives" (cf. 1967:25), both of which imply a capacity for subjective understandings, evaluations, feelings, and decision-making that enables man to construct his world actively within the structure of his environment.

Current exchange theory has thus gone beyond the purely "behavioristic" approach of many reinforcement theories by recognizing, more or less explicitly, that the human mind mediates the relationships between stimuli and behavioral responses. A possible synthesis between symbolic interactionism and exchange theory might, therefore, begin with the premise that human actors have perceptions of objects and

events in their environment, but also that these perceptions are ordered by the actors to form a subjectively meaningful context within which intelligent action is possible. Such action is directed toward goals, aspirations, and expected outcomes—notwithstanding the fact that not all goals are explicitly recognized, not all aspirations are satisfied, and not all outcomes are the same as the ones anticipated. In exchange, persons define the perceived actions of others in terms of the rewards and punishments these actions entail for themselves, and "objects" are defined in a similarly subjective manner. Behavior is then directed to elicit responses from others which are anticipated to be rewarding for oneself. The point is that these rewards are not inherent in the response itself, but must be *defined* as such by the reward-recipient before they can motivate his behavior. Thus, it is not sufficient to postulate that a significant part of human interaction consists in mutual barter for the exchange of rewards. It is equally important to recognize that the rewards exchanged have symbolic significance for the interactants (cf. Homans, 1958:599, 606); they are "rewards" only in so far as the interactants assign that meaning to them.

Self

The concept of self is not only central to symbolic interactionism but can also be accommodated within the framework of exchange theory. While for Mead the "self" was primarily a process in which actors reflect on themselves as objects, other authors of the symbolic interactionist tradition have focused their attention on the "self-concept" held by actors.

Psychologists have often pointed out that men have a need for validating their self-concept by others, especially when they hold a positive image of themselves (cf. Thibaut and Kelley, 1967:44). Accordingly, Other can reward Person by expressing positive evaluations of Person consistent with his self-concept. Friendship and interpersonal attraction can thus be viewed as exchanges of mutually self-supporting evaluations and behaviors (cf. Thibaut and Kelley, 1967:43–44; Blau, 1964a:70).

⁷ Homans (1961) implies the subjective dimension in his treatment of distributive justice as practically unattainable because of the subjectivity of human valuation (246–247). The situation-specificity of values is reflected in his treatment of "value-per-unit-activity" (41). Homans tends to disregard the cultural framework within which values are formed, a point on which he differs from Blau (1964a, 1964b).

A similar bridge between exchange and self-theories has been pointed out by Thibaut and Kelley (1967:245-246). When discussing the "internalization" of roles, they suggest one process in which situationally specific selves are socially developed. They argue that a person may choose to play certain roles in a group because he is rewarded for it. Social reinforcements perpetuate his role-conforming behavior to a point where he identifies himself (partially) in terms of his roles in the group. This type of approach appears to be very fruitful for constructing a dynamic theory of social behavior that includes both symbolic interactionist and reinforcement perspectives. Over time, imbalances may develop between the actual reinforcements of a set of acquired behaviors and the self-concept of the actor; this happens when, for example, self-identification becomes an autonomous source of motivation perpetuating behaviors even when the reinforcements originating them have changed. A theoretical focus on this kind of imbalance could greatly stimulate our theories of behavioral dynamics.

The preceding considerations suggest that the self and social reinforcement can be interdependent but autonomous sources of motivation. Accordingly, exchange theory and symbolic interactionism can be mutually complementary in that one theory explains what the other leaves open. In addition, we suggest that an integration of symbolic interaction and exchange theory can fruitfully incorporate the conception of self. It is a central principle of exchange that the behavior of persons is designed to elicit favorable responses from others. Consequently, persons seek to behave in manners they believe to be rewarding to others in the expectation that the others, in turn, will reward persons by their actions. This implies that persons predict the behavior of others, which is only possible by "taking their roles" imaginatively. Thus, a conception of something akin to Mead's "generalized other" can be incorporated in the most general assumptions of exchange theory. Homans himself was well aware that expectations derived from past experience with *specific* others tend to become *generalized*

and associated with any other person in similar circumstances:

When one man makes a request of another and is turned down, although the conditions surrounding the other resemble those of men who provided similar rewards in the past, the man that made the request is apt to display some degree of anger. The other is "the sort of man" who "ought" to have granted the request (1961:73).

Furthermore, insofar as exchange behavior necessitates predictions of Others' responses toward Person's self and actions, it requires Person to view himself in terms of the perceptions and interpretations held by Others about his own self and actions. Thus, by looking at themselves as "objects" through the eyes of those Others from whom they wish to receive rewarding responses, persons are able to act intelligently in exchanges. The operation of what Cooley called the "looking-glass self" is, therefore, another basic assumption of a synthesis between symbolic interaction and exchange theory.

The point can be illustrated by the example of reference group theory, according to which self-other comparisons are important determinants of expectations and evaluations of relative rewards. Exchange theorists have cited considerable empirical support for the thesis that persons expect the rewards they receive to be equivalent to the rewards received by people in similar positions and who have provided similar services to others who provide these rewards. Correspondingly, it is no surprise that "reference group theory" has been "claimed" by both exchange theorists (Thibaut and Kelley, 1967:88; Blau, 1964a:151-160; Homans, 1961:73-74, 151-152, 248) and symbolic interactionists (cf. Kuhn, 1964a; Shibutani, 1955).

Society

In their analyses of social structure, both symbolic interactionists and exchange theorists take the observation of microsocial processes as their strategic point of departure. The central unit of analysis for both classes of theorists is the social "act" or "activity," although authors differ in their respective conceptualizations. While in symbolic interaction theory, acts are always con-

ceived as constructive and creative processes, exchange theorists present a less unified perspective. Whereas Blau and Thibaut and Kelley in many places imply the constructiveness of human acts, Homans' "activities" simply refer to overt behavior as a unit of observation.

In both theories human *interaction* is conceived as the mutual stimulation of responses by a set of actors (cf. Mead, 1934: 144-145; Thibaut and Kelley, 1967:10; Homans, 1961:35). In their approach to interaction, exchange theory and symbolic interactionism can be linked in two ways. In the first, interaction consists of formative processes of "fitting developing lines of conduct to one another" (Blumer, 1969:66), or "stabilizing" behaviors at levels that are perceived as most "profitable" under current conditions (Homans, 1958:601). In this sense, interaction is continuously constructed and reconstructed by actors who "test" the adequacy of their actions in relation to the responses of others. Even relatively recurrent patterns of interaction are "stable" only until "further notice."⁸ Second, exchange may be conceptualized as symbolic interaction. We have already suggested that in exchange persons may define and interpret the activities of others in terms of their usefulness in meeting their own needs; that is, subjective meanings are assigned to the activities of others. Persons may view their own actions in terms of the putative interpretations and definitions of others. Thus, exchange goes beyond a "conversation of gestures" to symbolic communication in which the meanings of objects and activities are shared by the interactors (cf. Mead, 1934:144-152).

Continued interaction tends to give rise to relatively stable social positions. Thibaut and Kelley's previously cited conception of role routinization through identification deals with such processes on the small group

level. In the larger societal context, institutionalization represents the stabilizing of established behavior patterns through social control. One very important convergence of symbolic interactionists and exchange theorists is that they tend to view institutions dialectically as arising out of the actions of individuals which, once established, exist as objective realities in their own right over and against the individuals.

For Mead, institutionalization arises out of the individual's capacity to assume the roles of a "generalized other" and respond to particular situations in common with other members of a group or community. An institution is, thus, a shared behavior pattern in a particular, socially defined situation which is so organized that it enables actors to take the attitudes of others toward their own activities (Mead, 1934:260-262). Such institutions are, on the one hand, conservative mechanisms of social control by perpetuating an existing social order (Mead, 1964:20). On the other hand, Mead recognized that institutions need not necessarily be oppressive obstacles to the expression of individual spontaneity. Rather, he argued, that institutions define socially responsible conduct in a broad sense that leaves ample room for individual flexibility. He suggested the operation of a dialectical process in which the self learns to understand itself by reference to institutionalized definitions; as an individual thus matures, he becomes increasingly able to impress modifications upon social structures through mindful, pragmatic action (Mead, 1934:262-263, esp. fn. on p. 236. Cf. also Blumer, 1969: 17; Berger and Luckmann, 1967).

In exchange theory we find several references to institutions and institutionalization which share the dialectical perspective taken by symbolic interactionists. For Blau, the dialectic property of institutions is reflected in what is required to maintain them. These requirements are first, the internalizing of values and commitments by the actors as a source of legitimation and support; and second, a formalized structure, supported by the dominant powers in the community, which channels the behavior of actors (1964a:273-277). The interaction of subjectively held legitimizing values and objec-

⁸ This process of "fitting together" has been explicitly recognized in both theories. In symbolic interactionism the point has been stated by Blumer (1969:17-19, 66-67), and especially by Berger and Luckmann (1967:55-59). In exchange theory, the formative processes have been described by Thibaut and Kelley (1967:64-79) and Blau (1964a: 69-85).

tive structures leads to the dialectics of institutions, in the process of which

the very cultural values that legitimate existing institutions contain the seeds of their potential destruction, because the idealized expectations these values raise in the minds of men in order to justify the existing social order cannot fully be met by it and thus serve as justifications, if need be, for opposition to it (1964a:280).

This view of individual and society as a dialectic unity appears superficially to contradict the "psychological reductionism" advocated by Homans (e.g., 1961:380-381). But Homans himself remains a psychological reductionist only as long as he analyzes "elementary" behavior. As he expands his approach to the analysis of larger social structures, he clearly recognizes that "institutional" structures form a reality *sui generis* independent of the "non-institutional" elementary behaviors; both forms of behaviors are described by Homans in terms of a dialectic process of mutual transformation (1961:378-398).

Values such as justice or fairness are similarly treated both as individual activities and social norms in exchange theory. Arguing that assessments of fairness are subjective "matters of taste" (1961:247), Homans glossed over his recognition that consensus among actors on the meaning of "justice" "is easier to achieve among people who in similar backgrounds have acquired similar values" (1961:247). Blau (1964a:156; 1964b) more explicitly recognized that exchange is not possible unless socially shared norms stipulate what should be returned for a given benefit received.

Dynamic Aspects

The problem of structural balance. In Mead's social psychology, a fundamental dualism between micro and macro structures is that of the individual organism and his "environment." The environment is not objectively there but subjectively defined in terms of action problems to be solved. What does and does not constitute the "environment" of an individual varies according to the problems at hand and can, strictly speaking, be determined only *ex post facto*, since it is functionally defined as that which is being acted upon (Mead, 1938:361; also

Shibutani, 1961:97-108). On the other hand, the environment becomes changed by human action, thus giving rise to new problems of definition and action and constituting a new "social world" for the individual who, in turn, must now redefine himself as a social object and solve new action problems. Individual and environment thus mutually determine one another, and the very nature of the relationships between the two is a constant source of change in these relationships (Mead, 1934:127-134).

In exchange theory the dualism of micro and macro structures becomes apparent in the above-mentioned contradictions of elementary and institutionalized behavior, and in the recognition that exchanges take place not between isolated individuals but within a larger societal context. This context differentially affects the interactants' bargaining positions, the relative values of the rewards exchanged, and the legitimation of relationships. Homans shows awareness of these problems, e.g., in his treatment of the nature of "givens" or "external system" (1961:205-231) and in his discussion of what may be considered as the conflict between individual needs and organizational demands (1961:397). Foremost among exchange theorists, Blau makes it a point to view macrostructures as a hierarchy of overlapping substructures which are partially integrated and partially in conflict (1964a:283-311). He is quite explicit in recognizing the larger structural supports of power in exchange (1964a:325-326); the point is illustrated in his thesis that the means for social esteem in one group can be obtained in socially disapproved ways in another group whose approval is considered less significant (1964a:105-106).

This suggests that the lack or incompleteness of integration at different levels of social organization is a major source of its dynamics. The point is *not* that there is no integration, rather that integration in historical reality is always short of complete and that conflict and "incoherence" (or: imbalance, dissonance, disintegration) are normal features of social life. On this issue, both symbolic interactionists and exchange theorists have postulated that incoherence

is not only a *practical* reality but an *inherent* quality of social organization.

One source of incoherence inherent in social interaction is discussed in terms of "multiple selves." Mead recognized that multiple personalities are "normal" since our self-concepts vary with the different aspects of social structures in which we are involved. He assumed ideal-typically that a unitary social structure will be reflected in a unitary personality structure (1934:144), but a "dissociation of personality" (1934:144) can take place in which the unitary self is broken into its components reflecting different and potentially conflicting aspects of social structure. While Mead in most of his writings seemed more concerned with the ideal-typical unity of the multiple self, later writers have stressed its dissociative aspects. Kenneth Gergen (1968) has argued persuasively that selves are "situational" and that seeming contradictions in behavior in dissociated circumstances may therefore not be perceived by the actors as incompatible. On the other hand it is possible that different social contexts are not dissociated but make conflicting demands upon actors. Reference groups may not be mutually sustaining, and actors then have the choice of becoming marginal individuals or committing themselves to one group at the exclusion of the others—a point made by both symbolic interactionists and exchange theorists (cf. Shibutani, 1967:166–167; Blau, 1964a:105–106). Indeed, both approaches are compatible with the proposition that *any differentiated pattern of social organization contains the inherent capacity of generating conflicting self-concepts and interests, both between and within individuals*, and that perfect integration of such social structures is, therefore, impossible in reality.

Conflict and Change. Incoherence in social organization is closely associated with the problem of conflict. For Mead, conflict was an inherent quality of social organization in that cooperation and antagonism among individuals are both of social origin (1934:303–305) and contribute to the functioning of social structures (1934:305–306). Most importantly, the multiplicity of a self's social involvements results not only in what we called incoherence, but also in conflict

among individuals (cf. Mead, 1934:307).⁹ Conflicts are resolved through reconstructions of situations and social structures (Mead, 1934:307–308). As has been shown above, such reconstructions are inherently temporary because reconstructions change social worlds for self and others, thus resulting in needs for new reconstructions. The dialectic structure of this argument becomes strikingly similar to the principle applied throughout Blau's exchange theory (1964a:26): "... that balanced social states depend on imbalances in other social states; forces that restore equilibrium in one respect do so by creating disequilibrium in others." In this continuous process or rebalancing conflicts arise, are solved, and give rise to new conflicts (cf. Blau, 1964a:7, 33–59, 220, 280, 314–321).

Change, as incoherence and conflict, is an ubiquitous possibility inherent in social organization. As indicated above, the notion of interaction between individual and social environment as a continuous process of reconstruction made it impossible for Mead to view social institutions as inherently conservative, rigid, and inflexible. Rather, the imbalances in the reciprocal determination of self and environment lead to a constant change in selves and environment (1934:202–204, 307–311). The "problem of society," thus, is not to maintain a given order, but "to incorporate the methods of change in the order of society itself" (1964:21).

Within the framework of exchange theory, change is also conceptualized as a dialectic consequence of imbalances in reward and power structure. According to Blau (1964a:335), such imbalances arise when vested interests, powers, institutionalization, or the internalization of cultural values help to maintain "undeserved" rewards for those in privileged positions. "Traditional institutions, endowed by pro-

⁹ Mead has elaborated the ecological aspects of this problem by suggesting that with increasing population size and societal complexity the capacity of the individual to play everybody else's roles becomes increasingly impaired. The degree of the division of labor is thus directly related to the conflict-proneness of a society. Mead also maintains that the universality of social differentiation makes the "ideal" integrated society a utopia (1934:317–327).

found values with symbolic significance, tend to defy innovation and reform even when changes in social conditions have made them obsolete;" and powerful groups defend their vested interests regardless of the value of their "service" to society.

Homans describes such imbalances in a very similar fashion. Institutional processes can come into conflict with antithetical "elementary" behavior and generate a new institutional synthesis (1961:395). Not surprisingly, both Homans (1961:152) and Blau (1964a:197) qualify the functional theory of stratification in very similar terms: status and prestige are rewards that are only partly deserved, since they may be maintained through institutionalization and power without providing the benefits on the basis of which they had originated. It is such built-in contradictions that generate dissatisfaction, conflict, and strain toward change in social organization (cf. Blau, 1964a:304-305).

TOWARD A THEORETICAL SYNTHESIS

The preceding analysis suggests that symbolic interactionism and exchange theory converge when we treat exchange as symbolic interaction.¹⁰ In exchange, men "produce" themselves through symbolic interpretations of realities and reward-directed, constructive action. Such reward-directedness, or "rationality," does not imply that men necessarily interpret the actions of others "correctly" or that their own actions necessarily bring the desired results or any rewards at all (Singelmann, 1971). But social action is subjectively meaningful and purposive; knowledge of the "objective" bargaining positions of interactants does not enable us to predict their behavior satisfactorily unless we know how they interpret their situation and what value they assign to that which the others have to offer. The

dynamics of social organization rests in the paradox that "realities" have subjectively assigned as well as objectively given significance for human actors. "Objective" realities constrain behavior, but the subjective interpretations of such realities direct actors to change these boundaries. There is a continuous dialectical process in which objective realities become "subjectified" by human actors. "Reality" can thus be conceived only as simultaneously objective *and* subjective (cf. Berger and Luckmann, 1967).

In the light of the preceding discussion, it appears desirable to formulate exchange theory as a process of symbolic interaction. While this endeavor has not been the purpose of this paper, the main elements of such a formulation can be sketched summarily:

1. In exchange, actors construct normative and existential definitions of themselves, others, actions, goals, and assessments of "fairness."
2. These definitions are not only subjectively constructed but to a large extent socially shared and thus constitute a constraint external to the individual actors.
3. In exchange, the hedonistic strivings of actors are limited and qualified by the nature of the subjective and socially shared definitions of the objective world which includes the self and others.
4. In exchange, actors will change their behaviors or definitions when:
 - a. changes in the objective world render existing behaviors and definitions problematic,
 - b. changes in some of their subjective definitions render other definitions or existing objective conditions and behaviors problematic.

These propositions, which make explicit some assumptions of Blau, Thibaut and Kelley indicate the relevance of cognitive consistency theories and may guide the formulation of theories about concrete behaviors and social structures. The advantage of such a formulation lies chiefly in its potential for explaining the dynamics of behaviors which could be viewed by outside

¹⁰ I would like to emphasize that I do not reject the significant contributions which have been made by the more experimentally and behavioristically oriented exchange theorists (e.g., Emerson). But I contend that social behavior outside the laboratory can at least sometimes be better explained by the additional references to mental and symbolic processes. This paper is an attempt to indicate the lines along which such an expansion of our model can be undertaken.

observers as "irrational" and contradictory to the self-interest of the actors. The thus-labelled behaviors can be more fruitfully conceived as subjectively meaningful within the context of the actors' definitions and interpretations. A case in point are theories concerned with the persistence of "traditional" exchange patterns in the face of changing objective conditions. An approach stated solely in terms of objective reinforcements and bargaining positions will fail to account for such persistence of tradition. Conversely, changes in definitions (e.g., of distributive justice) may induce actors to reject traditional social patterns even though there have been no changes in patterns of objectively available reinforcements.

It may be argued that the postulate of meaningfulness is ultimately tautological since it permits an *ex post facto* explanation of any behavior, regardless of what has been predicted. This becomes especially apparent in proposition (4) according to which given imbalances induce actors *either* to change their behaviors *or* relevant definitions. Space limitations allow only a few general comments here. This paper has explored the convergences between the most general premisses of exchange theory and symbolic interactionism. The most general premisses of *any* theory, however, are ultimately tautological. What distinguishes scientific theories is that they spell out the historical contingencies under which concrete predictions derived from the theory can be tested. Such contingencies can be specified in the analysis of historical phenomena and include those of the relevant objective conditions as well as subjective definitions involved in a given interaction pattern. Once these contingencies have been empirically specified, changes in any one or more of them enable the observer to predict adaptive behavioral changes of given actors. Longitudinal studies thus enable us to verify our theories based on the assumption that human behavior is *meaningful* by observing what happens when given behaviors have become *meaningless* over time.

CONCLUSION

The preceding discussions suggest that in the analysis of institutionalized behaviors exchange can be fruitfully conceived as sym-

bolic interaction in which human actions are viewed simultaneously as subjectively meaningful and objective realities in their own right. With this conception, the old question whether the "ultimate" premisses in the explanation of social behavior are "psychological" or "sociological" becomes reduced to the pragmatic question for the individual investigator of what is the most useful strategy for his particular purpose at hand. The perspective outlined above can increase the generality of exchange theory while at the same time solving the problem of tautology pragmatically. Instead of testing the ethnocentric assumptions that behaviors are "rational" or guided by "false consciousness," we test whether the contingent variables which were posited enable us to construct accurate predictions of behavior changes. In this manner the dialectic unification of two seemingly disparate theoretical orientations can give us a better understanding of social behavior than the mere "adding up" of the two approaches taken by themselves.

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ROLE DIFFERENTIATION *

GORDON H. LEWIS
Carnegie-Mellon University

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The concept of role differentiation has been used by sociologists to explain a wide variety of social phenomena. Most who have used the concept have assumed that task and social roles are incompatible. The present paper clarifies the concept of role differentiation and re-evaluates some of the basic evidence. Neither the data originally published by Bales nor data from a number of more recent studies strongly support the proposition that task and social roles are incompatible. The paper also comments on some recently proposed methods for measuring role differentiation.

IN the last twenty years the phenomenon of role differentiation has been used often to explain not only many phenomena in ad hoc informal groups, but aspects of the family (Zelditch, 1955; Grusky, 1957), organizational processes (Etzioni, 1956), and the course of developing nations

(Ness, 1965). Typically the concept of role differentiation has been used in ways which imply that it refers to a reasonably specific phenomenon and that the evidence for that phenomenon is well based. Whether the phenomenon is well delineated, however, is debatable. Even Bales and Slater, the early proponents of the concept, adopted a series of positions about role differentiation, a fact not appreciated by many of their readers. If the concept of role differentiation is to continue to play an important role in sociological theory, it might be good to take another look at the concept and the evidence for it.

* An extended version of papers read at the annual meetings of the Ohio Valley Sociological Society (1971) and the Pacific Sociological Association (1972). I wish to give special thanks to Joseph B. Kadane and James C. Moore, Jr., for their criticisms of an earlier draft of this paper and to Phillip Bonacich, Claire Kaplan and Thomas Weisman for the use of their data.

The purpose of this paper is to clarify the concept of role differentiation. The paper, which begins with a review of the basic propositions and the early evidence, suggests a more precise definition of the term "differentiation" and uses this definition to evaluate new and old evidence. Neither the data originally published by Bales and Slater nor any of several recent studies show substantial evidence for the often encountered proposition that task and social roles are incompatible. The final section of the paper comments on some recently proposed methods of measuring role differentiation.

Background

The papers published by Bales and Slater have taken at least three distinct positions on the relations between task and social roles. The first was that the task leader (the person who contributed the best ideas) and the social-emotional leader (the person who was best-liked)¹ would tend to be one and the same (Bales, 1953:145). Indeed, Bales speculated tentatively that this person would also be the one who initiated the largest proportion of acts, who did the most to guide the group and who was the least disliked. Bales termed such a group "simply organized."

Although Bales did not expect the correlations among these variables to be perfect, he referred to "evidence of some appreciable degree of positive correlation" of these dimensions and adopted the hypothesis of the "simply organized group" as a "working hypothesis" (1953:146). This first hypothesis was short-lived.

Analyzing the data from four five-man groups, Bales displayed in graph form the relationship between activity and each of the other dimensions, see Graph 1 (from Bales, 1953:147, Chart 2).² With regard

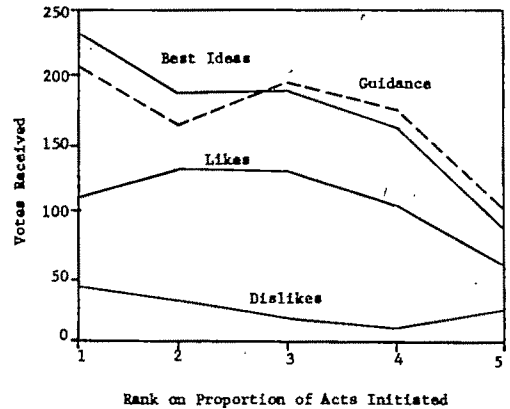
¹ Bales and Slater often used the terms "social-emotional leader" and "best-liked man" interchangeably. Burke (1967) has pointed out that the terms are not equivalent, but at this point in our review we shall follow the practice of Bales and Slater.

² Bales points out that there were twelve separate sessions of these four five-man groups; but since multiple sessions of a single group are not independent, it seems more important to stress the fact that the data come from four groups. The data in

GRAPH 1

"TOTAL NUMBER OF VOTES RECEIVED" ON EACH
OF FOUR ROLES BY RANK ON PARTICIPATION

BALES, 1953



to that graph, Bales commented that "there is a departure from the prediction of the (simply organized) working hypothesis: on both curves (guidance and best ideas) the second man is unaccountably low" (1953:146). Moreover, Bales added, "a more serious departure from the prediction is in terms of the curve on liking. . . . There the top man is unaccountably low, and the second man is highest in the group—by an insignificant margin, but still enough to give birth to the idea . . ." (1953:147, italics supplied). The idea was that "there must be something about high participation and specialization in the technical and executive directions which tends to provoke hostility" (1953:147).

The insights obtained from the information in Graph 1 were elaborated (Bales, 1953; Bales and Slater, 1955; and Slater, 1955) and became the nucleus for a new hypothesis about the relation of task and social roles: that these two roles are incompatible. It is interesting that on the basis of as little data as this, the simply organized hypothesis was replaced by an hypothesis very nearly its converse.

The basic type of data provided for the second hypothesis consisted of the joint occurrence of pairs of roles. These analyses were provided both for each pair of roles—with all sessions combined—and for selected

Graph 1 are the result of aggregating the data from all men of rank 1 from all sessions.

pairs of roles over time. Tables of both types appear in Bales (1953), Bales and Slater (1955), and Slater (1955).³ A typical matrix of role correspondence is shown in Table 1 (from Bales, 1953:153).⁴ Considering the joint occurrence of pairs of roles, Table 1 shows that those pairs which involve the liking role occur less often than pairs which do not.

Additional data provided in support of the second hypothesis are contained in Table 2 (constructed from Table 4 of Slater, 1955:504), which shows changes in the frequency of occurrence of ideas and liking roles from session to session. As can be seen in column three, the number of observed joint occurrences changes from eleven to three to four and then to two.⁵

The third hypothesis advanced by Bales appeared in work published in 1956 and 1958. In those papers Bales suggested that task and social roles are not antithetical, but independent. Whether they are jointly held or not depends on the interaction style of the task leader.⁶ In an attempt to recon-

Table 1. % of Meetings in Which Each Type of Two-Role Combination was Performed by the Same Man, 61 Sessions.

Role	Role			
	RM	BI	G	L
TI	62.3	60.3	44.6	25.4
RM		45.8	35.6	37.7
BI			54.9	30.0
G				27.0

*TI = Top Initiator; RM = Received Most; BI = Best Ideas; G = Guidance; and L = Best Liked.

cile the differences between the second and the third hypotheses, Bales argued that although the roles might appear independent for some population of groups, particular subsets of the groups might display various relations. Some might be characterized by a relationship of association, others by differentiation and still others might be independent. Obviously this argument does not explain why Bales and Slater (1955) apparently found a *consistent* relationship of differentiation, unless it was by chance.

Bales is not alone in failing to reconcile the second and third hypotheses or the studies which bear on them. Most sociologists who have written about role differentiation

³We have followed the practice of citing all three of the early reports of differentiation (Bales, 1953; Bales and Slater, 1955; Slater, 1955) although it appears that they are not independent accounts. Bales and Slater (1955), for example, reported the following distribution of groups size three through seven (4,3,4,4,0); the distribution reported by Slater (1955) for groups size three through seven is (4,4,4,4,4). Apparently the difference between these two vectors constitutes the number of groups from which new data had been collected. While this practice of cumulating the data can serve some useful functions, unless the procedure is carefully pointed out, unwary readers can be misled into assuming that the reports are independent and thus attributing a grossly magnified level of generality to the conclusions.

⁴While we would have preferred to use Table 2 from Slater (1955:502) which contains the largest cumulative amount of data, the analysis in that paper was done separately for "high status consensus" groups and "low status consensus" groups; and it is difficult to reconstruct the combined matrix.

⁵The information in column four of Table 2—expected joint occurrences—will be explained shortly.

⁶The data analysis presented for this hypothesis was not nearly as simple as the hypothesis. In that analysis comparisons were made among the average scores received by the set of persons with a given "feedback ratio" (high, medium or low) who were rank *i* on initiation and the average scores received by the persons with the same feedback level, but a different rank on initiation. Since,

Table 2. Frequency of Occurrence of Ideas and Liking Roles, Slater's Data, 1955.

Session	Number of Groups	Observed Joint Occurrences ^a	Expected Joint Occurrences
1	20	11	4.4
2	20	3	4.4
3	20	4	4.4
4	20	2	4.4

^aThe "observed" frequencies represent the numbers which provide the closest approximation to the percentages reported by Slater. Difficulty in attempting to reconstruct Bales' or Slater's original figures has been noted before (Wheeler, 1957) and is apparently due primarily to ties (Bales and Slater, 1957).

In general, feedback ratios must be negatively related within a given group, Bales' comparisons must often have consisted of differences between high-feedback-ratio *i*-th highest-initiators from one set of groups and high-feedback-ratio *j*-th highest-initiators from another set of groups.

have ignored the implications of the later work. Burke (1967), for example, cites the 1956 and 1958 papers but fails to mention the substantive importance of their findings.

It is interesting to speculate why the third hypothesis has received so little attention. It may be due to the fact that the second position was reprinted early (Hare *et al.*, 1955) and hence was available to more readers than either of the later papers; it may have been due to the apparent number of studies corroborating the findings of the second hypothesis (Bales, 1953; Bales and Slater, 1955; and Slater, 1955) although we have already commented on the non-independence of this sequence of studies. The lack of attention may have been due to Bales' failure to reconcile the differences between the conclusions of 1956 and 1958 and the position apparently supported in his and Slater's 1955 work. Or it may have been due to a "general belief" in the second hypothesis, to which Bales himself referred (1956:148). In the next section, we shall show that the data believed to support the second hypothesis never did provide strong support.

Reanalysis of the Evidence

The basic analyses by Bales and Slater, as well as those by more recent researchers (Burke, 1967; Gustafson and Harrell, 1970; Wilson, 1969) have been based on identifying the individual who is most closely associated with the performance of a given function. Role occupancy has typically been determined by having each group member rank the members of the group, summing each member's ranks on his performance of the specified function, and selecting the person with the smallest sum of ranks (where "rank 1" is the "highest").

Although there are reasonable objections to restricting analysis to the person who ranks first on a given dimension,⁷ a matter to which we shall return shortly, such a method of identifying role occupants does allow one to give a precise meaning to dif-

ferentiation, as well as other potential relationships between a pair of roles.

Adopting the convention that holding a role is equivalent to being the member most closely associated with a role, it seems reasonable to define a pair of roles as (1) *differentiated*, if knowing that a certain member holds one role decreases the probability that he holds the other role, (2) *integrated*, if knowing that a certain member holds one role increases the probability that he also holds the other role, or (3) *independent*, if knowing that a certain member holds one role provides no information about whether he holds the other.

With this definition of role relationships, the number of joint occurrences which would be expected if the two roles were independent can be calculated. In that case, the probability that two roles would be held jointly is simply the product of the probabilities of holding each of the roles. For a group of size m , the probability that a particular member holds two roles if they are independent is $1/m^2$, and the probability that *some* member holds both roles is $m(1/m^2) = 1/m$. The expected number of joint occurrences for a distribution of group sizes is $\sum n_m(1/m)$, where n_m is number of groups of size m .

Returning to Table 2, if one focuses on the decreased incidence of joint occurrence of task and social roles (column 3), he might conclude with Bales and Slater that the roles are antithetical, that holding one precludes holding the other. Column 4, on the other hand, shows the number of joint occurrences which would be expected if the two roles were independent.⁸ In the second, third, and fourth sessions, the number of observed joint occurrences was less than what would be expected if the roles were independent, but the differences are slight. The only *substantial* difference is in the first

⁷ This restriction ignores differences between a group in which the person who ranks first is barely ahead of the person who ranks second and a group in which the person who ranks first is head and shoulders above the person who ranks second.

⁸ One could perform statistical tests of the null hypothesis that the roles are independent, but the resulting information is of dubious value. Statistical tests with their exact null hypotheses tell us more about the inadequacies of our sample sizes than they do about the nature of the phenomena we are interested in. The assumption of role independence is posited here not for its relation to statistical testing, but because it provides a dividing line between role differentiation and role integration.

session, in which the relation is obviously *not* one of differentiation.

Again, in Table 1, for any pair of roles, if the roles were independent, then the expected percentage of joint occurrences would be 22.2%.⁹ None of the percentages in Table 1 is less than that. Some are low, but none is less than the percentage expected by chance.¹⁰

Tables 1 and 2 are but two of many tables presented by Bales and Slater. Examination of the other tables from the basic literature (Bales, 1953; Bales and Slater, 1955; Slater, 1955) provides little if any evidence for the existence of role differentiation as more precisely defined.

Additional Evidence

The above conclusions are not unique to the findings of Bales and Slater. Similar findings have occurred in several other studies. Three of the studies to be cited have been published. The other two are unpublished: Phillip Bonacich gathered data on twenty-four task-oriented discussion groups at Harvard in the summer of 1966, and Claire Kaplan and Tom Weisman gathered data on twenty-two such groups at Yale in the spring of 1968.

The Harvard groups consisted of students previously unacquainted who met together for one afternoon to discuss a series of problems. The problems were to construct crossword puzzles of a particular design. Each group consisted of five males or five females. The groups met for four sessions, the first and third of which were task-oriented. At the end of these sessions, the participants were asked to fill out a questionnaire which asked them to rank all participants on their ability and to respond to seven "social distance" type questions.

⁹ To obtain this expected value one needs the distribution of group sizes. Apparently there were three three-man groups, four four-man, four five-man, and five six-man groups in sessions one and two. Sessions three and four were similar except for having only two three-man groups and one three-man group, respectively (cf. Bales, 1953: 150).

¹⁰ In the data published by Slater (1955:502, Table 2), the expected value was 21.8%, assuming identical distributions by size for high and low status consensus groups. The observed values were 32% and 16.5%, respectively.

Following common procedures, the persons who scored highest on these evaluations were taken to be those associated with the task and social roles.

Table 3 shows the expected and observed joint occurrence of task and social roles. In only one case (males, session three) was the joint occurrence of a pair of roles observed less often than chance, but the difference is slight. Overall, the evidence in Table 3 suggests role integration.

The Yale groups differed slightly from the Harvard groups. Each group discussed four human relations problems, each of which was treated as a separate session. At the end of each session each group member was asked to rank all group members (including himself) on who had the best ideas and (excluding himself) whom he liked best. Table 4 presents the expected and observed frequencies with which "ideas" and "liking" occur jointly. In no instance was the observed occurrence of this pair of roles less than that expected by chance. In general the evidence favors role integration.

Among the published studies, it is somewhat harder to evaluate the extent to which task and social roles occur jointly since the data are often not analyzed that way.

In an article on the effect of the laboratory situation on discussion groups, Wilson (1969) reported that role differentiation was higher in "Not-Observed" groups than in "Observed" groups (1969:220). While he followed the common procedure of obtaining sociometric evaluations of each member, Wilson departed slightly from the "top-man" form of analysis. "The top two ranks on both dimensions were placed in a High-Rank Group and bottom three ranks were placed in a Low-Rank Group in order

Table 3. Frequency of Joint Occurrence of Ability and Liking Roles, by Type of Group and Session, Harvard Data.

Sex	Session		Observed Occurrence	Expected Occurrence
		N		
Males	1	8	5	1.6
	3	8	1	1.6
Females	1	16	5	3.2
	3	16	8	3.2

Table 4. Frequency of Joint Occurrence of Ideas and Liking Roles, by Session, Yale Data.

Session	N	Observed Occurrence	Expected Occurrence
1	22	9	4.4
2	22	15	4.4
3	22	8	4.4
4	22	15	4.4

to determine how often members in each condition tended to be in the High- or Low-Rank Groups of both sociometric dimensions" (1969:229).

Within the limits imposed by grouping the first and second ranked persons together, one can recast the data provided by Wilson and compute a measure of association, such as Kendall's Q. Table 5 shows such an analysis. There is a fairly strong positive association between the dimensions in the Observed condition and only a weak negative association between the dimensions in the Not-Observed condition. An equally appropriate interpretation of these findings would be that there is more role integration in the Observed than the Non-Observed condition.

Another article published in the same year (Burke, 1969) also gives indirect evidence that task and social-emotional roles

occur jointly. Burke's study investigated conditions which produce role differentiation. In terms of his operational definitions of task and social-emotional roles, which differed from Bales and Slater's operational definitions, Burke reports finding more role differentiation in groups in which task participation has "low legitimation" than in groups in which it has "high legitimation." Nevertheless, Burke also reported that "in three-fourths (fifteen of twenty) of the groups, . . . (the task leader) received the *least* amount of hostility" (1969:164, italics added). Assuming that "receiving least hostility" is equivalent to being "best liked"—a dubious assumption in some situations—Burke's study appears to give added evidence of the integration of task and social-emotional roles.¹¹

Finally, evidence bearing on the role differentiation hypothesis was provided in a recent study by Gustafson and Harrell (1970), who reported "less role differentia-

¹¹ Burke departs from Bales and Slater in defining the social-emotional role so that it depends on the perception of what the person has contributed to the solution of social-emotional problems in the group and not just on whether the person is well liked. This modification is reasonable, but since Burke is building on the early role differentiation work and since he apparently accepts the conclusions reached in that work, it seems appropriate to ask of his data, as well, whether they support the original hypotheses.

Table 5. Relation Between High-Best-Ideas and High-Best-Liked in Two Conditions*

"Observed"				"Not-Observed"					
Ideas				Ideas					
		High	Low			High	Low		
Liked	High	16 (9.2)	5 (11.8)	21	High	7 (8.4)	14 (12.6)	21	
	Low	6 (12.8)	23 (16.2)			13 (11.6)	16 (17.4)		
		22	28			20	30		
Q = .85				Q = -.28					

* The marginals in the two tables depart slightly from the constraints described by Wilson (1969:229). Values expected on the basis of independence given these marginals are shown in parentheses within the table.

tion than was found by Bales and Slater" (1970:299, *italics supplied*). In their analysis of the joint occurrence of role pairs, they found *even higher* relative frequencies with which idea and liking roles occurred jointly. In terms of the present paper, what Gustafson and Harrell found was not less role differentiation, but more role integration.

In sum, when roles are defined in terms of the person most closely identified with a role, none of the reviewed evidence indicates that these roles are antithetical, that holding one makes it less likely that one would hold the other. Quite the contrary, several studies showed that the task and social-emotional roles are integrated.¹² But it is also clear that our conclusions are based on a particular procedure for identifying "role occupants" a procedure which has some undesirable characteristics. Before assessing the importance of the present findings, therefore, it seems worthwhile to comment on the use of particular measures of role relationships.

Measures of Role Differentiation

Any review of the role differentiation literature reveals a variety of measures and analyses. Even the articles cited used a number of different procedures. Bales' and Slater's primary analyses were based on the joint occurrence of role pairs, and role occupancy was defined in terms of the single individual most closely associated with a given role; but Bales and Slater also used rank order correlations and comparisons of interaction profiles in establishing and interpreting role differentiation.¹³ Wilson too used joint occurrence of roles but did not confine his analysis to the top man on each dimension. Burke confined his analysis to the top man but introduced a different way

of identifying the top man and a different definition of role differentiation.

In general the procedures to measure role differentiation can be classified according to (1) whether they include only the top person on a given dimension or the positions of each group member and (2) whether the positions on each dimension are based on sociometric evaluations of the group members or on measures constructed from recorded observations of the actual behavior of group members. Dichotomizing these two classifications yields four types of measurement procedures. Bales and Slater's analysis of the joint occurrence of roles as well as Burke's measure of role differentiation are examples of analysis limited to overall evaluations of top men. Typically rank correlations have also been based on overall evaluations, but they use the information about all members of the group. Profile comparisons (Bales and Slater, 1955) have relied on direct measures of performance, but only of the top man on each dimension. A measure proposed by Pollay (1968) relies on direct measures of performance, but uses information about the functions performed by each group member.

Each of these measures has its limitations and uses, and analysis of the joint occurrence of roles is not an exception. One difficulty with analyzing joint role occurrence where role occupancy is limited to the top person on a given dimension is that it ignores the question of whether the "leader" is a "leader" in more than a trivial manner. It is possible that analysis of the existing data by another procedure would yield quite different conclusions. But to argue that would be to miss the point of using the joint occurrence of roles in the present case.

We used it here since it is (unlike some other procedures) both nominally and operationally consistent with Bales and Slater's formulation of role differentiation. Bales and Slater used it to establish the basic phenomenon, in reporting the evidence on which most of the work in the area has been built. We used it here, more precisely, to re-evaluate the evidence.

It would be trite to observe that the choice of an analytic procedure should proceed from the conceptualization of the problem. It is equally true though less often

¹² The evidence is conflicting on whether role integration declines over time. Bales' and Slater's data indicated that role integration does decline, the data from the Yale study say no, and the data from the Harvard study are equivocal. In the study by Gustafson and Harrell, the data from the class of 1967 support the contention of a decline, but the data from the class of 1966 do not (1970:305).

¹³ For a reassessment of the work on differences in interaction profiles, cf. Bonacich and Lewis (1972).

noted, that measurement procedures help establish the conceptualization of phenomena. We will, therefore, comment briefly on two current candidates for measuring "role differentiation."

The first was proposed and used by Burke (1967, 1968, 1969). Though his analysis was confined to the task and social-emotional leaders, his procedure for identifying them departed from the summation of ranks used by Bales and Slater. "The group members rated each other on eleven items measuring task and social-emotional leadership performance. These ratings were then standardized and factor analyzed. The factor scores generated for each member serve as measures of his task leadership performance and his social-emotional leadership performance. From these scores, task and social-emotional leaders (top men) were identified . . ." (1969:163).

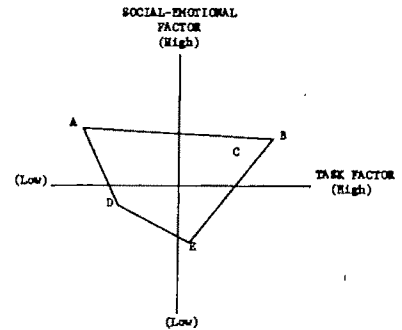
Using task performance and social-emotional performance as orthogonal dimensions in a two-dimensional space, one can locate the task specialist and the social-emotional specialist as points in that space. Indeed, one can locate all of a group's members in that space (see Figure 1).¹⁴ The task and social-emotional specialists are by definition the point(s) furthest to the "east" and "north": points B and A in Figure 1; and role differentiation as defined by Burke is the distance between these two points: the length of the line $\overline{AB}$.

Burke's intent in suggesting this measure of role differentiation was to have a measure such that "At one extreme, if the task leader and the social-emotional leader are the same person, the distance is zero and there is no differentiation. At the other extreme the two points are on opposite sides of the graph and there is a maximum amount of differentiation" (1967:385-386).

One problem with the measure is that like all top man analyses it is very sensitive to the definition of leadership. Perhaps the easiest way to demonstrate this is to repeat the data in Figure 1, but decreasing—ever so slightly—the social-emotional score of

¹⁴In order to sketch more vividly the relations among the members, we have also included in Figure 1 the convex hull of the set of members. As Figure 1 shows, it is not necessary that all members be located on the convex hull.

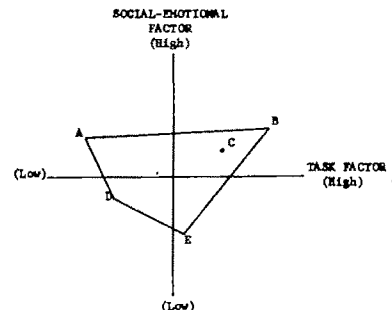
FIGURE 1
REPRESENTATION OF A GROUP IN "TASK"
AND "SOCIAL-EMOTIONAL" SPACE



person A; see Figure 2. In this case there is no differentiation. The degree of role differentiation can change from some "maximum" or "near maximum" to "zero" by miniscule changes in the location of particular actors.

A second limitation of Burke's measure is that it does not at this point lend itself readily to measuring the converse of role differentiation, viz. role integration. To construct such a measure from Burke's procedure would require that one find an expression for maximum role differentiation; but that appears to be a function of the number of questions, the numbers attached to the alternatives, the group size, and the factor weights. Even if an expression for the maximum distance between the actors

FIGURE 2
REPRESENTATION OF ANOTHER GROUP IN "TASK" AND
"SOCIAL-EMOTIONAL" SPACE



could be found, Burke's measure would still be subject to the other liability.

Pollay (1968) recently advanced another procedure for measuring role differentiation. Given a vector T of task performances, $(t_1, t_2, \dots, t_n)$, in which t_i is a measure of the task performance of member i , and a vector S of social-emotional performances, $(s_1, s_2, \dots, s_n)$, in which s_i is a measure of the social-emotional performance of member i , Pollay suggests that role differentiation be defined as

$$P = 1 - \cos \theta$$

where θ is the angle between the vectors S and T . In vector notation

$$\cos \theta = \frac{S \cdot T'}{|S| \cdot |T|}$$

where T' is the transpose of T and $|X|$ is the length of vector X ,

$$(|X| = (x_1^2 + x_2^2 + \dots + x_n^2)^{1/2})$$

This measure has a number of interesting properties. P equals zero when "each individual contributes the same proportion of the total task activity as he contributes to the total social-emotional activity"; and it reaches its maximum of one "if an individual, or subset of individuals, performs the entire task activity and another individual, or a completely unique subset of individuals, performs the entire social-emotional activity" (Pollay 1968:121). In addition, since the measure is based simply on the angle between the two vectors, it is unaffected by scalar changes of either vector. The measure is invariant to the total amount of activity in each area, and depends only on the relation between the proportion of activity that each actor produces in each area. Pollay justifies excluding the amount of time that the group spends in each area on the grounds that this "reflects not the group structure but the nature of the external demands placed on the group as a whole" (1968:122).

One difficulty in using Pollay's measure is that it requires that one have ratio-level measurement of each person's task and social-emotional performance. Pollay suggested using frequency counts from a Bales' interaction process analysis; but that type of data is not easily obtained, and even when painstakingly gathered, is often of dubious reliability (cf. Waxler and Mishler, 1966). On the other hand, there are com-

parably difficult problems in the meaning and reliability of sociometric judgments.

There are several advantages to Pollay's general procedure. First, it can be adapted to measure role integration. If P^* is the measure of role integration, let $P^* = 1 - P = \cos \theta$.

Second, it can be used not only for analyzing the group as a whole but for various partitions of the group. Let person 1 be the person with the highest task measure and person 2 be the one with the highest social-emotional measure. Further, let S_L be the vector containing just s_1 and s_2 , T_L the vector containing t_1 and t_2 ; and let S_0 and T_0 be $S - S_L$ and $T - T_L$, respectively.

Since

$$\begin{aligned} \cos \theta &= \frac{S \cdot T'}{|S| \cdot |T|} \\ &= \frac{s_1 t_1 + s_2 t_2 + \dots + s_n t_n}{|S| \cdot |T|} \\ &= \frac{S_L \cdot T'_L}{|S_L| \cdot |T_L|} + \frac{S_0 \cdot T'_0}{|S_0| \cdot |T_0|} \end{aligned} \quad (1)$$

$$\cos \theta = \frac{|S_L| \cdot |T_L|}{|S| \cdot |T|} \cos \theta_L + \frac{|S_0| \cdot |T_0|}{|S| \cdot |T|} \cos \theta_0 \quad (2)$$

where θ_L and θ_0 are the angles between S_L and T_L and between S_0 and T_0 , respectively. Thus, the relation between the vectors for the group as a whole is a weighted¹⁵ combination of the relationship between the vectors for the top men (in this example) and the relationship between the vectors for the rest of the group. This decomposition of the $\cos \theta$ extends to any partitioning of the group.

Finally, the tack Pollay takes clarifies what one means by the concept of "role." Different roles consist of performance specialization. Pollay recognized, as did Bales, the difference between what a person does and how he is perceived, but it seems preferable to acknowledge that difference by distinguishing between a role and its perception.

¹⁵ The weights are not the simple proportional contribution of the subsets of members, but magnify the proportions of higher participants, since the length of a vector is the square root of the sum of squares of the contributions of individual members.

Burke's and Pollay's work will no doubt help shape the thinking about role relations. But we have digressed somewhat from our major task, re-evaluating the basic data of role differentiation.

Final Comments

Pursuing a more exact meaning for the term role of differentiation and re-examining particular studies was not meant to result simply in calling "lack of role integration" what we used to call "role differentiation." That would be purely a semantic matter. What is at stake is the conception of a phenomenon which has captured the attention of many social scientists. Instead of trying to explain why task and social roles typically cannot be integrated, we need to explain why they often are.

Such a change in perspective should have two effects. First, it prompts searching for explanations of role relations in different areas. The kinds of explanations and theories postulated to explain the absence or presence of role *differentiation* are not likely to be the ones most helpful in explaining the presence or absence of role *integration*. Secondly, in the search for conditions under which roles are integrated and those under which they are not, the process of search, the places searched, the experiments done, and the questions asked are all likely to differ from those in the past.¹⁰ These points are obviously related; good theory and good research always are. Such changes should help us develop a better understanding of role relations and the internal dynamics of groups.

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SIZE AND THE STRUCTURE OF ORGANIZATIONS: A CAUSAL ANALYSIS *

MARSHALL W. MEYER
Cornell University

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Data describing 194 city, county, and state departments of finance in 1966 and 1971 support the following assertions: (1) The effects of size on other organizational parameters—the number of subunits, levels of hierarchy, and supervisors—are ubiquitous. (2) Causality is unidirectional; other parameters of organizations have almost no effect on size. (3) The effects of size are greatest on parameters which managers can most easily manipulate. (4) Apparent relationships among parameters other than size vanish when size is controlled.

THE impact of size on other characteristics of organizations has been widely discussed, but we have little reliable knowledge about its actual effects. Some investigators discern few relationships between size and other variables (see, for example, Hall *et al.*, 1967); some even assert that organizations can be studied without considering size (Zelditch and Hopkins, 1961); whereas others base general theories of organizational processes on apparent effects of size (Blau, 1970; Blau and Schoenherr, 1971). Such disparate interpretations

exist because most data on organizations are inadequate to test causal hypotheses. Data describing large numbers of organizations at one point in time may yield stable correlations among variables, but to draw causal inferences from such correlations is risky for two reasons.

First, causal inferences require assumptions about the time ordering of variables which are necessarily arbitrary when one is considering characteristics of organizations. The most common assumption has been that size precedes other organizational attributes, but this has never been proved. One can easily imagine types of organizations in which size depends on other attributes—for instance, retail stores which build new branches, then increase their staff and volume of business simultaneously. A second reason is that cross-sectional data on organizations are affected by certain boundary conditions which may artificially inflate or otherwise distort correlations. I have discussed this elsewhere in detail (Meyer, 1971). The important point is that given some reasonable assumptions, certain correlations among attributes of organizations are inevitable. For example, ratios of subordinates to supervisors must increase with

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size given a fixed number of levels of supervision; and the number of levels of supervision must also increase with size given fixed spans of control.

Two arguments will be made in this paper, one methodological, one substantive. The former is that longitudinal data are required to assess the impact of size on other organizational variables or relationships among organizational variables when size is controlled. The latter is that attributes of organizational structure are largely a result of size and that they affect neither size nor one another. The reader may judge whether the substantive findings lend credence to the methodological argument; suffice it to say that the two are equally important.

ADVANTAGES OF THE LONGITUDINAL APPROACH TO ORGANIZATIONS

Longitudinal data on organizations do not suffer the limitations of cross-sectional data. Longitudinal data require no assumptions about time ordering of variables as in cross-sectional analysis. Instead the data themselves allow one to determine the direction of influence among a set of variables measured at two or more points in time. And longitudinal data also allow for the possibility of reciprocal causation in which two or more variables influence one another simultaneously. Path-analytic techniques for handling longitudinal data are summarized in Heise (1970) to which the reader is referred. One is primarily interested in the magnitudes of the coefficients of paths linking variables describing organizations (or people, or societies) at different times. Intratemporal coefficients are of less concern. A causal effect of say variable X on Y is indicated if the absolute value of the path coefficient linking X measured at the earlier time, X_1 , with Y at the later time, Y_2 , is substantially larger than the path linking Y_1 with X_2 . Should both paths be of approximately equal magnitude, reciprocal causation or interdependence is present.

In addition to showing the direction of influence, longitudinal data on organizations are not constrained by boundary conditions as are cross-sectional data. The value of any variable describing an organization at an earlier time does not restrict the value of any other variable at a later time. Indi-

cators of organizational structure are thus logically if not empirically independent. This, as we shall see, is an important virtue.

Longitudinal data may require some assumptions about the interval or lag time between changes or disturbances in a causal variable and their effects on dependent variables, but such assumptions are not always required. Ideally, one would like to have intervals between measurements correspond to actual lag times between causes and effects. But this is nearly impossible in practice. Lag times vary enormously between organizations of different types—the hot dog vendor can relocate much more quickly to follow his clientele than the post office can—as well as between units of a given organization. And lag times are no doubt different for different variables in a single organization—one responds to a fire alarm much more quickly than to a request for authorization to hire an additional clerk.

It appears, however, that the interval between measurements need not correspond to actual lag times to yield accurate results, provided that two conditions are met. The first is high short-term stability among values of variables, but not long-term stability which would degrade lag coefficients. My impression of the organizations from which data for this study come is that they meet this condition nicely. Changes occur, but they are incremental rather than dramatic. (This is discussed extensively in Meyer, 1972: Chapter 6.). The second condition is the absence of two-way or reciprocal causation, particularly when a coefficient in one direction is positive and that in the opposite direction negative. Reciprocal causation is remarkably absent from the data to be examined below, but there is no guarantee that relationships among characteristics of organizations are always so straightforward as the data in this article might lead one to believe. (A brief but trenchant discussion of the problem of lag time in longitudinal data is provided by Pelz and Lew, 1970.)

THE PARAMETERS OF ORGANIZATIONAL STRUCTURE

Causal relationships among four parameters of organizational structure will be examined in this article. The parameters are

size, the number of major subunits (called divisions), the number of levels of hierarchy, and the number of supervisors in an organization. The last three are of considerable theoretical interest. The number of subunits and the number of levels of hierarchy are measures of horizontal and vertical differentiation respectively. And the number of supervisory personnel corresponds in at least some accounts (see, for example, Starbuck, 1965) to the size of the administrative or overhead component in organizations.

The data to be reported here are from a study of 194 departments of finance in units of city, county, and state government in the United States. These departments are headed by the chief financial officers of their respective jurisdictions and are responsible for collecting, accounting, disbursing, and auditing local government funds. Some are in charge of budgeting and some operate a data processing facility, but the effects of these activities on finance administration, which are enormous, cannot be considered here. Departments of this type are described in Meyer (1972: Chapter 1); see also Blau and Schoenherr (1971: Chapter 12), and Meyer (1968). Two hundred and fifty-four finance departments were surveyed in 1966, and the same organizations were asked to supply information on their administrative structures in 1971. Some of the 254 departments were lost through absorption into other agencies by 1971. Others have split but could not indicate which of the new units is clearly headed by the chief financial officer. Still others tell us that they are understaffed, overworked, or both, and that the requested data will be forthcoming after the end of the current fiscal year. This initial study is only the beginning of a second wave of data collection. All 254 departments will eventually be included in the research. Data on the incomplete set of 194 departments are reported now because the initial findings are extremely suggestive. The 1966 data show no important differences between the 194 finance agencies we have in hand and the outstanding sixty. Finance departments, it should be noted, are administrative agencies of local governments and thus substructures of larger units. They hardly represent all

organizations or all government agencies. But since every organization that handles money has financial administration of some sort, the findings reported below might be safely generalized to the administrative component of organizations.

Measures of the four parameters of organizational structure to be considered here should be described. Size is simply the number of full-time employees in a department. In the table and figures, size is labeled SIZE66 and SIZE71 for measures in 1966 and 1971 respectively. The number of major subunits is a count of divisions (or, sometimes, bureaus) in a department. Excluded from the count are any units called divisions but containing only the department head and his secretary or administrative assistant. This variable is labelled DIVS66 and DIVS71. The number of levels of hierarchy was established by asking each division head to identify who reports to him, who in turn reports to his immediate subordinates, and so on until the lowest level was reached. The count of levels within each subunit was summed across and divided by the number of divisions. The total number of hierarchical levels was this mean number of graduations within divisions plus one level for the department head. The variable is referred to as LEVS66 and LEVS71. Finally, the number of supervisors consists of those people above the lowest level of hierarchy in each division, plus the department head, all of whom spend some fraction of their time planning, directing, and reviewing the work of others. This variable appears in the table and figures as SUPS66 and SUPS71. The items were worded almost identically in the 1966 and 1971 surveys. Some small differences had to be tolerated because personal interviews were not used in 1971. What evidence we have indicates substantial reliability among our measures. Among the 194 departments, the correlations of the four parameters are quite similar in 1966 and 1971. The one difference between correlations that is statistically significant—the association of the number of divisions and number of levels which was $-.09$ in 1966 and $.09$ in 1971—occurs among coefficients which are not themselves significantly different from zero. The correlations among

the four parameters in 1966 and 1971 appear in Table 1.

SIZE AS THE CAUSAL VARIABLE

The hypothesis to be tested in this article is that size is a cause of other parameters of organizational structure, whereas other parameters neither cause size nor are causally related to one another. We do not intend to exclude environmental and technological variables as possible causes of attributes of organizational structure, but such variables are not considered here. We do intend, however, to show that once size is taken into account, the other parameters show no causal effects; organizational structure is thus a result of size. This has bearing on some ideas that have appeared recently in the literature, particularly the notion that one effect of structural differentiation in organizations is to increase the administrative component. Specific assertions rather than abstract propositions will be put forward here. Their main virtue is that they can be modified or negated which, I hope, is what others will try to do. The assertions are as follows:

1. *The effects of size are ubiquitous.* Figures 1, 2, and 3 show the simple causal relationships of size to the number of subunits, levels, and supervisors in departments of finance. The coefficients of paths between the two measurements of size are extremely high, .86 in Figures 1 and 2, which is the zero-order correlation of SIZE66 with SIZE71, and .96 in Figure 3 which is the standardized regression coefficient net of the number of supervisory personnel. Paths connecting 1966 and 1971 measurements

are much lower among the other parameters: .28 for subunits, .16 for levels of hierarchy, and not significantly different from zero for the number of supervisory personnel. It is no surprise, then, that the cross-coefficients between size in 1966 and the other parameters in 1971 are substantial and increase as the coefficients connecting the different measurements of other parameters decline. The causal effect of size on the number of subunits is .21; its effect on the number of levels of hierarchy is .36; and the path linking the earlier measurement of size with the later measurement of the supervisory staff is .73.¹

2. *Causality is unidirectional; other parameters have almost no effect on size.* Figures 1, 2, and 3 show no substantial paths between the earlier measurements of number of subunits and levels, and the later measurements of size. There is only an uncertain path (-.13) between the number of supervisors and size.² It may well be that increasing numbers of supervisors inhibits growth of organizations. This would be sociologically interesting, but the data are at best equivocal. And the caveat about reciprocal causation, particularly reciprocal causation involving negative loops, must be remembered.

3. *The effects of size are greatest on parameters which managers can most easily*

Table 1. Correlations Among The Four Parameters of Organizational Structure

(1966/1971 correlations)

	DIVS	LEVS	SUPS
SIZE	.57/.51	.43/.47	.84/.85
DIVS	---	-.09/.09	.35/.46
LEVS	---	---	.58/.53

¹ Alas, the question of collinearity arises, to which there is no sure answer. But we can make some suggestions. Let us take as given the .73 zero-order correlation between SIZE66 and SUPS71. Assume also that net of SUPS66, the path coefficient connecting SIZE66 with SUPS71 is .67 — which it would be if the path between SUPS66 and SUPS71 were significant. In order to obtain a significant path (.14) between SUPS66 and SUPS71, the correlation of SIZE66 with SUPS66 would have to fall to .42 to satisfy the equation $.73 = .67 + .14r$ where r is this correlation. And even if the true coefficient of the path between SUPS66 and SUPS71 were as high as .60, a path would still exist between SIZE66 and SUPS71 regardless of the correlation of SIZE66 with SUPS66. Hence, aside from effects of measurement error which we think was quite low for these variables, collinearity does not seem to account for the causal impact of size on the number of supervisors.

² Logarithmic transformation reduces somewhat the autocorrelation of size (to .77) leaving more unexplained variance in SIZE71, increases collinearity between SIZE66 and other independent variables, and does not affect substantive results. The researcher may pick his poison.

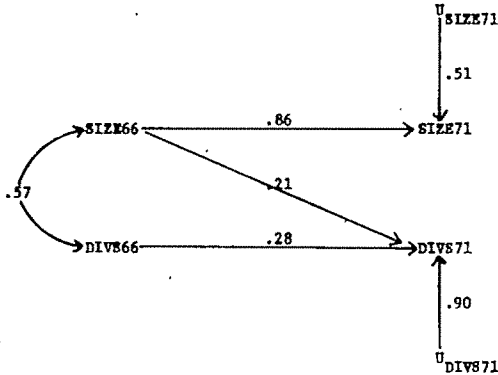


FIGURE 1. SIZE AND NUMBER OF SUBUNITS

manipulate. Here we are generalizing somewhat beyond what the data themselves permit, but the generalization adds validity to the specific findings. Among finance departments, the names and responsibilities of at least some subunits, hence their number, are often mandated by statute.³ Even if this is not the case, to remove a division is to take away someone's personal bailiwick and another person's arena of political influence. To add a division is to create a competing bailiwick or to dilute one's arena of influence. Major structural changes of this sort are thus excruciating and relatively uncommon. This is reflected in the very modest (.21) effect of size on the number of subunits among finance departments. By

³ Of the 254 departments surveyed in 1966, twenty-two had all divisions established by statute, and some but not all divisions were mandated by statute in thirty-eight more. In another sixty-three, the duties of the department head and responsibilities of the department, but not its administrative structure, were fixed by law.

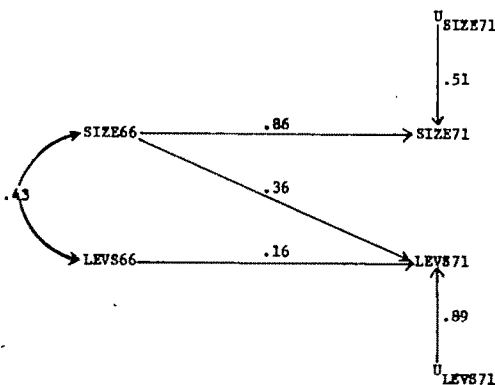


FIGURE 2. SIZE AND NUMBER OF LEVELS OF HIERARCHY

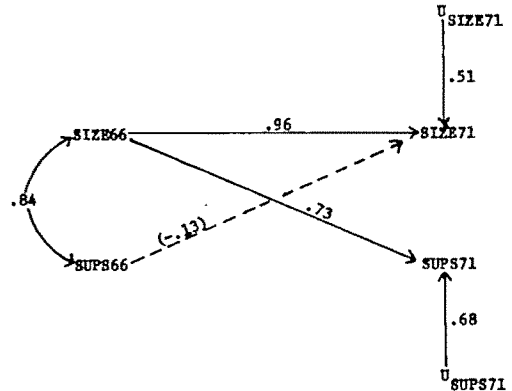


FIGURE 3. SIZE AND NUMBER OF SUPERVISORS

contrast, the number of supervisory positions can be changed, within limits, almost at will. One need only let positions lie vacant or hire additional supervisory help if needed. Thus, the impact of size on the number of supervisors (.73) is quite substantial. The addition or diminution of ranks is of intermediate difficulty. It is surely less easy, for example, to eliminate or reinstitute the rank of Instructor than to eliminate or add a particular Instructor. On the other hand, a proposal to disband the Department of Chemistry would surely cause everyone more grief than the question of Instructorships. Hence, the effect of size on the number of ranks in hierarchy (.36) falls between its effects on the number of subunits and its impact on supervisory positions.

4. *Apparent causal relationships among parameters other than size vanish when size is controlled.* The reader is referred to the path diagrams in Figures 4, 5, and 6. Without controlling for size, the data suggest modest causal links between the number of subunits, the proliferation of levels of hierarchy, and the number of supervisory positions in organizations.⁴ Figures 4, 5, and 6 quickly diabolize one of this notion, however. In Figure 4 which includes size, the only significant cross-paths are the two connecting size with the measures of horizontal and vertical differentiation. It should be noted that Figure 4 (like Figures 5 and 6) includes paths not significantly different from zero. Coefficients of the significant paths have

⁴ Path diagrams from which size is omitted will be sent to the reader on request.

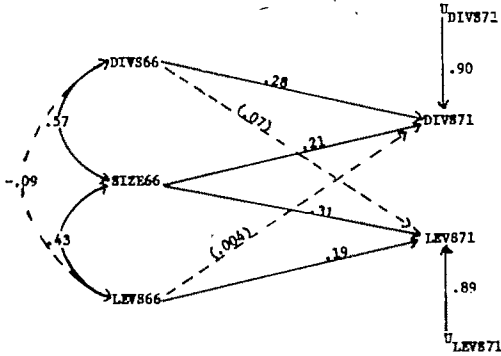


FIGURE 4. NUMBER OF SUBUNITS AND NUMBER OF LEVELS OF HIERARCHY CONTROLLING FOR SIZE

not been re calculated with the non-significant paths omitted from the regression equations. The outcome of such recalculations is obvious. The path model in Figure 4 would be precisely the models in Figures 1 and 2 "back-to-back" save for the absence of SIZE71 as a dependent variable. The same pattern recurs in Figures 5 and 6. These models, in which size is also controlled, would be almost the same as Figures 1 and 3 together and Figures 2 and 3 together if we recalculated coefficients after eliminating the non-significant paths. In Figure 5, neither the number of subunits nor the number of supervisory personnel have any causal effect when size is controlled. (The causal impact of size on the number of subunits vanishes too; what was a weak but significant correlation has been decomposed into two paths neither of which differ much from zero.) Finally Figure 6 again shows that the only causal connections are between size and the other parameters—the number of levels and the number of supervisors—just as in Figures 2 and 3.

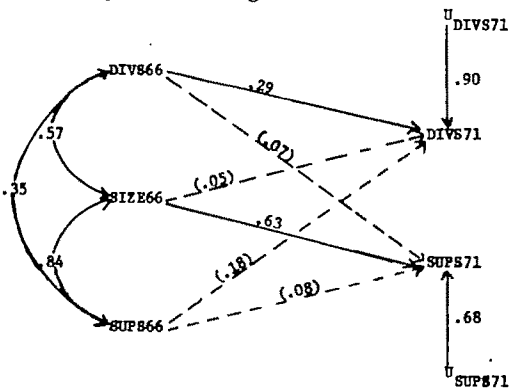


FIGURE 5. NUMBER OF SUBUNITS AND NUMBER OF SUPERVISORS CONTROLLING FOR SIZE

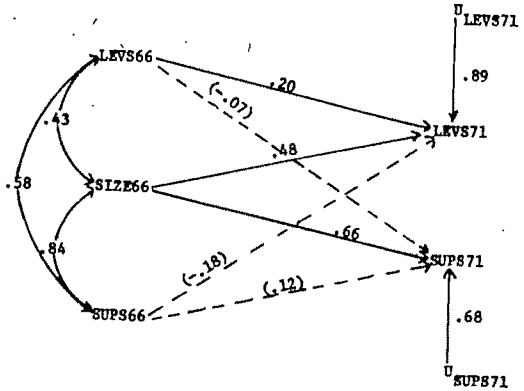


FIGURE 6. NUMBER OF LEVELS OF HIERARCHY AND NUMBER OF SUPERVISORS, CONTROLLING FOR SIZE

IS SIZE THE ONLY PARAMETER?

If one had only the correlations in Table 1, his conclusions would probably be very different and certainly more equivocal than those presented in this paper. Associations between size and other parameters would be observed. And causal connections might be imputed if not proved. Cross-sectional data show roughly the same direct effects of size as longitudinal data—if one could ascertain that they were indeed effects. When one examines other associations, however, inconsistencies appear. Cross-sectional data show that controlling for size, a negative association exists between the number of subunits and the number of levels of hierarchy in an organization (Meyer, 1968: 219). The longitudinal analysis here shows no such relationship, clearly indicating that the earlier finding was a statistical artifact caused by constraints inherent in cross-sectional data. Cross-sectional analysis also showed positive relationships between both horizontal and vertical differentiation and the relative size of the administrative component in organizations when size was controlled (Blau, 1970:213-216). Again, no such relationships appear when longitudinal data are used. This is especially remarkable if we remember that our measure of administrative overhead was the number, not proportion, of supervisory employees. Again, the earlier findings reflect a statistical artifact due to boundary conditions that pervade cross-sectional data on organizations.

Several interpretations of the present findings are possible. One is that parameters of organizational structure other than size

are unimportant and reflect only considerations of convenience, for example, the need to keep spans of control within reason and lines of communication short. Kaufman and Seidman's (1970) research seemingly supports this view. They found no relationship between structural features and the activities of a set of government bureaus. A second interpretation is that not-for-profit bureaucracies notoriously resist change and alter administrative arrangements only in response to the exigencies of growth or declining budgets. Other types of organizations might, according to this interpretation, exhibit causal relationships among attributes of formal structure other than size. Chandler's (1962) account of the transformation of large corporate structure suggests that one benefit of rearrangement of units and decentralization was decreased administrative overhead. A third possible interpretation is that while parameters or organizational structure other than size are basically unrelated to one another, they have important consequences for people's behavior in organizations, for relationships between organizations and their clients and competitors, and for the ability of organizations to incorporate and use new techniques and technologies effectively. This last possibility will be pursued in continuing research on the 254 departments of finance.

The most important finding to emerge from this preliminary study, however, is that one cannot underestimate the impact of size on other characteristics of organizations. Size must always be controlled when analyzing relationships among properties of organizations. But constraints and boundary conditions inherent in cross-sectional data often inflate or distort the effects of size; hence, size cannot truly be taken into account in studies that examine sets of organizations at only one point in time. The solution to this dilemma is the longitudinal design which is time-consuming, expensive, and fraught with uncertainties about lag times. But the longitudinal approach is the only solution. It is hoped that students of organizations will have the patience and

enthusiasm to collect data which are adequate to test causal hypotheses. Without these data, a theory of organizations based on predictive and non-obvious propositions can never emerge.

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STRATEGIES FOR ORGANIZATIONAL INNOVATION: AN EMPIRICAL COMPARISON *

RONALD G. CORWIN

The Ohio State University

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The paper identifies several sociological strategies of intervention used in a national effort to reform social organizations and compares their relative effectiveness. Different streams of sociological literature dealing with unplanned social change suggested concepts and variables that might apply to the problem of deliberate organizational reform. Indicators were then derived from questionnaires and interviews with new teacher-interns, experienced teachers, and college professors who were part of a national experiment to train teachers for low income schools (the Teacher Corps program). The analysis is based on forty-two public schools and ten universities located throughout the United States. The dependent variable is the number (and innovativeness) of new technologies introduced into schools through the program (derived from a content analysis of interviews).

A factor analysis of thirty-five indicators revealed seven factors underlying the different explanations. A regression analysis based on these factors indicated that approximately half of the variance in the dependent variable could be explained. Technological innovation appeared to be produced by a combination of: (a) a dominant outside organization staffed by competent and liberal members; (b) competent, receptive boundary personnel in the host organization; and (c) functional interdependence and channels for cooperation to take place. These conditions underscore the importance of characteristics of the general organizational context in which innovation is taking place. The findings also suggest that a mixture of conceptual approaches is needed to explain organizational change.

THE capacity of modern man to control his destiny in a changing organizational society hinges on his collective ability to deliberately change the key formal organizations. Many attempts to intervene in organizations have been recorded in the human relations literature (Roethlisberger and Dickson, 1939; Coch and French, 1948; Likert, 1961; Bennis, 1966; cf. Leavitt, 1965). This literature reports several attempts to modify organizational structure, usually as a means of improving the task performance of the people (e.g., Morse and Reimer, 1956; Ginzberg and Reilly, 1957; Chapple and Sayles, 1961). However, compared to the importance of the topic, with notable exceptions (McCleery, 1957; Burns and Stalker, 1961; Aiken and Hage, 1970) sociologists have devoted scant attention to the frequent attempts being made today to intervene in organizations (Blau and Scott, 1962). Virtually no consideration has been given to

the relative effectiveness of different strategies of reforming organizations. Perhaps because social theorists customarily attributed an independent life to social systems, which often seem beyond the control of man, they have been preoccupied with unplanned social change.

But, the literature can nevertheless suggest some critical concepts and types of variables that might help identify sociological intervention strategies and compare their relative effectiveness. Writers representing diverse and overlapping streams of thought have stressed several types of variables. It has been postulated that an organization can be more easily changed:

- If it is invaded by liberal, creative and unconventional outsiders with fresh perspectives (Gouldner, 1954; McCleery, 1957; March and Simon, 1958; Clark, 1960; Carlson, 1962; Shepard, 1967; Pareto, 1968);
- If those outsiders are exposed to creative, competent, flexible socialization agents (Giddings, 1897; Baldwin, 1911; Thomas and Znaniecki, 1918-1920; Cooley, 1922; Dewey, 1922; G. H. Mead, 1934; Newcomb, 1943; Clausen, 1968);
- If it is staffed by young, flexible, supportive, and competent boundary personnel, or "gatekeepers" (Tarde, 1890; Kroeber, 1944; Menzel, 1960; Riley and Riley, 1962; Rogers, 1962; Berelson and Steiner, 1964);

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- If it is structurally complex and decentralized (Durkheim, 1933; Cillie, 1940; March and Simon, 1958; Loomis, 1959; Burns and Stalker, 1961; Wilson, 1963; Berelson and Steiner, 1964; Greiner, 1965; Leavitt, 1965; Carroll, 1967; Sapolsky, 1967; Thompson, 1967; Aiken and Hage, 1968, 1970; Clark, 1968; Evans, 1968);
- If it has the outside funds to provide the "organizational" slack necessary to lessen the cost of innovation (Guetzkow, 1965);
- If its members have positions that are sufficiently secure and protected from the status risks involved in change (Thompson, 1967);
- If it is located in a changing, modern, urbanized setting, where it is in close cooperation with a coalition of other cosmopolitan organizations that can supplement its skills and resources (Litwak and Hylton, 1962; Putney, 1962; Wayland, 1964; Clark, 1965; Evan, 1965; Terreberry, 1965; Becker and Stafford, 1967; Evan and Black, 1967; Aiken and Hage, 1968; Turk, 1970).

These variables pertain to characteristics of new members, of the socialization agents, of the boundary personnel, of the organizational structure, of the resource pool, of the members' status and of the organization's social and interorganizational context. The antecedents can be traced to several overlapping macro-theories of change identified by Hagen (1962); Moore (1963); Smelser (1967); Applebaum (1970), and others.

SETTING OF THE STUDY

Because many government programs are attempting to manipulate precisely the structural variables which theory suggests are important, they should be of central interest to sociologists. The data reported here were taken from a larger, longitudinal study of the Teacher Corps, a nationwide, multi-million dollar government program whose aim is to promote educational reforms in low income schools through innovative teacher training programs, especially by opening a path to teaching for liberal arts graduates and members of minority groups who otherwise might not have entered the profession.¹ Because it was de-

signed to attack complex social problems on a national scale, the program has tried to mobilize a coalition of organizations, each of which contributes special resources and skills to improve teaching in low income schools.

A special two-year internship allows interns to be employed in several cooperating public schools, while attending college part-time. Colleges of education are expected to experiment with curriculum and personnel policies, and to establish cooperative relationships with schools and communities and with other departments in the university. In the words of one Teacher Corps brochure, the program seeks to use the team approach for training purposes, to provide for special and continuous supervision of interns, to incorporate the spirit of the Peace Corps, to establish a closer relationship between theory and practice, to set up a route into teaching for some who might otherwise have been excluded, and to involve the community as a training ground by extending the walls of the classroom into the community. The program is divisible into two main parts and several components: a pre-service program for interns, which includes graduate courses in education as well as some observation of schools and/or the community; and a tri-partite, in-service period consisting of university study, a practical internship in poverty schools and experience with poverty children and their families in the community.

Typically, a group of thirty to fifty liberal arts graduates (interns) and several experienced teachers who serve as team leaders receive about eight weeks of special pre-service training at a college or university. Following pre-service the group is broken into four to six teams composed of

were more politically liberal and held less conservative opinions; were more likely to want to work in racially integrated schools; more often expected to continue teaching in poverty area schools; identified more closely with students' interests; subscribed to a more *laissez faire* attitude toward student discipline in the classroom; placed more emphasis on developing the creative capacities of children and allowing them to do what they think is important (compared to teaching knowledge of subject matter or respecting authority or following instructions); and expressed more confidence in the ability of their pupils to learn and to graduate from high school.

¹ Chi square analyses of a battery of opinion items and scales indicated that, compared to other new teachers (N=162) and experienced teachers (N=578), the interns (N=158) more closely conformed to a liberal, humanitarian, ideal type of teacher. Specifically, they came disproportionately from professional, technical, and managerial homes; expressed less loyalty to the school administration;

three to eight interns and one team leader. Each team is assigned to a school serving a poverty area, usually an elementary school where they spend 60 percent or more of their time on a weekly basis. In the beginning they may work with small groups of students on specific lesson plans. As they gain experience, their tasks become more complex. They have experimented with cross-age tutoring, socio-drama, multi-ethnic centers, black history courses, multilingual programs, etc. They are expected to spend about 20 percent of their time with academic coursework at the university, which includes inter-disciplinary work in the social sciences as well as in colleges of education, and leads to teacher certification and a masters degree in two years. Finally, the interns are supposed to devote about 20 percent of their time on community activities, conducting surveys, working with social agencies, visiting parents, and participating in civic and (sometimes) political activities to familiarize themselves with the environment in which their students have grown up, and to try to bridge the gap between the school and the low income community it serves.

There has been a pervasive goal conflict between the national office, which stresses the objective of educational reform (Graham, 1968), and the local schools which attempt to use the resources provided by the Teacher Corps to supplement their existing programs. The assumptions of this program are at variance with the traditional training model, which holds veteran professionals responsible for socializing the novices. Interns have been led to view themselves as autonomous colleagues of the experienced teachers and as agents of change, a view that is not shared by most teachers and many professors, who insist on placing the interns in traditional teacher-trainee roles. More fundamentally, the entire program attempts to act as a unique type of change agent, consisting of a complex network of organizations acting in concert and designed to promote innovation within member organizations and in organizations outside the immediate network. Generally, the literature has overlooked organizations in the role of agents of change intent on changing other organizations. But the liter-

ature on interorganizational relationships (Aiken and Hage, 1968) provides a starting point, and the Teacher Corps can be used as one strategic case.

PROCEDURES

The Sample

Ten universities with Teacher Corps programs and forty-two cooperating schools were visited for one week by teams of interviewers during the fall and winter of 1968-1969.² The ten programs were purpos-

² The following universities were included in the study: University of Miami, St. Cloud State College, Pacific Lutheran University, Livingston State University, University of Missouri at Kansas City, University of Southern California, University of Nebraska at Omaha, New Mexico State University, Temple University, and East Tennessee State University. The total questionnaire sample includes: 266 interns, 872 classroom teachers, 53 team leaders, 60 principals and other school administrators, 100 graduate students in education not in the Teacher Corps, and 45 university faculty members, deans, and program directors. On the second visits questionnaires were returned by 117 interns, 24 team leaders, 107 teachers cooperating in the program, 26 principals and other administrators, and 31 university faculty members and program directors. Nine-hundred and thirty-two persons were interviewed on the first visit and 186 were interviewed twice. The average rate of questionnaire return on the first visit was 73%, and for the second visit, 92%. In addition to the four experimental schools at each location, a "comparison" school was included in the study, which was not participating in the Teacher Corps but which was matched as closely as possible to the participating schools (on the basis of their mean proportion of students from families at the poverty level, racial compositions of faculty and student body, and proportion of teachers with advanced degrees). These do not, of course, constitute a control group in any rigorous sense; and in any event, space precludes systematic discussion of these results here except to note that the results do not change the conclusions and added too little to the interpretation to warrant inclusion here. However, anticipating the following analysis, one finding may be worth noting here: on the measure of "organizational flexibility," the comparison schools were more *receptive* to change than the participating schools; but on the measure of innovation, they were actually undergoing *less actual change*. From our interviews and observations, it appeared that on the one hand, the receptivity of the comparison schools had not been dampened by the aggressiveness of interns who had threatened teachers in the participating schools; organizational flexibility was inversely correlated with the predisposition of the interns to take initiative, the proportion who were males, who majored in the liberal arts, who identified themselves as politically very liberal, and who pre-

ively selected from thirty-five scheduled for operation in the fall of 1968, and represented a range of types and reputed quality.³ Rural and urban programs in every region of the country are represented in the sample; four of the programs service Negro children and two service Mexican-Americans and Indians.

Indicators and Measures

Thirty-seven indicators were used to guide the collection of data treating some of the concepts and variables identified earlier. In effect, these indicators represent the independent variables in this analysis. A more detailed account of them and the rationale for their selection is presented elsewhere (Corwin, forthcoming). It will suffice here to acknowledge the problems involved in relating concepts to indicators, and the looseness of fit between the two in the social sciences (see Torgerson, 1958). The thirty-seven variables, which were assumed to be proxies for more complex phenomena, in turn were operationalized in terms of measures presented in Figure 1. They are grouped in the table according to the face validity initially used.

The dependent variable is technological change in schools. Each school was given a score based on all reported or observed intended and unintended changes, weighted for the "innovativeness" of each change (rated by two judges). Each innovation was scored on a three-point scale on each of five dimensions: new classroom methods,

ferred the creative type of teacher. These characteristics account for 58 percent of the variance; while characteristics of the schools and teachers accounted for only 6 percent. On the other hand, Teacher Corps teams, under the leadership of team leaders and with the cooperation of universities, often provided a catalyst to translate the potential for change into action, a factor that was not present in the comparison schools.

³ The reputed quality of the local program was based on ratings by the staff at the national Teacher Corps headquarters in Washington and by a panel which rated programs for refunding. In view of the small number of cases, it was decided to avoid programs that were reportedly failing and would not be likely to produce any change at all. None of the programs was rated poor at the time of selection and all were ranked in the upper third of the programs. The cooperating schools at each program were selected on a random basis when more than four schools were participating.

new materials, changed relationships with clientele, the addition of implementing personnel, and extra-curricular activities. The weighting was based on the assumption that changes requiring altered or new social relationships and those affecting the basic curriculum are of greater importance than additive changes or changes in extra-curricular activities or other peripheral activities. Thus, team teaching, the introduction of black history, mixed-age grouping, and the use of indigenous laymen, received higher weights than did the use of films or the establishment of a new photography club, for example. A similar procedure was used for weighting secondary changes which, in addition to the above dimensions, included reports of improved attitudes on the part of students or professors or teachers. A school's score, then, is a product of the number of planned and unplanned innovations times the weights assigned to each dimension of change that applied to that innovation.⁴

ANALYSIS

The analysis of data was organized around three steps. First, the degree and direction of association among the dependent and independent variables was computed. In addition to examining these relations, this step was undertaken to reduce

⁴ Elizabeth Hanna supervised this phase of the coding. The term, innovation, has been used in a variety of ways (cf. Hage and Dewar, 1971; Becker and Whisler, 1967). It is viewed here as a variable pertaining primarily to a deliberate change in structural relationships and procedures in a particular organization that could lead secondarily to changed outputs. Carroll (1967) used a comparable procedure to rate the innovativeness of curriculum changes in a medical school in terms of the extent of restructuring of relationships among the faculty and students required by the change. Another procedure was used by Evan and Black (1967) who asked respondents to describe one proposal that had been implemented and one that had been rejected by management. Most other investigators have used check lists to indicate the presence or absence of specific types of innovations (Becker and Stafford, 1967) or have asked executives to list the number of new programs and services implemented (Hage and Aiken, 1967; Hage and Dewar, 1971). The latter investigators used follow-up questions to distinguish an expansion of existing services from new programs.

the number of independent variables by eliminating any found to correlate highly with others.

The second step was to submit the thirty-seven independent variables to factor analysis. As was noted earlier, the explanatory perspectives used to delineate indicators overlap considerably; and the indicators themselves were selected and grouped primarily on face validity. The objective of the factor analysis was to identify inductively the common factors underlying the thirty-seven independent variables. The re-

sult was that (a) concepts were refined by identifying constructs more closely fitting the indicators and measures, and (b) the explanation of organizational innovation was clarified by reducing the multiplicity of independent variables to a few salient and more abstract factors.

The third step in the analysis was to examine the amount of variance in the dependent variable explained by the emergent factors individually and collectively. This step was accomplished through regression analysis.

Figure 1
INDICATORS

I. INDICATORS ASSOCIATED WITH CHARACTERISTICS OF NEW MEMBERS	
1. Mean political liberalism of interns	A nine-item Likert scale assessing political attitudes toward public policies (e.g., toward the welfare state). Split-half reliability, $r = .60$ adapted from a scale developed by Peterson.
2. Interns' emphasis on pupil control	Five Likert items concerning attitudes toward the rights of students to govern their own conduct and to participate in school policy decisions. Mean inter-item correlation, $r = .33$.
3. Difference in political liberalism of teachers and interns	Mean difference in (1).
4. Proportion of interns with liberal arts major	Percent of all interns in the school who have liberal arts major
5. Quality of interns' undergraduate college	Colleges in the program rated on a combined index of college resources (e.g., percent of Ph.D.'s and five other indicators) and selectivity of the student body. Based on work by Nash (1969) and Cass and Birnbaum (1970-71).
6. Mean percentage difference in status of interns and teachers in education, race, and sex	Percentage differences between teachers and interns in each school based on race, sex, education level and length of time in the region.
II. INDICATORS ASSOCIATED WITH CHARACTERISTICS OF THE BOUNDARY PERSONNEL	
7. Proportion of university faculty who consider themselves very liberal politically	Proportion of respondents in each school or college who identified themselves as radical or highly liberal (as opposed to moderately liberal or moderately or highly conservative); significantly correlated with scale distributions.
8. Political liberalism of the team leader	Same as above (7).
9. Competence of the school principal	Ratings of interns' supervisors by interns and other teachers in the school on six Likert-type items. Mean inter-item correlation $r = .56$.
10. Political liberalism of the classroom teachers	Same as (1).
11. Principals' support of the program	One Likert item describing interns', team leaders' and classroom teachers' opinions on the principal's support of the program.
12. Tenure of the principal	Total number of years in the school.
III. INDICATORS ASSOCIATED WITH CHARACTERISTICS OF THE SOCIALIZATION AGENTS	
13. Interns' satisfaction with university faculty	The mean scores of interns on a six-item Likert scale assessing their esteem toward their professors. (Mean inter-item correlation $r = .48$ adapted from a scale developed by Peterson).
14. Quality of the university	Same as (5).
15. Quality of school	Combined ratings of interns and teachers on four Likert items concerning level of satisfaction with the quality of teachers, administration, materials and pupils in the school. (Mean inter-item correlation, $r = .61$).
16. Competence of the team leader	Same as (9).
17. Teachers' emphasis on pupil control	Same as (2).
18. Proportion of teachers who prefer creative teachers	Proportion in each school who prefer the "creative" type of teacher over three other types described in short paragraphs; types differ on the emphasis placed on subject matter and on the exercise of adult authority (developed by Steber and Wilder, 1967).
IV. INDICATORS ASSOCIATED WITH THE ORGANIZATIONAL STRUCTURE	
19. Centralization of decision making within the school	Combined estimate of teachers and principals about where in the school or system final decisions are made about seven issues (such as permission to use team teaching).
20. Size of the university	Student enrollment.
21. Size of the school	Student enrollment.
22. Proportion of teachers who participated in the proposal	Based on self reports, by school.
V. INDICATORS OF RESOURCES AVAILABLE	
23. Ratio of federal funds allocated for the program per intern	Total funds divided by number of interns.

(Continued on p. 446)

(Figure 1. Continued from p. 445)

VI. INDICATORS OF STATUS SECURITY	
24. Proportion of program funds controlled by the local school	Percent of total Teacher Corps budget allocated to school.
25. Proportion of teachers who are members of union	Based on self report, by school.
26. Professional orientation of classroom teachers	School means on a 16 item Likert scale measure four role segments: orientation to the client; and to colleagues, and emphasis on teachers' decision-making authority, and on knowledge as a basis of competence; split-half reliability $r_n = .65$.
27. Client orientation of teachers	Sub scale of the above (26).
28. Administrative orientation of classroom teachers	Six Likert items concerning an individual's expressed loyalty to the school administration. Split-half reliability, $r = .81$ (Corwin, 1970).
29. Quality of teachers' undergraduate college	Same as (5).
30. Proportion of teachers with an M.A. degree	Based on self reports, by school.
31. Rules and procedures orientation of classroom teachers	School means on two sub-scales consisting of twelve Likert items, loyalty to the administration ($r_n = .81$) and emphasis on rules and procedures ($r_n = .84$).
VII. INDICATORS OF THE SOCIAL AND INTER-ORGANIZATIONAL CONTEXT	
32. Cooperation between the school and college	From a content analysis of interviews, each school was assigned a weighted score based on three dimensions: the frequency of its contact with the local college, the quality of the relationship and the amount of cooperation.
33. Modernization of the state	Level of economic and social modernization of each of the ten states in which the programs are located, based on a six component index developed through a factor analysis by Herriott and Hodgkins (1969).
34. Number of other training programs at the college	Highest number reported.
35. Size of the city	U.S. Census, metropolitan area.

Intercorrelation Among the Variables

The magnitude of correlations among the independent variables ranges from .00 to -.98. There was not a sufficiently high correlation among a significant number of variables to warrant making many substitutions.

The Independent Factors

All variables were submitted, ungrouped, to an oblique factor analysis. This procedure yielded seven dimensions (see Table 1).

The first factor refers to the social context of the training program. Four of the components refer to characteristics of the university (its size and quality); two pertain to characteristics of the local community and region (size and modernization). Large and competent organizations probably are characteristically found in modernized social settings.

The second factor reflects the teaching staff's cosmopolitanism and independence of judgment. The teachers' professionalism refers to the emphasis they place on their right and technical competence to make decisions, especially their responsibility for the students' welfare. The fact that such teachers do not exhibit a strong sense of loyalty to the school administration, coupled with their political liberalism, are qualities indicative (a) of their ability to act inde-

pendently of established authority structures and (b) of their receptivity to social change.

The third factor pertains to the amount of emphasis the organization places on maintaining internal control through controlling pupils, rules, and centralized decision making; the difficulty of maintaining control is probably associated with organizational size. The proportion of program funds controlled by the schools indicates the amount of control they exercise over the program (relative to the college's influence).

Factor four pertains to the school administration's competence and the administrative support given to the program. It consists of the principal's competence rating, his years of experience, the quality of his school, and his willingness to support the program.

The nature of the interorganizational relations between the schools and colleges is tapped by factor five. Characteristics of the boundary personnel hold the key to the relationship. The factor includes the competence and liberalism of the university faculty and of the team leader at the school and the amount of cooperation between the two institutions.

The sixth factor concerns the status of the teaching staff, as reflected in their level of formal education, the quality of their

undergraduate college, and the strength of their influence on the program proposal.

The last factor is comprised of the interns' characteristics, most of which reflect basic differences between them and the typical classroom teacher in the sample. The interns tended to be more liberal politically, to have graduated from better colleges and to place less emphasis on pupil control; and the two groups differed typically in race, sex, and level of education.

Explaining the Variance in Organizational Innovation

The seven factors account for 51 percent of the variance (Table 2). Three factors account for most of the explained variance (48 percent): (1) the quality and interdependence of the boundary personnel, which refers to characteristics of the university faculty and team leaders, including the cooperation between them ($B = .59$); (2) the organizational control exercised by the school ($B = -.23$); and (3) the uniqueness of the outside change agents ($B = -.39$). The status of the teaching staff ($B = -.10$), the quality and modernization of the context ($B = .16$), the competence of the administration ($B = .11$) and the professionalism and social liberalism of the staff ($B = .01$) contribute little to the explanation once the first three factors have been considered.

DISCUSSION

Relationship to the University. The most salient factor, then, is the quality and interdependence of the boundary personnel. This factor consists of four variables: the competence of the university faculty members (as rated by the interns); the faculty members' political liberalism (based on their own self-identity); the extent of cooperation between the school and the university (assessed from a content analysis of the interviews); and the competence of the experienced teacher-supervisors, the team leaders (based on the combined ratings of teachers and interns). In this connection, an inspection of Table 1 suggests that interns' ratings of the faculty members' competence ($r = .53$) and the faculty's political liberalism ($r = .66$) are probably more important than either the university's prestige or its resources. Perhaps the better universities are too socially remote

and too well integrated into the academic system to have developed the interest or competence for this kind of problem. Also, the better colleges tend to attract more liberal arts majors ($r = .61$) from better

Table 1. Component Loadings of Seven Factors Yield by an Oblique Factor Analysis*

1. Quality & Modernization of Context	
Size of university	(-.79) ($r = .32$)
Quality of university	(-.90) ($r = .17$)
Modernization of region	(-.91) ($r = .13$)
No. of other training programs	(-.69) ($r = .02$)
Federal funds allocated per intern	(-.63) ($r = .39$)
Size of city	(-.94) ($r = .19$)
2. Professionalism & Social Liberalism of Staff	
Professional orientation--teachers	(.83) ($r = .05$)
Administrative orientation--teachers	(-.42) ($r = .28$)
Liberalism--teachers	(.43) ($r = .12$)
Client orientation--teachers	(.86) ($r = .05$)
Liberalism of the team leader	(.57) ($r = .43$)
3. Organizational Control by the Schools	
Teachers' emphasis on pupil control	(.81) ($r = .11$)
Rules & Procedures orientation--teachers	(.43) ($r = .04$)
Centralization of decision-making	(.75) ($r = .33$)
Proportion of program funds locally controlled	(.63) ($r = .33$)
Size of school	(-.65) ($r = .09$)
4. Competence of the Administration	
Principals' support for program	(-.72) ($r = .05$)
Competence of the principal	(-.86) ($r = .15$)
Quality of the school	(-.75) ($r = .07$)
Tenure of the principal	(.22) ($r = .02$)
5. Quality & Interdependence of Boundary Personnel	
Interns' satisfaction with university faculty	(.66) ($r = .53$)
Proportion of university faculty who consider themselves very liberal politically	(.37) ($r = .66$)
Cooperation between school & college	(.93) ($r = .35$)
Competence of the team leader	(.29) ($r = .01$)

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(Table 1. Continued from p. 447)

6. Status of Teaching Staff	
Proportion of teachers with MA degree	(-.91) (r = .10)
Quality of teachers' undergraduate college	(-.44) (r = .14)
Proportion of teachers in the union	(-.63) (r = .25)
Proportion of teachers who participate in proposal	(-.52) (r = -.26)
7. Uniqueness of Outsiders	
Political liberalism of interns	(-.62) (r = -.32)
Proportion of interns with liberal arts major	(-.92) (r = -.19)
Interns' emphasis on pupil control	(.57) (r = .20)
Difference in political liberalism between teachers & interns	(-.46) (r = -.16)
Quality of interns' undergraduate college	(-.73) (r = .12)
Difference in status of teachers & interns based on education, race, & sex	(-.56) (r = -.32)

* The first figure (in parentheses) refers to the factor component loading from the rotated matrix. The second figure (in parentheses) refers to the zero-order correlation between each independent variable and the dependent variable.

undergraduate colleges ($r = .70$)—factors which already have been reported to be negatively associated with innovation (not reported in tables).

The overriding importance of the university faculty supports the contention of some writers that organizations change primarily in response to an outside stimulus. But, more specifically, it reinforces the generally acknowledged significance of interorganizational relationships, and underscores the crucial role played by boundary personnel in particular, in this case represented by the school's team leaders and the university faculty members. In this sense, the findings support the diffusion approach with its emphasis on the importance of the openness of boundary personnel to change. While the technical competence of the occupants of both boundary roles appears to be important, the attitude climate set by the univer-

sity faculty seems to be even more important.⁵

The dominating role of the university can be attributed to several sources. First, compared to schools, universities are more cosmopolitan, relying on regional or national financial and recruiting bases, and exporting their products throughout the nation and the world. In addition to this institutional base, faculty members probably tend to be more professionalized than teachers and less tied into the local political structure. Moreover, they have less investment in the existing technology of the schools (as opposed to their investment in their own institutions). Finally, the fact that universities are the prime contractors for this program, the program director being a faculty member, enhances the university's influence. However, this leverage for change, no matter how great, cannot be applied unless there is a channel through which the university can exert its influence, that is, a working relationship between the universities and schools. Competent and receptive team leaders are a necessary component of such a channel.

Comparison with Other Environmental Conditions. To put it another way, the major dimension of the environment contributing to innovation is the amount of cooperation between the school and the university. With that factor controlled, modernization of the state makes only a small contribution. There is some support for the contention that innovation can be promoted by adding outside resources, although in this case the volume of total funds is less important ($r_p = .11$) than the relative level of funding provided for each new trainee ($r_p = .40$). (Figures not reported in the tables.) There is, however, slightly more change in schools associated with larger universities ($r = .32$), but these variables are relatively unimportant compared to other factors examined. That the number of training programs at the university is inversely associated with

⁵ Even with the proportion of interns having liberal arts degrees and the amount of cooperation between the school and college controlled, the liberalism of the university faculty continues to contribute .50 to the R^2 ; whereas once these three variables have been controlled, faculty members' competence adds little more (R^2 added = .02).

Table 2. Seven Factors Correlated with the Introduction of New Technologies into Schools: Oblique Step-Wise Regression Analysis (N = 43).

No.	Factors (Title)	r	R ²	R ² added	Par- tial	T- Value	Beta Weight	F
5	Quality & Interdependence of Boundary Personnel	.64	.405	--	.600	4.37	.588	27.22
3	Organizational Control Exercised by the Schools	-.26	.445	.040	-.301	1.84	-.228	2.79
7	Uniqueness of Outside Change Agents	-.13	.475	.030	-.304	1.86	-.388	2.18
6	Status of Teaching Staff	.07	.493	.018	-.095	.56	-.095	1.34
1	Quality & Modernization of Context	.17	.503	.010	.155	.91	.161	.69
4	Competence of Administration	.08	.507	.004	.093	.55	.109	.31
2	Professionalism & Social Liberalism of Staff	-.10	.507	.000	.019	.11	.014	.01

program change ($r_p = -.40$) (not reported) suggests that the proposition that change occurs more readily in otherwise changing environments needs to be reconsidered. Perhaps we should allow for a ceiling effect, that is, the fact that it is also more difficult for a given program to make additional changes when there is already a high rate of change.⁶

Organizational Control. The second critical factor is the extent of internal control exercised by schools, consisting of four variables: centralization of the school system's decision-making structure; stress which classroom teachers and administrators place on rules and procedures; emphasis which teachers place on pupil control (i.e., strict discipline); and the proportion of program funds controlled by the schools.

This factor reflects the other side of the coin, i.e., the capacity of the cooperating, host organization to filter and shape the changes proposed by the cooperating outside organization. The first two variables form a "containment dimension," through which an organization develops the capacity to control its members and which determines how effectively it can regulate its members' reactions to proposed changes. Note that these two variables operate in opposite directions and that the positive correlation between the centralization of the decision-making structure and technological change

($r = .33$) is at variance with the widely held assumption that decentralized systems are more adaptable. The higher administration was usually responsible for introducing this program to the schools. Under these conditions, the more that power is concentrated at the top, the more effective the program will be (Griffiths, 1964). In other words, a centralized organization does not for that reason necessarily resist change; rather, centralization gives it the capacity to select and enforce innovations as well as to resist them. At the same time, where teachers emphasize classroom discipline, it probably both reflects and reinforces strong administrative control.

The tendency for innovations to decline as the proportion of funds controlled by the local schools increases ($r = -.33$) reflects the conflict in goals between these two institutions. Control over the funds is a lever for coopting the program. Schools tried to use funds to supplement their existing programs rather than to innovate. There may have been a slight tendency for the more competent principals to gain more control over program funds ($r = .22$) (not reported). While it is probably true that educational decisions are moving up and out, placing initiative outside the local system (Clark, 1965), local school boards often maintain final veto power over changes they do not want.

The Role of Dissonant Change Agents. The third factor reflects the amount of dissonance between the change agents and the members of the host organization. But con-

⁶ The number of other training programs ranged from one (at one university) to six (at two others); the typical college of education had between three and four separate training programs.

trary to the theory, the correlation is negative. Independent field observations and interviews also confirmed that interns were often a source of friction and tension in the local schools, and that schools tended to react defensively when even a few of these critical, liberal, change-oriented newcomers were introduced.⁷ Perhaps the resulting conflict was not entirely counter-productive. There were small positive correlations between innovation and (a) the amount of conflict between interns and other members of the program ($r = .26$), and (b) the number of teachers reporting that the program had created problems for them ($r = .22$) (not reported). These correlations support the notion that conflict often accompanies change. But the fact that the correlations are not higher also suggests that conflict does not necessarily produce extensive change under certain circumstances.

Professional Competence. Finally, despite the theoretical importance one might attribute to variables relating to the professionalism and competence of the administrators and teachers, these variables were less important than the characteristics of other boundary personnel more directly associated with the program. Teachers' participation in the proposal is the other side of administrative centralization discussed above and is related to the dimension of teachers' competence. Their participation had only a minor effect; and contrary to usual expectations and findings from other studies (Aiken and Hage, 1970), the small effect which does appear is negative ($r = -.26$). The power equalization model presumes that changes are produced from the consent

of the parties involved. However, some innovations are forced into a system, which arouses the incumbents' suspicion or opposition. This was often the case in this program. When subordinates fail to agree on the objectives or strategies of these proposed changes, but are relatively professionalized, their participation in the decisions places them in a better position to sabotage the innovation (Mulder, 1971); and under conditions when their participation is only nominal, the strategy can backfire by producing unrealistic expectations (Lefton, Diniz and Pasamanick, 1959). Thus, this strategy can inhibit as well as promote innovation, depending on the initial balance of power, the degree of consensus, and the status threat involved.

It appears that neither (a) status security, as reflected in the teachers' competence, or (b) their professionalism promote change, judging from the negative correlation in the first instance and the low correlation in the second. Perhaps even more important note that, in view of the theoretical importance often attributed to the more generic social context, the characteristics which made up the quality and the modernization of the school context did not explain much. Again, the more immediate organization context reflected in the cooperation with the university, appeared to take precedence over these more general context variables.

These latter findings may have implications for the current decentralization issue concerning demands for community control being made by ghetto residents in large cities. On the one hand, the data support critics' contentions that the professionalism and technical competence of teachers in itself is no assurance that efforts will be made to improve the schools. On the other hand, administrative decentralization could reduce the capacity of central administrators to impose change from the top down; and more important, perhaps the whole controversy over the schools' relationship with their communities has eclipsed an equally important relationship—namely the relationship of the schools with innovative universities.

SUMMARY AND DISCUSSION

The findings should be interpreted with caution in view of the necessarily crude

⁷ That the presence of unconventional, change-oriented newcomers created problems in the schools is reflected in three other analyses. The characteristics of interns contributed to the explanation of both (a) the number of conflict incidents reported among members of the program, and (b) the proportion of teachers reporting that the program had created problems for them. The difference in the liberalism of the interns and teachers, and the proportion of liberal arts majors were both correlated with each variable. The defensiveness of schools in the face of this conflict is also revealed in (c) parallel effects which the presence of liberal newcomers appears to have had in promoting organizational inflexibility; five characteristics of the interns explain more than half the variance ($R^2 = .55$).

ways in which some of the complex concepts were measured. But a comparative analysis of alternative explanations, despite its tentativeness, points the way to more systematic models of organizational innovation. Toward this end, by drawing on the assumptions of other writers (Gouldner, 1970; Gerard, 1957; Clark, 1968) and by extrapolating from the tentative findings reported here, we shall offer some general propositions:

- (1) *the way an innovation is conceived and implemented is a product of a combination of forces inside and outside the organization.* Organizations exist in an environment of constraint; and therefore, the success or failure of an innovation will vary with the context in which it is introduced. This might include characteristics such as the quality of the institutions involved and modernization of the region, but at least in this case the data indicated that the characteristics of the general organizational network were the environment's most salient aspects. More specifically, technological innovation was a product of a balance of power between strong but interdependent organizations. The necessary ingredients include: (a) a dominant outside organization staffed by competent and liberal members; (b) competent, receptive boundary personnel in the host organization; and (c) functional interdependence and channels for cooperation to take place. These conditions can be reformulated to apply to characteristics of the organizational set itself: i.e., there will be more technological change in organizational networks in which there is unequal balance of power, but a high degree of interdependence among the organizations, and in which boundary roles are staffed by cosmopolitan, liberal, and professionally competent members.
- (2) *characteristics of both occupation and organization must be taken into account in order to explain innovation.* That is, the outcomes of attempts to change an occupation are conditioned by the characteristics of its organizational context, such as the centralization of the decision-making structure and standardization; whereas organizational innovations are influenced by the characteristics of the occupational group, such as in this instance, the teachers' emphasis on pupil control. However, in this study, the competence, professionalism and social liberalism of the rank-and-file professionals in the host organization were less important than either the comparable characteristics of the boundary person-

nel or the characteristics of the organizational structure.

- (3) *a split develops between the established leaders of the profession and a new generation.* The latter is identified with outside leaders, who stand to gain from new roles that promise to open new channels of success and to circumvent the status monopoly controlled by traditionalists. Conversely, since the proposed changes would redistribute existing tasks that were formerly the responsibility of other units, the newcomers are resisted; a dialectic develops in which the traditional, conservative procedures are reasserted or slightly adapted in order to compete with new procedures.

This last point deserves further comment. An attempt to influence the change process by introducing change agents as an independent force can be effective only under limited conditions, which were not present in this case. Even with the two most influential factors controlled, the more liberal the interns in a school, the less innovation that occurred. This fact indicates that their ineffectiveness was not simply a product of the institutional balance of power, but was inherent to the strategy itself as employed in this program. The program's tactics of course, provide an extreme test of the replacement approach. The attempt to marry the change-agent roles to the apprenticeship system placed the interns in a precarious position between two powerful organizations. They were representatives of the outside organization in the schools (i.e. the university), but could count on little direct support from remote university professors; while, they were directly supervised by defensive teachers. Sensing this resistance to them, and often finding the schools conservative toward change, in most programs a vocal minority of interns resorted to confrontation tactics. However, the conflict theory of change presumes a balance of power which did not exist in this case. As inexperienced newcomers to the profession, still in training and temporarily assigned to schools under direct supervision of experienced teachers, the interns could not gain leverage within the schools—even though, ironically, these very characteristics enabled them to maintain the autonomy which encouraged them to take risks involved in promoting change. Nor did the interns constitute a sufficiently

critical mass in any of the schools to provide power from numbers or to promote the development of a strong peer group. They were so outnumbered and overwhelmed by the structural defenses available to school administrators and teachers that the schools were able to neutralize their efforts. Indeed, the interns' militancy gave the teachers little latitude to compromise without jeopardizing their authority, which created a win-lose situation. Teachers retaliated by completely withdrawing their support for interns' proposals. Thus, while some change accompanied conflict, the fact that interns had little leverage from which to wage a successful conflict helps explain the negative correlation between technological change and the proportion of liberal arts interns in the program.

This suggests that two additional conditions must be present which were not present in this case: (a) the change agents must be introduced in sufficiently large numbers, and (b) at more than one echelon in the hierarchy. The impact might have been modified either by assigning a higher ratio of interns to each school, or by recruiting interns into the administrative ranks. But in any event, the experience in this program suggests that caution should be exercised in using the replacement strategy for innovation without further examining the conditions under which it can be successful.

In conclusion, explaining technological innovation requires a *combination* of several conceptual approaches. The pattern of variables identified here has moved beyond the framework which initially guided the study design. It appears that there is a great deal of both truth and fiction in the various streams of thought which served as the original guidelines. The various types of variables seem to supplement one another. But, even in combination, the variables explored here explain only part of the total variance. Some strategies may be more effective than others, but the crucial element seems to be the way they interact. The conditions under which any given strategy is applied, that is, the situation into which an innovation is introduced, seems to be as critical as the strategy itself. Perhaps the fundamental dimension underlying the variables considered here is the way local organizations are

insulated from, or integrated into, the larger context. Thus, decentralization gives local interests leverage over unwanted changes, while centralization more closely links the system to the broader system, as do institutional cooperation and liberal and competent boundary personnel. Future research will have to give more attention to the conditions which enable organizations to maintain functional autonomy within an increasingly interdependent, changing society (Gouldner, 1959; Katz, 1968).

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THE GROWTH OF SCIENCE AND ECONOMIC DEVELOPMENT *

MARION BLUTE
University of Toronto

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While most indicators of scientific activity have been shown to be growing through time at an exponential rate, a cross-sectional analysis suggests that scientific growth does not greatly outstrip economic growth. Power functions relating percentage increases between variables in a linear or non-accelerating fashion were found to approximate the relationship between scientific and technical publications in 1961 and five indices of the early '60's level of economic development better than several other function types, including the exponential. Depending on the economic development index used, a 10% increase in the latter was found to be associated with an increase of between 7.5% and 12.5% in scientific and technical publishing. A table of residuals or nation-by-nation deviations of the actual publications scores from two of the fitted power functions is included to suggest other variables which would have to be introduced into a fuller model of scientific growth.

THIS century's presumed explosive scientific growth, not viewed as ever closer approximations to truth¹ but simply as activity occupying man's time costing money, consuming forests and flooding libraries, has led to scientific attempts to measure the phenomenon, technical attempts to cope with it, and humanistic attempts to assess its significance.² This paper aims, first, to demonstrate with an often used index (the number of scientific and technical serials published), that the phenomenon of the "growth of science" has been exaggerated by the techniques used to analyze it and second to examine the non-economic social and political conditions which might foster scientific activity.

The Growth of Science as an Exponential Function

Interest in this question is not new. In 1908, Darmstaeder, a German, published a dated list of discoveries and inventions.

* The author most gratefully acknowledges the inspiration of Dr Lewis S. Feuer, Professor of Sociology, University of Toronto, in raising this problem originally and his advice in the course of its examination, while at the same time accepting full responsibility for any weaknesses in the analysis or conclusions.

¹ For two conflicting viewpoints on this see Popper (1963) and Kuhn (1962).

² Kochen (1967) presents a technical slant and Sweeney (1966) a more humanistic one.

In the quarter of a century ending in 1400 there were 33 listed; for the quarter ending in 1500 he recorded 50; in the similar period ending 1600 he recorded 127; for 1700, 218, for 1800, 680; and for 1900, 2,880. (Ogburn, 1932:27)

In 1932, William F. Ogburn, viewing Darmstaeder's statistics, observed that "the curve of growth moves upward at an ever-increasing rate . . . somewhat as compound interest grows" and referred to it as "an exponential curve" (1932:28). He concluded that society must use adult education to cope with the combined trends toward early marriage and "a vastly increased volume of total knowledge." (1932:29)

Of the more recent attempts to measure this growth, those of Derek J. de Solla Price (1961, 1963) have received much attention. Price studied a number of indices of scientific activity (ranging from the number of publications in several fields to the number of known chemical compounds) and concluded:

All of these show with impressive consistency and regularity that if any sufficiently large segment of science is measured in any reasonable way, the normal mode of growth is exponential. This is to say, science grows at compound interest, multiplying by some fixed amount in equal periods of time. Mathematically, the law of exponential growth follows from the simple condition that at any time the rate of growth is proportional to the size of the population or to the total magnitude

already achieved—the bigger a thing is, the faster it grows. In this respect it agrees with the common natural law of growth governing the number of human beings in the population of the world or of a particular country, the number of fruit flies growing in a colony in a bottle, or the number of miles of railroad built in the early industrial revolution . . . such an empirical law of growth for science . . . is so far-reaching that I have no hesitation in suggesting it as the fundamental law of any analysis of science (Price, 1963:4-5).

Price further estimated the doubling time of these indices to be between ten and fifteen years (1963:6) and declared that the scientific explosion is "dwarfing all other explosions of non-scientific human growth" (1963:14). Because such growth rates could not continue indefinitely, he reasoned that what appeared to be an exponential function, must in fact be semi-logarithmic and was approaching a ceiling (1963:20-31). Since these estimates were published the odd cautionary note has been introduced (cf. Mantell, 1966) but in general, they have been confirmed (cf. Holt, 1968).

Trends and Laws

The conclusion that science (as measured by literary productivity for example) has been growing through time at an exponential rate is not at issue here. So have many other things. At issue is the old adage that one may not divorce a trend from its conditions. A scientific explanation of a thing or event is given in terms of its "relatedness" to other things or events rather than in terms of something internal to itself. This relatedness is expressed in two ways: first, as a set of variables and their coefficients of which y is said to be a function; and second, as one or more boundary or initial conditions under which the relationship is said to hold. The latter of course have the same conceptual status as the former, but are not explicitly included in the equation because our knowledge of their effect is limited to a single value or subset of their possible values. Coleman points to the absence of this relational feature when he refers to a differential equation in one unknown

$$\frac{dy}{dt} = a + by$$

as expressing "immanent change" (1968:435), and Blalock points to its absence when he comments that "expressing a change in x as a function of time is an admission of one's ignorance as to the causal dynamics" (1969:91).³ Most scientists I believe then, would be loath to accept as "law," Price's documented trend that science has been growing at an exponential rate.

The point is simple but important to the logic of the study of social change, and despite the voices raised in protest, the problem persists. Karl Popper, for example, objected to the status of "historical" theories as macro-equivalents of time-series data analysis projected as unconditional trends (1961). More recently, Robert Nisbet has criticized both classical and contemporary sociological theories of development for the image of immanent change which they convey (1969). Today, each time an investigator treating categories of a single variable uses empirical frequencies rather than theoretical considerations to yield probabilities for applying a Markov chain model, an unconditional and unconditional proposition is being formulated in another guise. Moreover, these diverse methodologies manifest a concern for predicting history rather than variable properties of social systems—in the precise and limited way science uses the term "prediction." This was certainly true of the classical sociologists pursuing the great questions about mankind's fate, but it is worth noting here that Markov chain models entered sociology from mathematics through the applied science of industrial engineering.

None of this should be taken to deny the utility of empirical time-series generalizations either to science or the world of practical affairs. For an applied science seeking

³ The idea of a cybernetic system has, of course, modified this requirement somewhat. But it makes little sense to think of something as determined solely by its own feedback. Customarily a feedback term is one of several—the others, expressions involving variables other than y . We continue to think of such a function as incomplete however, and our eventual aim as Blalock points out is to replace such terms. (1969:95)

to predict to a complex and little-understood world, with modifications, they are often the only rational alternative. Furthermore, such generalizations can be important to pure science. As Blalock puts it, they "may serve as a challenge to explain why" (1969:91); and in some cases, such information may be combined with that yielded by cross-sectional analysis to estimate the "true" parameters of relationships. However, pure science does not seek them for their own sake. Our central concern must be to seek conditional relationships rather than the time paths of single variables.

Two Questions

To return to our subject, though we may never understand the complex of inter-related variables enough to parcel out "causes," we can in general associate the growth of science and technology with economic development. It makes more sense therefore to ask first, not "What function relates scientific and technical productivity to time?" but "What function relates it to economic development?" And second, "Do nations showing the greatest deviations from this function (of whatever shape) group in ways which suggest other major social and political factors affecting scientific activity?"

The Growth of Science and Economic Development

The total number of current scientific and technical serials published as of 1961 was obtained for forty nations and groups of nations from Gottschalk and Desmond's "Worldwide Census," conducted by the Science and Technology Division of the U.S. Library of Congress (1963),⁴ and

⁴ The census included the natural, physical and engineering sciences but excluded the social sciences except for some "hard" branches. It was compiled by working with bibliographic sources and by contacting leading documentationists in a number of countries. They "strived for comprehensiveness" in their own words and estimated the world's total of current titles at 35,000 plus or minus 10%. Another and improved estimate was compiled by K. P. Barr of the National Lending Library for Science and Technology, Boston Spa (1967). Barr estimated a world total of 20,000 titles by including

adjusted to an index of the number of publications per millions of population, using U.N. midyear population estimates for 1961 (U.N. Statistical Office, 1966: Table 4).⁵ Two indices of economic development were used as follows:

- (1) Millions of KWhrs of electrical energy produced in 1961 (U.N. Statistical Office, 1969: Table 138) adjusted to per 10,000 population (U.N. Statistical Office, 1966: Table 4)
- (2) Per Capita gross domestic product at factor cost in 1963 in hundreds of U.S. dollars (U.N. Statistical Office, 1969: Table 182 divided by 10^2).⁶

only those the NLL had successfully obtained, but he provides no nation-by-nation breakdown detailed enough to be useful; nor is such included in the NLL's published catalogue. In any case, for our purpose, the criteria of inclusion by acquisition may have rendered this more recent estimate more biased than the across-the-board inflation of the census estimate, particularly against nations at a greater linguistic and political distance from the English-speaking west. Barr points out for example, that China obviously has more serials than the NLL has been able to obtain. We considered trying to estimate the relationship's "true" parameters by cutting the former estimates by a percentage to reduce them to the NLL total for the first cross-section, and by using the half-dozen or so countries given by the NLL common to the first estimate for the second cross-section. However, we decided that the drastic difference in N's and the different selection criteria could only lead to misleading or unintelligible results.

⁵ Where the "Worldwide Census" combined totals for nations, the population totals were naturally also combined in calculating the publications index. Gottschalk and Desmond's "other" category was not used.

⁶ For Index 2, data for 1963 was used in the absence of comparable statistics for 1961. The statistical yearbook remarks that Index 2 is "designed to facilitate international comparisons of levels of economic activity." It "should be considered as [an] indicator of the per capita production of goods and services of the countries represented and not as [a] measure of the standard of living of their inhabitants. . . . A special effort has been made made to present a complete set of estimates for the year 1963." (United Nations Statistical Office, 1969: 562) The estimates however were not complete; and from the forty nations and groups of nations included in Index 1, the USSR, Poland, China, Czechoslovakia, Yugoslavia, Hungary, Rumania, Bulgaria and North Korea were dropped when Index 2 was employed. In addition, Germany was dropped since the publications and electrical energy indices combined east and west which was impossible here. These changes account for the different n's on the two

Scattergrams suggested, as we expected, that the production of scientific and technical serials was a positive function of each economic index, and we attempted a number of fits using least squares techniques. Table 1 presents the overall results.

These results are consistent. The power functions are clearly the superior fits, while the exponentials and two polynomial types are poor second choices with little to distinguish from among them. The semi-logs lag further and the straight lines further yet.⁷

indices. It should be stressed that this change in no way invalidates the findings. The two economic indices were employed as repetitively demonstrating similar general relationships with scientific and technical literary productivity; and of course, in each case, the appropriate publications index, totals and n's were used in the calculations.

⁷ Analysis of variance tests for the significance of r for both indices for all fits are significant at the .001 level, a not too surprising finding given the size of the n's. Direct tests for the improvement in fits of the different function types are complicated somewhat by the fact that four were fit to y and two to log y but especially by the fact that except for the second and third degree polynomials, the same number of parameters (two) are used. Hence the method chosen following Blalock's application to a single case (1960:315-317) was to compare the variation explained by categorizing the x values with that explained by each of the fits separately, expecting that the correlation ratio based on these categories would be significantly greater than r² for the worse fits but not for the better ones. This was found to be the case.

$$F = \frac{(E^2 - r^2)(N - k)}{(1 - E^2)(k - 2)}$$

(where E² = the correlation ratio or the proportion of variation in y explained by the categories and

A close look at the results for one index makes clear that *scientific and technical literary productivity is only a moderately accelerating function of economic development and not the shockingly accelerating exponential function of the time series analyses. In fact, percentage changes in the former are only a moderately positive, but not accelerating function of percentage changes in the latter.*

Figure 1 shows the data for index two and the fitted power function. The nature of this curve, shown as a straight line on a double logarithmic graph, may require some clarification. The general form of a power function, $y = ax^b$ indicates that y varies with some constant power of x. In this case, $y = 2.15x^{1.22}$. The slope is positive,

$$\frac{dy}{dx} = 2.62x^{-.22}$$

and increasing,

$$f'' = .58x^{-.73}$$

However, by taking the logarithm of both

k = the number of categories) is significant at the .05 level for the linear, semi-log and exponential fits for both indices allowing us to clearly reject these at least. Furthermore, for the power fits, r² is greater than E²; and tests of this difference with appropriate alterations in the formula above also yield F's significant at the .05 level. Further indications of the goodness of fit of the power functions is suggested by the residual distributions presented in Table 2, which show no systematic bias by the value of x and have no outliers, in fact, no values greater than 2.5 standard errors from the fitted curves.

Table 1. Correlation Coefficients for Six Function Types: Publications Fitted by Least Squares on Two Economic Indices (All Logs are to the Natural Base e).

Function Type and General Form		Index I n = 40	Index II n = 30
		r	
straight line	$y = a + bx$	.55	.68
semi-logarithmic	$y = a + b \log x$	.65	.72
exponential	$y = ae^{bx}$ or $\log y = \log a + bx$	.60	.78
2nd ⁰ polynomial	$y = a + bx + cx^2$	.69	.79
3rd ⁰ polynomial	$y = a + bx + cx^2 + dx^3$	.72	.80
power	$y = ax^b$ or $\log y = \log a + b \log x$	.89	.91

Table 2. Deviations in Standard Error Terms About Two Fitted Power Functions.

Index I		Index II	
Belgium	2.12	Taiwan	2.41
Denmark	2.03		
Portugal	1.38	Portugal	1.83
Switzerland	1.18	Belgium	1.52
		Austria	1.15
		Switzerland	1.03
France	.94	Latin America	.51
Netherlands	.91	Netherlands	.46
Taiwan	.80	Finland	.45
Austria	.71	Japan	.44
Yugoslavia	.64	Denmark	.36
Finland	.63	South Korea	.32
Thailand	.46	Norway	.29
Indonesia	.42	Sweden	.14
South Korea	.41	France	.05
Latin America	.41	Italy	.04
Hungary	.32		
Sweden	.20		
New Zealand	.19		
Italy	.18		
Bulgaria	.16		
Poland	.07		
Pakistan	.04		
Japan	.03		
Australia	-.03	New Zealand	-.15
Germany	-.06	India	-.29
Turkey	-.20	Thailand	-.40
Phillippines	-.24	Ireland	-.47
Ireland	-.24	United Kingdom	-.50
United Kingdom	-.32	Africa	-.55
Czechoslovakia	-.48	Spain	-.72
Greece	-.68	Indonesia	-.75
India	-.72	Australia	-.77
Rumania	-.73	Phillippines	-.91
Spain	-.86	Pakistan	-.91
Norway	-.96	Turkey	-.92
U.S.A.	-1.32	Greece	-1.52
Africa	-1.36	Canada	-1.62
U.S.S.R.	-1.80		
Canada	-1.81		
China	-1.95		
North Korea	-2.50	U.S.A.	-2.21

sides of this function, and stating it in the form

$$\log y = \log a + b \log x$$

one sees that the logarithm of y varies with some constant multiple of the logarithm of x ; that is, the relationship between the logarithm of x and that of y is linear. Figure 1 plots the actual x and y values rather than their logarithms, but on a scale illustrating the relationship between the latter.

The meaning of the fact that a power

function best approximates the relationship between scientific and technical literary productivity and economic development is perhaps intuitively grasped more readily than the technicalities of its representation. Double logarithmic graphs such as that in Figure 1 are used to help us visualize percentage changes in two variables. Equal distances along an axis of this type of graph represent equal percentage changes. (A change from 1 to 10 is represented by the same distance as a change from 10 to 100, i.e. both changes of 900%). Therefore, were a percentage increase in economic development accompanied by an identical percentage increase in science, the line would be at a 45 degree angle to the x axis. As we see, however, it is only moderately steeper. In fact, the slope of this line, as the linear form of the equation shows, is 1.22; i.e. an increase of 10% in economic development is accompanied by an increase of 12.2% in science. Simply put, *the slope of this function in its logarithmic or "proportional" form is a constant slightly greater than one, and it does not increase or accelerate with increasing values of x .*

Figure 2 shows this best-fitting power function as well as the straight line, the semi-logarithmic, the exponential and the third-degree polynomial fits for Index 2 again but on a natural scale for comparative purposes. We have explained that the elasticity, that is, the ratio of the percent change in science to the percent change in economic development of the power function for Index 2 was a constant, 1.22. The elasticity of the exponential function for the same index is $.16x$.⁸ This expression equals 1 at $x = 6.25$ (i.e. the point at which a percent change in one variable accompanies an equal percent change in the other), and equals 1.22 at $x = 7.6$. Thereafter, it continues to increase and at $x = 20$, is 3.2. Hence, the less-well-fitting exponential function underestimates the ratio of

⁸ This is derived as follows:

$$y = 4.18 e^{.16x}$$

$$\frac{dy}{dx} = (4.18)(.16)e^{.16x}$$

$$\frac{E_y}{E_x} = \frac{\frac{y}{y}}{\frac{x}{x}} \cdot \frac{\frac{dy}{dx}}{\frac{y}{y}} = \frac{x(4.18)(.16)(e^{.16x})}{(4.18)(e^{.16x})} = .16x$$

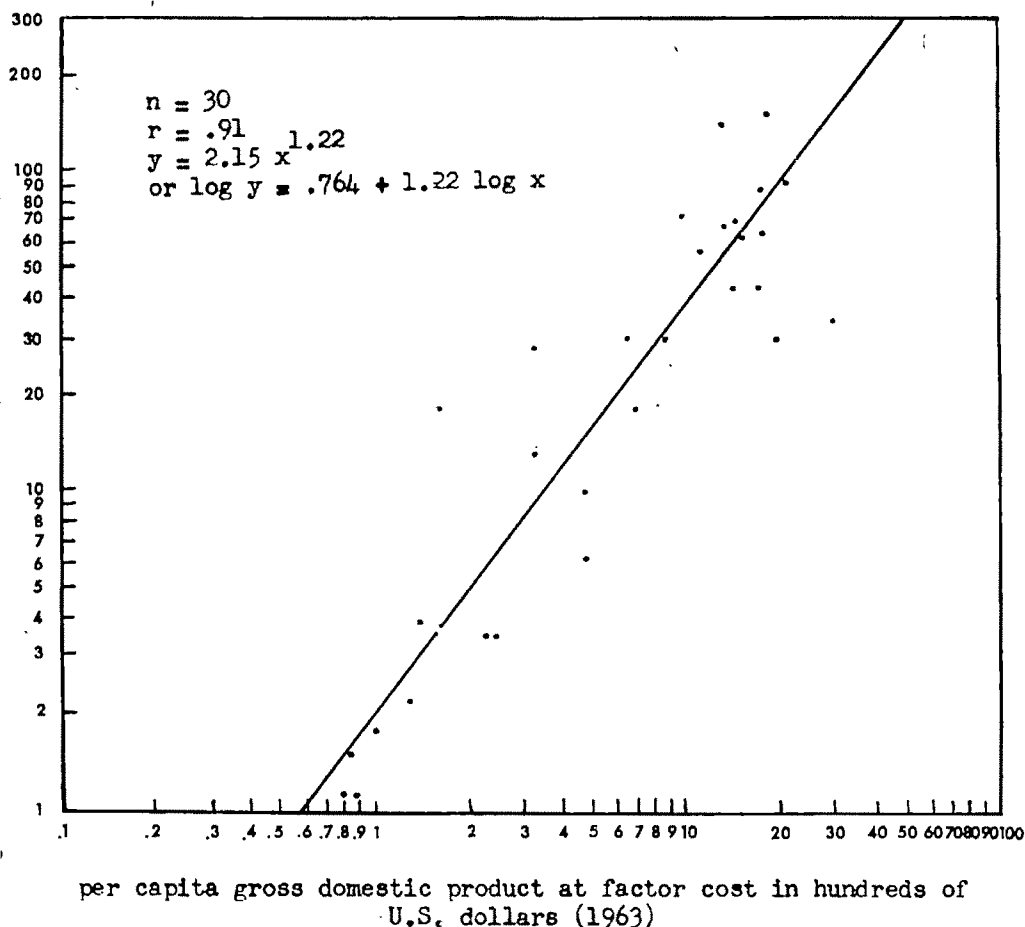


FIG. 1. DATA & FITTED POWER FUNCTION FOR INDEX 2

percent growth in science per unit percent growth in economic development at values of x less than 7.6 and exaggerates it thereafter.

Three other indices of economic development⁹ strongly confirm the above conclusions.

⁹ The three were:

- 1) Per capita national income in 1963 in hundreds of U.S. dollars. (U.N. Statistical Office, 1969: Table 182 divided by 10^3)
- 2) Per capita gross national product at market prices in 1963 in hundreds of U.S. dollars. (U.N. Statistical Office, 1969: Table 183 divided by 10^3)
- 3) Beckerman's proposed index of per capita real private consumption in 1960. (1966: Table 5)

The result (including significance tests) closely followed those for Index 2. In no case for example, did the correlation coefficient on any of these indices for any function type deviate by more than .03 from those presented for Index 2 in Table 1.

A slight exception to these generalizations is the first (electrical energy) index where these estimates' conservative nature is even more pronounced. Figure 3 shows the fitted exponential and power functions for this index. When economic development is measured by the production of electrical energy, an increase of 10% in the latter is accompanied by an increase of only 7.7% in scientific and technical literary productivity.

Other Factors

Having established power functions as best describing the relationship between the quantity of scientific and technical serials published and some common indices of economic development, our second question remains. Do the deviations for each

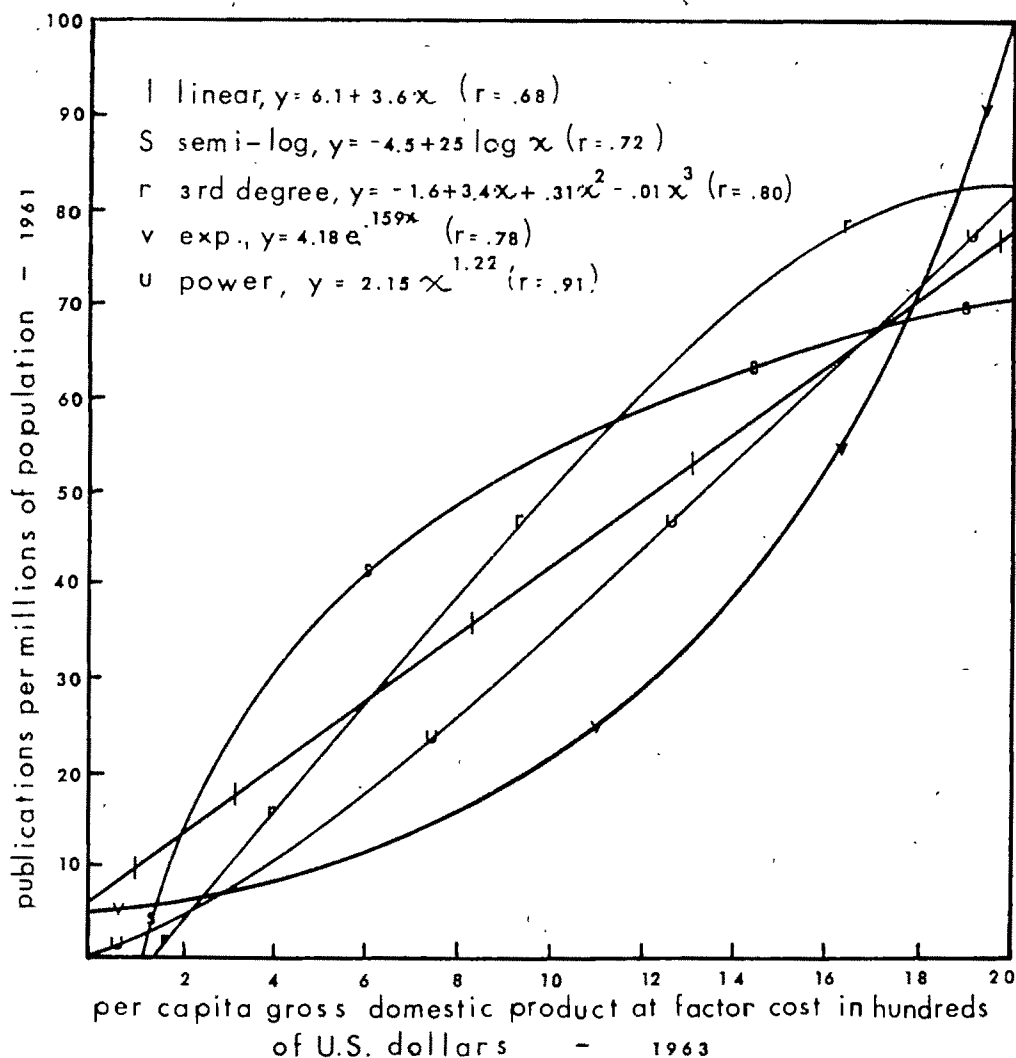


FIG. 2. FIVE FITTED FUNCTIONS FOR INDEX TWO

nation from these power functions group in meaningful or suggestive ways? Table 2 lists the included nations in order, from those showing the greatest positive deviations (or residuals) in standard error terms from their expected publications score to those showing the greatest negative ones.¹⁰

¹⁰ The derivation of these values is somewhat complicated. The standard error, $s_{y,x}$, a measure directly analogous to a standard deviation, is the distance on either side of the line of estimation within which we expect two-thirds of our actual publications scores to fall. The standard error of the linear or logarithmic form of the equation or

$$s_{\log y, \log x} = \sqrt{\frac{\sum (\log y_i - \log y_{ci})^2}{N}}$$

In other words, those nations closest to the horizontal line publish what we would expect given their level of economic development, while nations near the top and bottom of each column are furthest from what we would expect in the positive and negative directions respectively. Again, the results are fairly consistent for both indices

but is a variable function of x in the power form of the estimating equation. Hence

$$\begin{aligned}
 s_{y,x} &= (a)(x^b)(e^{s \log y \cdot \log x} - 1.) \\
 -s_{y,x} &= (a)(x^b)(e^{-s \log y \cdot \log x} - 1.) \\
 2s_{y,x} &= (a)(x^b)(e^{2s \log y \cdot \log x} - 1.)
 \end{aligned}$$

and similarly $-2s_{y,x}$, $3s_{y,x}$, and $-3s_{y,x}$.

(and for the three not presented here but are characterized least ambiguously in ad hoc ways.

Only Index 1 includes a group of Soviet bloc nations, and the results of these are inconclusive. While none are among nations showing the greatest positive deviations, they are scattered across the rest of the scale. Yugoslavia, and to a lesser extent, Hungary, Bulgaria and Poland show positive deviations, while Czechoslovakia and Rumania show negative, with the Soviet Union showing a substantial negative deviation. Third world nations (with the remarkable exception of Taiwan at the top of Index 2 and of the three confirming indices not shown in Table 2 as well, and of North Korea and China at the very bottom where they are included) also tend to fall almost uniformly within the more moderate range ($\pm 1s_{y,x}$).

It is likely that at least some residuals shown in Table 2 may reflect problems with the publications index, either by measurement error in its use or in the nature of the index itself as a measure of scientific activity. We have grounds for supposing

that the former has occurred, for example, when we consider the sequence of Asian nations. On Index 2, for example, these stand in descending order: Taiwan, Japan, South Korea, India, Thailand, Indonesia, Philippines, Pakistan and Turkey; and this, with China and North Korea at the very bottom on Index 1 where they were included, suggests that we may have obtained a better index of the ease of communication between the U.S. Library of Congress and the Asian nations than of scientific and technical publishing in those nations.

To the extent that scientific reporting crosses international boundaries, the publications index itself becomes a less accurate index of scientific activity. But the degree to which this loss of accuracy introduces systematic bias and in what directions is almost impossible to assess. Canada is a clear example of such bias, and there are almost certainly others. Canadian nationalists claim that foreign, and particularly American control of their economy results in a low level of research and development within her borders. Findings here would tend to support at least the belief that the

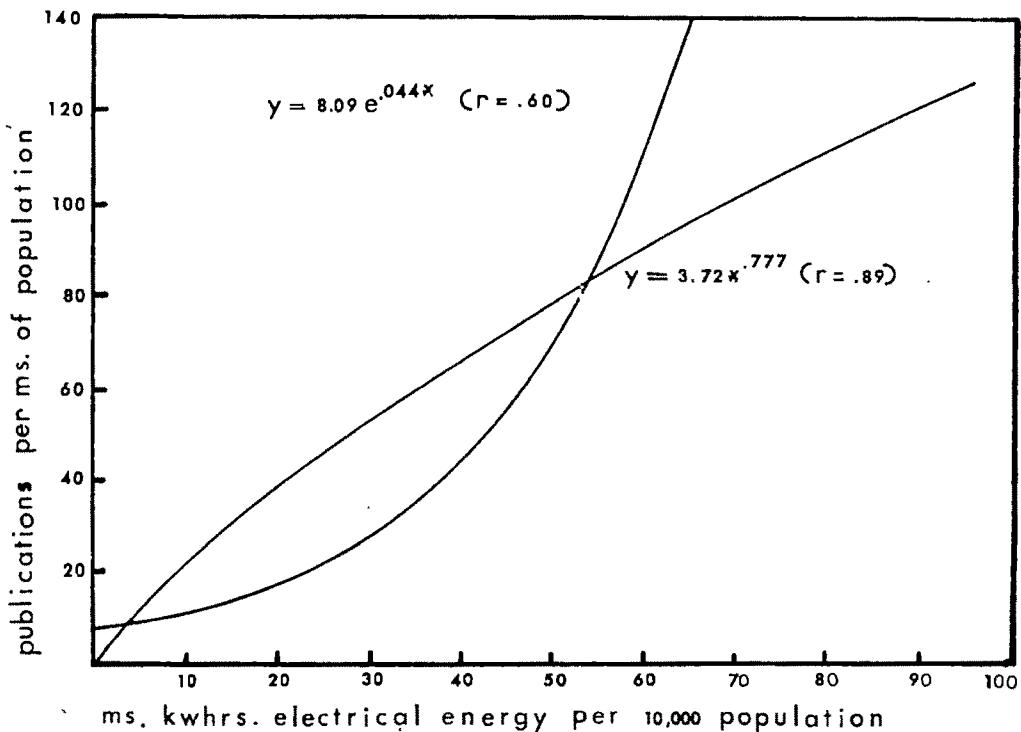


FIG. 3. FITTED EXPONENTIAL & POWER FUNCTIONS - INDEX ONE

level of research and development in Canada is low. However, the practise of Canadian scientists and scholars of publishing widely in American journals suggests that while the level of research may be low in Canada, it is unlikely to be as low as her position in Table 2 would indicate. The number of journals published in Canada is a biased index of research and development here.

Among the western nations, those with more authoritarian political systems differ greatly, with Portugal high, Spain in the moderately negative range, and Greece, with her low level of economic development taken into account, abysmally low on Index 2 and two of the other three as well. The major nations of continental western Europe cluster largely in the moderately positive range, but we find the United Kingdom in a moderately negative position with her three great offspring most often ranged below her—Australia still in the moderate range as on Index 2, for example, but Canada, as we have noted, and particularly the U.S. very low. It is hard to say much about the United States, standing alone as it does at the extreme end of the economic indices, raising great expectations. However, among the western nations some general tendencies are clear. Given what we would expect from their level of economic development, it is not in the U.K. nor her English-speaking offspring, the U.S., Canada, and Australia, nor in the other great powers of twentieth-century European history—Spain, Italy, Germany and France, nor even so much in the Scandinavian countries, that science in quantity flourishes. It flourishes rather in Belgium, Austria, and Switzerland. The effect is pronounced for Belgium; with the Hague an international headquarters. In actual numbers, in 1961, this nation produced between two and four times as many scientific and technical serials as would be expected from different measures of her economic development. That this group of small, multiethnic, and except for Austria, polyglot nations of western Europe, on the periphery of twentieth-century international politics, should contribute qualitatively to science might have been expected. That they should contribute disproportionately in quantitative fashion to "normal" science, to use Kuhn's term, is surprising.

This article takes a first step toward a more precise study of comparative scientific productivity. Of the ways according to Blalock (1969:6) of building a more complex model, we employed only one, the use of more complex forms of relationships than simple linearity. If the finding that percentage increases in science vary with those in economic development in linear fashion (with the ratio of the percentage increase in the former to the latter between $1 \pm .25$ approximately) were confirmed by measurements using different indices of scientific activity (and at different points in time), and second, if the deviations from these power functions fell into groups similar to these described here, this model could fruitfully have variables suggested by these groupings added to it. We might then begin to explain variations in scientific productivity unexplained by its available economic base. In this continuing effort to specify the full conditions under which science flourishes would a scientific sociology of science in part consist.

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ISSUES VERSUS CONTROVERSIES: SUBSTANTIVE AND STATISTICAL SIGNIFICANCE

K. W. TAYLOR

University of Manitoba

JAMES FRIDERES

University of Calgary

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Controversies over the value of statistical significance tests in assessing substantive significance in basic sociological research have persisted because the methodological issues involved have not been clarified. The issues are (1) the meanings or definitions assigned to the terms statistical and substantive significance; (2) the nature of relations between the definitions, and (3) the grounds for choosing definitions and their relations. An argument is made for: (a) defining substantive and statistical significance independently of each other and (b) forming methodological propositions relating these definitions in which statistical significance tests play no role in assessing substantive significance. The concept of methodological significance is introduced as a basis for choosing between alternative definitions and methodological propositions.

INTRODUCTION

WHILE controversies abound in recent sociological writing on the role of statistical significance tests in assessing substantive significance (Gold, 1969; Reynolds, 1969; Morrison and Henkel, 1969; Winch and Campbell, 1969), the issues remain as confused as they were when Selvin (1957) introduced the question. All the above writers agreed that substantive significance is not statistical significance, and that it is a mistake to confuse the two. They part company in their advice on using statistical significance tests. Gold, for example, suggests their use for all research designs; others claim that in survey research, statistical significance tests are not only unnecessary but misleading. We con-

tend that the debate has persisted because the manner in which it has been carried on precludes resolution. Three fundamental problems are evident. First, none of the disputants has explicitly defined what he means by substantive significance. While the term statistical significance is also not explicitly defined, the meaning assigned is always clear. It refers to a particular kind of testing procedure: conventional null hypothesis statistical significance tests. The second problem, then, is that no definition of statistical significance independent of this particular testing procedure is ever offered. Third, while various proposals for desirable relations between substantive and statistical significance are offered, the grounds for choosing definitions and relations are not articulated. If the foregoing

analysis is correct, any attempt at resolving the controversies, if not the issues, requires (1) explicit, general definitions of substantive and statistical significance independent of each other (at least initially), (2) the construction of some methodological propositions postulating desired relations between these terms, and (3) the construction of a set of criteria for choosing among competing alternative definitions and propositions.

Substantive Significance

We see theory as the substance of the empirical scientific discipline of sociology, but it is empirical data which may or may not have the attribute of substantive significance. Our basic position is that empirical data have substantive significance if they allow certain decisions to be made objectively about alternative theories.

Most generally, an empirical scientific theory is a set of explanatory statements. Specifically:

A *theory* is a set of statements analyzable into a set of interrelated propositions.

A *proposition* is a statement postulating the relation (e.g. direction, strength, causal status) between at least two variable attributes both of which are defined by the same unit of analysis.

A *calculus* is a set of rules (e.g. logical, mathematical) independent of the theory for manipulating propositions to make explicit the relations among all the variables of the theory.

In the definition of theory, the "analyzable into" criterion was designed to avoid issues involving the degree of formality in stating the theory. The "interrelated" criterion, or what Bunge (1967) calls "conceptual connectedness," refers to some minimal degree of "overlap" in the variables that appear in different propositions. The criterion is important because when the degree of overlap is below the minimum, the propositions cannot be operated on with a calculus. Propositions may postulate other kinds of relations between variables besides strength, direction and causal status. These, however, are fundamental to most other relations. Finally, the "defined on the same unit of analysis" criterion in the definition of a proposition avoids the levels of analysis

fallacy, one variety of which is commonly referred to as the "ecological fallacy" (Robinson, 1950).

Alternative Theories

Explanatory goals define alternative theories under consideration. The observed or postulated relations among events to be explained exist independently of sets of statements that explain them. If different theories lead to different predictions about the relations among the events to be explained, then for that explanatory goal, the theories become alternatives. For different explanatory goals, however, the same theories might not be alternatives.

For example, if the explanatory goal were to account for variation in religious behavior, Marxian theory would require investigating the specific historical conditions that lead different groups toward or away from false (religious) consciousness regarding their material and economic conditions. Functional theory of the Durkheim variety would prompt a search for the sentiments most essential to the institutional integration of society expressed and reinforced in religious ritual. If on the other hand one wished to explain the relation between religion and stability of beliefs, both functionalists and Marxists would perhaps agree, as Merton claims, that "religion operates as a mechanism for reinforcing certain sentiments among its believers" (Merton, 1957: 44).

Not all sets of propositional statements can function as alternative theories. A theory which postulated that a set of variables were unrelated—what might be called a "null" theory—cannot function as an alternative theory precisely because its propositions cannot be interrelated. If, for example, it asserted that variables A and B and B and C were unrelated, no logical or mathematical bounds would be placed on the relationship between variables A and C. That is, all directions, all strengths and all causal status relations between variables A and C would be consistent with the first two propositions. Thus, all null theories are arbitrary sets of statements having no relations with other statements in the theory. The minimum condition, then, for calling a

set of statements a theory is that the relations assumed or known to hold among a subset of its variables place bounds on the relations among the other variables that can be deduced or predicted via a calculus and the assumed relations.

Admissible Data

If empirical scientific theories are sets of statements for predicting what a set of observations will look like, evaluating theories involves comparing theoretical predictions with empirical configurations of observations. What characteristics must the observations have? The joint result of empirical data configurations and technical artifacts produced by the technical machinery used to select, record, measure and manipulate the empirical configuration will be referred to as constructed data configurations. All constructed data configurations contain technical artifacts to varying degrees. Admissible data contains only those artifacts which derive from the objectivity requirement—the data produced must allow for subsequent error checks.

A Definition of Substantive Significance

(a) a constructed data configuration does not contain technical artifacts beyond the objectivity requirement, and

(b) the constructed configuration is not an historical artifact, i.e., that reliability is defined for the constructed configuration

then,

a constructed data configuration is substantively significant if

(1) it is relevant to two or more alternative theoretical predictions and

(2) there is no match between the constructed data configuration and any theoretical prediction (i.e., all the theories are incorrect), or

(3) different degrees of matching between the constructed data configuration and the theoretical predictions allow at least a partial ranking of the correctness of the theoretical predictions and thus of the theories from which they were derived.

In the above definition, the "relevant" criterion merely refers to the condition that the researcher must be able to make explicit some "rules of correspondence" (Nortrop, 1948) between each theory and the data.

The definition means that *no decisions*

about the substantive significance of any set of data can be made if predictions from only one theory are at issue, unless the data show the theory to be incorrect. With such a definition, constructing alternative theories and predictions becomes central to the empirical scientific enterprise. We have already argued that null theories cannot serve as alternative theories. We would also argue that a theory that could be constructed at a moment's notice by reversing all the directions of the known or assumed relations between its variables would not qualify as an alternative either. Such a "mirror-opposite" theory is of course only conceivable when the entire set of the theory's basic relations was assumed and no known set of facts was relevant to the assumptions. Of course, we have left the realm of empirical theory with such a case and need not consider the possibility further. Thus, for empirical theories, "mirror-opposite" theories will generally not qualify as alternatives because they will be inconsistent with the variables' known relations.

It should be noted that we have defined substantive significance prior to and independent of statistical significance. We believe that it can also be assessed prior to and independent of statistical significance. Before we define statistical significance, note that the above definition was designed to incorporate the necessary conditions for objectivity with regard to substantive significance decisions. The necessary (but not sufficient) conditions for decisions to be objective are (a) a set of explicit alternative choices, and (b) a criterion or set of criteria for decisions that can be described independently of the alternative choices. In simpler terms these conditions amount to the minimum amount of information in terms of which the grounds for decisions can be described. Obviously, these conditions are only necessary and not sufficient because a subsequent check for objectivity may reveal further alternatives or an error in applying the criteria for choice.

Statistical Inference and Statistical Significance

One problem with the definitions of statistical significance in the literature analyzed

was that they seemed implicitly defined in terms of a particular kind of test of statistical significance—typical null hypothesis tests. What we seek is an explicit, general definition of statistical significance independent (at least initially) of the definition of substantive significance.

Statistical inference is a set of techniques for evaluating alternative decisions in terms of probability models. With an appropriate probability model, alternative decisions can be described in terms of their likelihood of being in error. The appropriate probability models for the present argument are those constructed to describe outcomes of random sampling from enumerated populations.¹ The alternative decisions of interest to the present argument are those concerning the nature of the relations between or among the variables in the enumerated population such that the observed relations in sample constructed data configurations could have been produced.

The fundamental tool of statistical inference is the random sampling distribution which describes all possible outcomes of random samples from enumerated populations (if the probability model used to construct the distribution is appropriate). Sampling distributions can be dichotomized in various ways and thus can be used, together with sample information, to attach probabilities of being incorrect to various decisions about variable relations in the enumerated population.

Usually, relations between variables are described by measures of association which can take on the infinite number of continuous values from 0 through +1 or from -1 through +1. Now, the variable relations in populations can presumably take on the same infinite number of values that the association measure can take on; and (with the occasional exception) sampling distributions can be constructed for each of the infinite number of values the measure of association can take. For which of the possible value sets of measures of association

should a sampling distribution be constructed? One chooses a value by means of an explicit technical or theoretical rationale. A "technical rationale" is based on the research activities used in selecting units of analysis to observe and measure. A theoretical rationale is one derived from a theory through the use of a calculus.

The technique of enumeration (or its approximation) and random sampling from this enumerated population gives the researcher a technical rationale for regarding a sample value of a measure of association as a relatively good or poor estimate (depending on the measure's variance) of the value of the measure of association in the population. If the estimate is good enough (has a small variance for that sample size) the researcher has a rationale for constructing a sampling distribution about that sample value and making inferences about the population value of the sampled statistic. Theoretical rationales will presumably postulate the entire range of directions and strengths that measures of association can take on, and these postulated values can serve as a rationale for constructing a sampling distribution. Of course, the actual construction of a sampling distribution is rarely carried out. In many cases assuming normality allows the researcher to use the normal distribution to describe the sampling distribution of his measures of association.

Given a rationale for choosing a value for constructing a sampling distribution, the subsequent dichotomizing of the sampling distribution allows the researcher to describe alternative decisions about the value of the variable relationships in the enumerated population in terms of their likelihood of being in error. *Statistical significance may be generally defined, then, as the probability that any decision made about a population on the basis of sample data configurations is incorrect.* This definition calls for explicit descriptions of the alternative decisions and the probabilities of their being incorrect. Presumably, the researcher chooses the decisions least likely to be incorrect.

The typical null hypothesis test of significance has not been mentioned because it has not yet been necessary to mention it explicitly. If the researcher has a technical rationale or theoretical grounds for postu-

¹ Analogous arguments can be made for the situation where the test of statistical significance is a test against the random assignment of subjects or objects to conditions rather than against a randomized selection procedure, but this situation is of less import to sociology.

lating a zero-measure of association in a population, then the test becomes merely a particular case of the statistical significance discussed above (but of course, it is not a null hypothesis test when so used). If the researcher has neither a technical nor a theoretical rationale for choosing the null hypothesis test, he must choose it on a priori (non-objective) grounds; and in so doing he is likely to have to make further non-objective decisions. First, if he fails to reject the null hypothesis he will be unable to describe the grounds for that decision, i.e., the probability that that decision is incorrect, if he uses only the sampling distribution for the zero value of the measure of association. This is not the case when he rejects the null hypothesis.

Now suppose the sample value of the measure of association is not zero but some small positive value, and that this value is used to construct a sampling distribution. This is not ordinarily done; but if it were it would become obvious that, using this sampling distribution, the probability of the population value being zero or negative is identical to the probability that the observed sample value or larger value could have been drawn from a population whose value was zero. Thus if one decides to fail to reject the null hypothesis, the decision can be shown objectively to be no better than one based on the observed sample value. The decision, then, can only be made on a priori grounds.

The above arguments obviously do not apply to multivariate techniques such as multiple regression where several independent variables are systematically introduced into an attempted total accounting for a dependent variable. In such a case, the systematic reduction in the variance to be explained at each step serves as a technical rationale for a test against a zero value of the partial association measure.

A Methodological Proposition

Given the above definitions for substantive and statistical significance, we would propose that *substantive significance can be defined and assessed prior to and independent of statistical significance*. In other words, statistical significance is neither nec-

essary nor sufficient for substantive significance. The role of statistical inference techniques is to describe substantive significance decisions in terms of their probability of being in error. Attaching probabilities of error should never lead to reversing a decision that would have been made without the probabilities being known, although they should effect the researcher's degree of confidence in his decisions.

The proposal categorically excludes null hypothesis significance tests for measures of association from playing any role in assessing substantive significance. The definition of statistical significance excludes null hypothesis significance tests also, whenever the null hypothesis, i.e., the zero value of a measure of association, is chosen on a priori grounds. We are tempted to say that null hypothesis statistical significance tests have some value as long as the null hypothesis is rejected. But, since it always can be rejected by manipulating sample size, rejecting alternatives that are only possible a priori is ineffective and less than crucial.

Methodological Significance

Why should the foregoing definitions and proposition be used as methodological guides? They should not unless information with *methodological significance* can be produced to show their superiority to alternatives. Our notion of methodological significance parallels those of substantive and statistical significance and, like them, will be based on the necessary conditions for objectivity, i.e., that the grounds for any decision be made explicit. The conditions are that a set of alternative decisions exist and that criteria of choice exist independent of any alternative.

The necessary conditions for objective choice were incorporated into the definitions of substantive and statistical significance (and this necessitated excluding a priori alternatives in the set of decisions). Empirical data relevant to two or more theories has substantive significance if it allows a choice to be made about which theory is more effective for the given explanation. Probabilities attached to values (or value ranges) of measures of association have statistical significance if they allow decisions

to be made about which value is least likely to be in error.

Given a statement of the goals of science, logical information about the effects of using alternative methodological procedures is methodologically significant if it allows choices of methodological propositions which are least in conflict with the stated scientific goals. For present purposes let us state one goal of science as "objective evaluation of information." We have already stated the necessary conditions for objectivity, and incorporated them into our definitions of substantive and statistical significance. In proposing that substantive significance could be assessed before statistical significance (as defined), we in effect claim that statistical significance is neither necessary nor sufficient for substantive significance, because it can only add precision to the confidence with which a particular substantive significance decision will be held. As long as a priori alternatives are excluded both from alternative theories and alternative population values of measures of association, subordinating the objective statistical significance decision should never conflict with the objective substantive significance decisions.

If the two decision processes produce conflicting results, then the researcher can presumably only make a priori, that is, non-objective, choices about which decision will take priority; and he will have to reassess what substance means, or his procedures. We will now show that in the literature previously cited, either the (inferred) definitions or their proposed relations will require the researcher to make non-objective decisions or to reassess his procedures.

Some Methodological Alternatives

In propositional form, Morrison and Henkel (1969) and Kish (1959) appear to be saying the following:

Morrison and Henkel

1. "The only source of necessary and sufficient criteria for the substantive significance of a particular finding is in the support it gives to specific expectations derived from a theory that provides an explanation." (Morrison and Henkel, 1969:136)

2. "Significance tests . . . are not legitimately used for any purpose other than that

of assessing the sampling error of a statistic designed to describe a particular population on the basis of a probability sample." (Morrison and Henkel, 1969:136)

Kish

1. Research results are substantively important if they are consistent with relationships postulated in substantive scientific theory. However, results may be of substantive interest because of their 'nature' or magnitude.

2. If, in survey research, the researcher is willing to assume that some variables have been randomized, significance tests may be used to separate the effects of these (assumed) randomized variables from the effects of explanatory variables.

3. However, confidence intervals are often more appropriate for reporting research than significance tests.

The above statements fail to mention alternative theories in their discussions of substantive significance and on that ground fail to meet the necessary condition for objectivity in selecting theories by data. Neither could describe the grounds for choosing a particular theory over others. Morrison and Henkel see null hypothesis statistical significance tests as useful in assessing statistical significance despite the facts that the null hypothesis can only be chosen on a priori grounds, the grounds for the "fail to reject the null hypothesis" decision cannot be made explicit, and the tests thus fail to meet the necessary conditions for objectivity. Kish hedges on the value of null hypothesis statistical tests by relegating their use to disproving a priori hypotheses (about randomizing variables unrandomized in survey research), but finally recommends the use of confidence intervals. The confidence interval is a dichotomizing of a sampling distribution to which error probabilities have been attached. As we argued earlier, no a priori alternatives enter into dichotomizing sampling distributions about observed sample values to conflict with objectivity.

Two other positions (Gold, 1969; Winch and Campbell, 1969) can be paraphrased as follows:

Gold

1. The magnitude of an association between variables is a criterion for substantive importance.

2. For an association between variables to be substantively important it is *necessary*,

but not sufficient, that it be significant according to a statistical significance test (with or without enumeration and random sampling).

3. If an association between variables is statistically significant, a sufficient criterion for substantive importance is that its magnitude be assessed with a measure of association and the researcher be able to make "the criterion for the acceptance of the importance of the association reasonably clear." (Gold, 1969:46)

Winch and Campbell

1. The magnitude of covariation in data is a criterion for distinguishing haphazard from systematic arrangements of data.

2. Significance according to a statistical significance test is a necessary criterion for substantive significance of a data arrangement but not a sufficient criterion for substantive significance.

3. The sufficient criterion for substantive significance of a statistically significant data arrangement is that the covariation in the data not be attributable to the artifacts of history, maturation, testing, instrumentation, statistical regression, selection, experimental mortality, selection-maturation interaction, interaction effects of testing, interaction of selection and experimental treatment, reactive effects of experimental arrangements, irrelevant responsiveness of measures and irrelevant replicability of treatments. (Winch and Campbell, 1969:141).

In the Gold and Winch and Campbell positions, the role, if any, played by theory is secondary. They take an abstract empiricist position on substantive significance in which data are seen as the substance of sociology. It follows that only strong relations between variables are important and that null hypothesis tests of statistical significance are not only useful tools for locating such relationships, but significance for such tests are a necessary condition for substantive significance. Perhaps part of the logic of the position is economic. If relationships among variables are of sufficient magnitude, then fairly small (i.e. economical) samples will discover them. If a small sample will not discover them, then, since the researcher has no grounds for investigating any but strong relationships, the fact that statistical significance can be manufactured for all magnitudes except truly zero relationships can be ignored.

Following the methodological prescriptions of Gold and Winch and Campbell will conflict with the necessary conditions for

objectivity in the following way. First, since achieving significance for a null hypothesis test is a function of *both* the strength of variable relationships, and the sample size, if only statistical significance is made a necessary condition for substantive significance without specifying minimum sample sizes, the grounds for claiming statistical significance cannot be made explicit.

Of course, statistical significance is only the necessary and not sufficient condition for substantive significance. Gold does not specify the sufficient condition, he merely requests that the criterion be made explicit. For sufficiency, Winch and Campbell specify that a statistically significant relationship not be a technical artifact. If this prescription is followed identical results that are necessary and sufficient for one study might not be so for the next because of a change in the sample size (or the number of individuals randomly assigned to groups). A way out of the dilemma might be to specify sample sizes in making these decisions. Neither Gold nor Winch and Campbell suggest such a procedure, and it seems difficult to do on any but a priori grounds.

Finally, Winch and Campbell's statement leaves open the possibility that theoretical artifacts, i.e., spurious relations, would be considered substantively significant if they were statistically significant and were not technical artifacts. While such a procedure is foreign to the researcher who views theory as the substance of sociology, it does not contradict the necessary conditions for objectivity where the substance of sociology is seen as data. It is difficult, however, to reconcile such a position with the notion of science as cumulative.

Finally, let us consider a paraphrase of what Reynolds (1969) appears to be saying.

1. An empirical test of a hypothesis has substantive import if it has value in making decisions about what should be done with the deductive system producing the hypotheses. This entails evaluating the full range of alternative statistical hypotheses it is possible to derive from a given theoretical model, *and* evaluating alternative theoretical perspectives, given that (a) studies can be replicated over time and over a multitude of population dimensions, and (b) measurement validity of the variables specified in the theoretical models can be demonstrated.

2. Statistical significance tests play a role in ascertaining the reliability of statistical hypotheses if

- (a) the effects of sample size on significance levels is recognized
- (b) descriptively accurate information about variables can be obtained
- (c) adequate sampling theory is used and
- (d) it is possible to avoid arbitrary selection of "significance levels."

3. "... [T]ests of [statistical] significance can lend tentative assurance of the reliability of statistical hypotheses . . . but this in no adequate substantive sense insures validity." (Reynolds, 1969:302).

The first statement that we attribute to Reynolds seems quite similar to our position though his use of "alternative" is ambiguous. If "different" statistical hypotheses derived from a "given theoretical model" are considered as "alternative" hypotheses, then what are "alternative theoretical perspectives," and how does one recognize them? How are they to be distinguished from "alternative" hypotheses? The problem would disappear if Reynolds used the term "complementary" rather than alternative when referring to different hypotheses derivable from a "given theoretical model." Given a set of propositions and some interrelations between them, all the hypotheses that can be made explicit through using a calculus are complementary in that they complete the "explanatory potential" of the original set of propositions and interrelations.

Apart from all the provisos of the second statement, clearly he advocates using null hypothesis tests of statistical significance. Thus, for the reasons spelled out earlier, his procedures cannot meet the necessary conditions for objectivity. It is difficult, however, to discover just what role Reynolds sees for null hypothesis statistical significance tests from his use of the terms "reliability" and "validity" with what appear to be their psychometric connotations. If by reliability in statement three he means "reject the null hypothesis with a very small probability of being in error" and "substantive . . . validity" means "substantive import," the proposal might mean that statistical significance according to a null hypothesis statistical significance test is necessary for substantive import, but not sufficient. The sufficient condition is presumably

that a test have value in making decisions about what should be done with the deductive system producing the tested hypothesis. To assert that a condition is necessary but not sufficient is to subordinate it to the sufficient condition. This appears to be what Reynolds is arguing, but his position might be that both criteria codetermine substantive import. Whatever the case, in many situations the two criteria will potentially be in conflict. It is easy to imagine results of great value for deciding what should be done with the deductive system, but in Reynolds' sense unreliable. The reverse situation—highly reliable results that have no value in deciding what should be done with the deductive system producing the hypothesis being tested—is harder to imagine if the researcher is careful; but of course, it is possible.

In summary, the methodological alternatives reviewed here fail to meet the necessary conditions for objectivity. They (1) fail to include alternatives in their (apparent) definitions of substantive significance, and/or (2) fail to exclude a priori alternatives from either the substantive or statistical decision processes, and/or (3) fail to subordinate one decision process to the other so as to remove potential contradictions between them.

CONCLUSIONS

If explicit, independent definitions of substantive and statistical significance are constructed to meet the necessary conditions for objectivity and if various methodological propositions for desirable relations between these ideas are evaluated according to information that has methodological significance, the controversies over using statistical significance tests can be resolved. The different positions in the controversy are simply logical outcomes of the definitions chosen and alternatives considered. A central issue remains a viable topic for debate: what shall be regarded as the substance of empirical scientific sociology?

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CUMULATIVE SOCIOLOGICAL KNOWLEDGE *

LEE FREESE

Washington State University

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This essay argues that a dynamic interrelation between theory and research is not sufficient for sociological knowledge to be cumulative. It is further claimed that, in spite of the fact that some sociological knowledge is unquestionably cumulative, no standards for accumulation have been explicated; therefore the standards we actually employ are probably not uniform and consistent; and therefore the cumulative development of sociological knowledge cannot be systematic unless formal standards are developed.

To explicate a concept of cumulative knowledge, an analysis is presented which draws upon elementary ideas from sentential logic and the scientific philosophy of Karl Popper. Five conditions for accumulation are developed from the analysis. These criteria are appropriate for constructing sociological generalizations in a natural language, in a manner that permits their progressive consolidation.

THE PROBLEM

ALMOST four decades have passed since Crane Brinton (1933) first posed the question "Who now reads Spencer?" and suggested that the influence of Spencer's thought had waned. Talcott Parsons (1937), quoting Brinton, posed the same question and answer in *The Structure of Social Action*, but while pronouncing Spencer dead Parsons helped to popularize among American sociologists the ideas of a relatively obscure Frenchman, Emile Durkheim. Today, some thirty five years after Parsons' investigation first appeared, it seems appropriate to ask: Who now reads Durkheim?

* I am indebted to Richard Ogles for his detailed analysis of an earlier draft which disclosed numerous errors and provoked several thoughtful suggestions.

The answer to this question, however, is different. A goodly number of sociologists still read Durkheim, for the very sensible reason that Durkheim had many good ideas. But the fact that we still read Durkheim *qua* Durkheim is a bit remarkable. It has been three-quarters of a century since Durkheim had his good ideas and more than one-third of a century since Parsons trumpeted them. In the intervening period Parsons' horn has not been silent. *The Structure of Social Action* was an attempt to integrate the ideas of Marshall, Pareto, Durkheim, and Weber into a unified theory of action to which Parsons has since attempted to contribute his own formulations so that the end result might be a cumulative body of principles. Parsons has left no doubt that he attempted to build on the work of his predecessors, and his investi-

gations as well as his attitudes indicate his overriding commitment to the development of cumulative sociological knowledge.

Yet we still read Durkheim. We do not study sociological theory as a body of principles regardless of who first formulated them; we study the thought of *Durkheim*, the work of *Weber*, the theories of *Parsons*. I do not take this to mean that Parsons failed in his attempt to develop a cumulative body of sociological principles. Quite the contrary. If anything we must judge that sociological theory is much more cumulative today than it was prior to 1937. If and when parsons' aims are realized, however, we shall probably study a general theory of action, not Durkheim, or Weber, or Parsons. For whatever the concept of cumulative knowledge may be taken to imply it must certainly refer, not to the pontifications of great thinkers, but to a related body of principles that has evolved in the light of reason and evidence and to which a community of investigators has contributed.

The desire that sociological knowledge be cumulative has also been expressed in other terms. Parson's dedication to developing theoretical knowledge and his immense influence on modern sociological thinking has led to the charge that whatever cumulative development he may have been responsible for has been hindered by too much emphasis on reason and too little on evidence. Indeed, when many sociologists confront the question of the cumulative development of sociological knowledge they have in mind a "gap" between theory and research. Whether or not such gap exists, and irrespective of how large it may be, both the perceived and actual cleavage between theory and research have certainly decreased in recent times. Here too there seems to be at least a rough discernible trend in the direction of cumulative development.

Lately a plethora of books attempting to specify just how theory is related to research has appeared. Texts and monographs by Blalock (1969), Dubin (1969), Glaser and Strauss (1967), Greer (1969), Lachenmeyer (1971), Mullins (1971), Reynolds (1971), Stinchcombe (1968), Willer (1967), Zetterberg (1965), and others address such issues as general rules for theory formation and theory evalua-

tion, techniques for constructing different types of theories, and the use of theory in specific research settings. Though these authors represent widely divergent theoretical styles, each seems to share two basic premises: (1) sociological investigations should consist of constructing and testing theories; and (2) theory construction is not the exclusive province of an intellectual elite, but is the proper responsibility of each sociologist when he defines some sociological phenomenon as problematic. The first premise seems to engender little opposition any more. The second premise seems defensible too since at present no one sociological paradigm has shown itself able to govern the myriad of heterogeneous sociological investigations. Therefore—the line of reasoning seems to be—since separating theory from research is a false characterization of scientific inquiry, let each sociologist be his own theoretician. And indeed were we to follow some of the specific proposals of the "theory constructionists," that infamous gap between theory and research might fast become an historical curiosity.

But a union of theory and research is not sufficient to solve the problem of cumulative knowledge. If every empirical research program were clearly the result of some identified theoretical rationale, and if every theory had immediately testable consequences, then the specific theory and data in question might be self-cumulative, but what of their relation to *other* theories and research programs? Even two investigators exploring the same phenomenon have no guarantee that their results will be cumulative for each other simply because each has a theory to explain and predict his data. What if there is no conceptual overlap between their respective theories? The identity of their subject matter may be totally obscured by divergent theoretical orientations or incompatible methodological strategies such that their resulting knowledge claims have little or no ostensible relevance to each other. Theories may be cumulative with the data they generate, but to be cumulative with each other they must somehow be consolidated. Parsons' classic conceptualization of the problem is still relevant:

Compared to the natural sciences the amount of genuine empirical research done in our field is very modest indeed. Even so, it has been fairly substantial. But the most disappointing single thing about it has been the degree to which the results of this work have failed to be cumulative. The limitations of empirical research methods, limitations which are being overcome at a goodly rate, are in part responsible for this fact. But *probably the most crucial factor* has been precisely this lack of an adequate *working* theoretical tradition which is bred into the "bones" of empirical researchers themselves, so that "instinctively" the problems they work on, the hypotheses they frame and test, are such that the results, positive or negative, will have *significance* for a sufficiently generalized and integrated body of knowledge so that the mutual implications of many empirical studies will *play directly into each other* (Parsons, 1954:350; emphasis in the original).

When Parsons uses the phrase "working theoretical tradition," he is not referring to some specific theory which is put to a specific test the results of which have immediate implications for reformulating the theory. He is referring to some logically organized body of principles, which determines the direction of empirical research and subsequent theoretical reformulations for a community of investigators. One does not have a "working theoretical tradition" when every sociologist is, by his empirical research, testing some special theory. One has such a tradition only when the results of many special theories tested by many investigators are related to each other—are additive as it were—with respect to some shared set of more inclusive general principles. The relationship of one set of theoretical knowledge claims to another is what we take here to be the problem of cumulative knowledge.

If, then, accumulation depends on a working theoretical tradition, how do we construct such a tradition? What Parsons really meant by such a tradition was nothing less than a general theory of action, his term for a unified body of principles of human social behavior. If we possessed such a body of verified principles we would have no problem of accumulation. But we do not, and therefore we must consolidate our present knowledge so that such a set of principles can be constructed. Merton (1968) suggested some years ago that we should

develop middle range theories and postpone any grand synthesis. Middle range theories abound in the literature today, and they attest to the cumulative development of our knowledge in the past two decades. But however much middle range knowledge may have accumulated, we must still logically consolidate middle range theories. And when those theories are anchored to provincial theoretical paradigms, the problem becomes even more acute. Law-like principles of social behavior should be able to transcend competing theoretical paradigms.¹ It should

¹ Kuhn's (1962) analysis of scientific revolutions seems to suggest this may not be possible. He argues that a paradigm contains both substantive theoretical assumptions about subject matter as well as methodological assumptions or strategies that determine the knowledge claims we formulate and restrict our discovery capabilities. Scientific knowledge is argued to be inextricably bound, not to a "world view" dictated by its basic epistemological assumptions, but to a "world view" inherent in the specific methodological and theoretical strategies that are adopted. Metaphorically, it is as if a paradigm imposes blinders; our knowledge of nature is determined by which blinders we wear, and only by wearing the same blinders can the scientific community achieve consensus. For a refutation of this view, see Schleffler (1967) who contends that Kuhn's position strips scientific inquiry of objectivity and is in any case unwarranted.

For purposes of the present analysis we grant that methodological strategies may to some extent determine theoretical content and linguistic style, but a substantive theoretical assertion generated from a given paradigm, regardless of its content or linguistic style, should be capable of being cumulative with a theoretical assertion generated by a different paradigm if both refer to the same empirical phenomenon. They should be capable of being cumulative because a phenomenon of nature in any scientific inquiry is assumed to have a permanence independent of our conceptions of it. Its independent character should, in principle, permit observations derived from alternative conceptions to be independently verifiable and falsifiable. If such observations can be so verified and falsified, then the formulations from which they were derived can be compared and a decision between the alternatives can be adopted. The decision which results from comparing the alternative conceptions becomes a question of utility, and this decision indeed determines the language in which we choose to express the discovery and the paradigm within which the knowledge is represented. But to argue *a priori* that critical tests between hypotheses generated by competing paradigms is not possible cannot be logically justified and, like the claim that men will never fly, it is subject to empirical disconfirmation.

be possible to show, for example, how Merton's (1968) middle range theory of reference groups, constructed within a structural-functional paradigm, is cumulative with, say, Cohen's (1962) middle range theory of reference groups, constructed within an experimental-formalist paradigm. Failing a demonstration that they are mutually cumulative it should be possible to show what is necessary to make them so.

That is difficult to do. If we had to explicate the standards we would use to judge whether a particular body of sociological knowledge was cumulative or the standards we would invoke to render it cumulative, we would be hard pressed to identify those standards. The search for a set of necessary and sufficient conditions for cumulative knowledge general enough to apply to any specific body of sociological knowledge, is a search for a phantom. No such conditions exist. The concept of cumulative knowledge has simply not been explicated. Yet without explicit criteria for deciding whether given knowledge claims are cumulative, we are forced to rely on intuitive criteria, and they may not always be consistent. Even if they were consistent, the intuitions of different investigators are rarely isomorphic, so it is not likely we are each using the same criteria. If we are not using the same criteria, then we are not likely to make the same evaluations regarding what has accumulated. If we do not agree on what knowledge has accumulated, then we are not likely to agree on what is yet to be discovered, what is an important problem, what is the next course our collective investigative efforts should take. If, in short, we do not share explicit, consistent standards for accumulating sociological knowledge, then whatever knowledge accumulates is likely to be largely fortuitous. And if the accumulation of sociological knowledge is fortuitous we are dreaming if we think we can construct a theoretical tradition which transcends special theories and competing paradigms.

At present we have accumulated considerable knowledge of some specific sociological phenomena. But if we allow our knowledge to continue to evolve haphazardly, it may not evolve in the direction we desire—that is, in the direction of greater

accumulation. If, however, we undertake a concerted investigation into conditions for cumulative knowledge, perhaps the results of such an inquiry would occasion a more systematic and directed evolution.

Conditions for Cumulative Knowledge

Since most sociological knowledge claims are stated in a natural language, and in order to restrict the scope of the present inquiry, we shall not address the problem of the accumulation of sociological knowledge that is formalized mathematically. We will also exclude the singular observation statements or individual findings of fact that often provide the impetus for sociological generalizations. Developing generalizations, however, is of central concern in sociological analysis. Independent of the question of justifying any generalization we may wish to make—a question we shall not explore—is the equally salient issue of constructing generalizations in a manner that permits their progressive consolidation. The present inquiry focuses on developing conditions for cumulative knowledge to permit the systematic construction of verbal sociological generalizations. By the term “generalization” we mean simply any substantive proposition asserted to be true for some specified domain which contains at least one case that has not been observed.

The discussion that follows, however, should not be construed as an attempt to describe the structure of scientific theory, and we do not assert that a generalization is the same thing as a scientific law. Both the structure of scientific theory and the syntactical expression of experimental laws are continuing subjects of debate within the philosophy of science. Our proposal is geared to the needs of practicing sociologists, not practicing physicists; and therefore it will not contribute to that debate. Physicists may formulate laws. At this point in the development of sociological inquiry we are justifiably pleased if we can formulate law-like assertions. When we do formulate law-like generalizations—as necessary precursors to genuine laws—we may be even more pleased when they are explicitly and demonstrably related to other propositions to form an integrated theory. Such is usually not

the case however. Most sociological theories take the form of discursive analyses from which the author's propositions must be reconstructed. Few can be shown to fit within a logically tight deductive model. Insofar as many of our theoretical propositions stand in relative isolation, we must first determine how they may be consolidated. Until that is accomplished, it is premature to apply the kind of rigid canons for theories and laws that might be imposed by scientific philosophers who in any event give conflicting advice. Therefore, the present analysis aims not to explicate the concepts of theory and law but to elucidate elementary standards for systematically interrelating sociological propositions when they are formulated. Concentrating on this modest initial objective seems a sound strategy: Generalizations must be demonstrably related to each other before they can accumulate to a point where they are capable of being formulated as laws in a manner that would satisfy philosophers of science. Our proposal is adequate only to the objective of initial accumulation and should be regarded as provisional.

Even so, the vast differences in the literary style in which sociological propositions are encased for public display may make our task seem impossible. This literary variety certainly testifies to our creative imagination. But fortunately for purposes of analysis we need not be concerned with the variegated styles of sentence construction in the English language. Instead we may analyze one basic logical form for compounding sentences available to us from the logic of sentential inference (the sentential calculus). By using a few basic concepts, such as "and," "or," "not," and others, the rules of this well known logic state what relations may obtain between statements and what inferences may be drawn from verbally stated premises. Within this propositional calculus, generalizations stated in English sentences may be expressed in the logical form known as the conditional. A conditional statement consists of an antecedent and a consequent and may be summarily described with the convention "X implies Y," sometimes also represented by the familiar "If X, then Y" (Suppes, 1957; Tarski, 1965).

Some of the properties of this logical form will give us a clue to the cumulative properties of verbal knowledge claims. First, it is important to note that a conditional statement in sentential logic may be either true or false. Second, with respect to the consequent, Y, the antecedent, X, may be either: (1) necessary, or (2) sufficient, or (3) necessary and sufficient (Suppes, 1957; Tarski, 1965).

One elementary way by which substantive knowledge can accumulate is through discovering that a generalization with an accepted truth value must be modified in the light of theoretical analysis or empirical evidence which supports an alternative generalization. Assume that G_1 is an empirically confirmed generalization asserting "If X, then Y," and assume that a subsequent analysis or test shows that G_1 is false and an alternative generalization G_2 is correct: "If X, and only if Z, then Y." The antecedent of G_1 has been modified such that Z has been conjoined to X so as to assert Z is necessary to imply Y. G_1 and G_2 are thus inconsistent statements: G_1 asserts that X is sufficient for Y, but G_2 asserts that, in addition to X, at least Z is necessary. If G_2 is known to be empirically true, then the truth value of G_1 is modified from true to false. The point is the modification of the truth value of G_1 in this instance is determined by the truth value of G_2 , a generalization which gives us new information—that Z is at least necessary for Y—and in the process corrects our misconception that X implies Y. It seems reasonable to regard such a progression between G_1 and G_2 as cumulative insofar as G_2 does provide us with new information which for the moment is verified.

Let us now consider the alternate case where G_2 provides us with false information. In this case G_1 is assumed to be false initially but is subsequently discovered to be true; whereas G_2 , at first thought true, is found to be false. In terms of our example this is tantamount to showing that Z is not necessary to imply Y, but that X is sufficient. A little reflection shows that this case, which appears to be different, is in fact identical to the previous case: One knowledge claim becomes false because another claim is verified. We now have, the

information, which we didn't have before, that Z is not necessary, and this enables us to express the relationship between X and Y more parsimoniously. For purposes of scientific inquiry the temporal order in which the inconsistent alternatives are formulated is irrelevant so long as a true knowledge claim can be delineated from a false alternative.²

Unfortunately, sentential logic allows only that a statement may be true or false, but not both simultaneously. There are no other alternatives. Scientific judgments, however, rarely conform to this simple choice, and it is right they should not. Before arguing that G_1 should be rejected for G_2 , it is appropriate to ask: What is the confirmation-status of G_1 ? If G_1 is well established empirically and has clear theoretical utility, then subsequent analysis or evidence which claims G_1 to be false must be evaluated in terms of the relative weight it has compared to arguments supporting the truth of G_1 . If G_2 resulted from an incidental, *post hoc* analysis, then G_1 is not likely to be rejected. On the other hand if G_2 were the result of a "critical test," then the truth value of G_1 is more in doubt. Depending on the persuasiveness of the arguments in favor of G_2 , the confirmation-

status of G_1 as a true or false statement may be indeterminate. The word "indeterminate" of course refers not to a statement's truth value but to the propensity of the relevant scientific community for accepting a statement as true or false. This community is called on to make such judgments of relative confirmation-status when presented with conflicting claims. All too often the appropriate response is to await further analysis or evidence and to declare the truth value of a knowledge claim presently indeterminate, despite the fact that the sentential calculus allows for no such designation.

We have, then, an initial condition by which sociological generalizations may accumulate, based on a judgment and comparison of their truth values in relation to each other.

Condition 1: *Modification of Truth Value.*

A generalization G_1 is cumulative with a generalization G_2 if the empirical confirmation of G_2 modifies the truth value of G_1 from any one of the following values to any one of the following values:

- (1) true
- (2) false
- (3) indeterminate

Condition 1 considers the case of generalizations whose truth values are modified in the light of subsequent discoveries. But knowledge can systematically progress by the addition of new claims which do not necessarily alter the truth values of previous claims. Suppose G_1 asserts the empirically verified claim "If X, then Y." Suppose also that subsequent analysis or evidence supports a different claim, G_2 : "If Z, then Y." The truth value of G_1 does not change if G_2 is empirically confirmed, for G_2 does not question the sufficiency of X for Y. The confirmation of G_2 merely shows that X is not *necessary* to imply Y, thereby confirming our original belief that it is sufficient only. It is often the practice to formulate antecedents as sufficient when we do not know whether they are necessary. The question of necessity can be left to

² This discussion seems to imply that knowledge claims found to be false are nevertheless cumulative if replaced by alternatives that are found to be true. The scientific community is of course nowhere very interested in accumulating false knowledge (if such is possible). Nevertheless in the history of science a good many knowledge claims survive despite the fact that they are regarded as false, primarily because they were catalysts to true discoveries. Thus we accept the theory of natural selection and reject the hypothesis of the inheritance of acquired characteristics. This hypothesis was significant, however, in the theories of evolution that preceded Darwin. To the extent that Darwin's discoveries were spurred by Lamarckian conceptions of evolution, it is not outlandish to regard these two theories as cumulative. Obviously, false knowledge claims lose their cumulative value in the long run as they are replaced by systematic knowledge claims believed to be true. In the short run, however, they may be significant pragmatically in determining the problems we select for inquiry and the solutions we propose. If those solutions turn out to be correct, it is often because we learn that the solutions we began with were incorrect. Therefore the role of false knowledge claims in the accumulation of knowledge cannot be disregarded.

subsequent analysis or evidence. If, in this example, X were subsequently discovered also to be necessary, then G_2 would have to read: "If X , and only if X , then Y ." Either way our knowledge of the relationship between X and Y is advanced by learning, first if it is true that X is sufficient for Y and, second if it is true that X is necessary. According to what we learn the antecedent of the conditional statement may be modified. In some cases this will modify truth values, in some cases not. We wish to count as cumulative those generalizations whose truth values are not modified when they are reconstructed, and hence in defining a cumulative property of verbal propositions it seems reasonable to suggest:

Condition 2: *Modification of the "Antecedent-Value."*³

A generalization G_1 is cumulative with a generalization G_2 if the empirical con-

firmation of G_2 modifies the antecedent of G_1 from any one of the following values to any one of the following values:

- (1) necessary, but not sufficient
- (2) sufficient, but not necessary
- (3) necessary and sufficient
- (4) sufficient, with necessity indeterminate
- (5) necessary, with sufficiency indeterminate

Any two alternative generalizations are cumulative, then, if one forces us to revise our judgment of the truth value of the other or if one forces us to reformulate the other. But generalizations may be cumulative in ways that do not involve their reformulation or a change in their confirmation-status. In particular, when we construct an explanation of a sociological phenomenon the components of that explanation may be said to be cumulative.

Consider the following empirical generalization: "Ghetto blacks have fewer employment opportunities than suburban whites." Assume this generalization is true and that as sociologists we would like to explain it. To explain an empirical generalization I take it to mean we must derive it from a set of specific initial conditions and one or more lawlike general principles of which the generalization to be derived can be shown to be an instance.⁴ It is convenient

³ Students of sentential logic will recognize I have taken some liberty with the standard interpretation of a conditional statement. Strictly speaking, it does not allow for the alternatives "sufficient, with necessity indeterminate" and "necessary, with sufficiency indeterminate." Necessary and sufficient antecedents are not "values" of a conditional statement either in the sense that truth and falsity are. However, in scientific inquiry—which is but one application of sentential logic—we often treat necessity and sufficiency as if they were values when we formulate knowledge claims. Often we do not know whether something is necessary or sufficient or both to imply something else yet we must nevertheless make some kind of a conjecture. Thus frequently the state of our knowledge is most accurately represented by the descriptions "sufficient, with necessity indeterminate" or "necessary, with sufficiency indeterminate," though sentential logic does not provide for such interpretations. Neither does it allow for the concept of relative confirmation-status. Nevertheless we are forced to employ such a concept in lieu of an exact and final determination of truth values. It is well known, for example, that the truth of the statement "All swans are white" cannot be absolutely determined since all swans cannot be observed. Because we nevertheless wish to verify universal generalizations, and because a two-valued logic is inadequate for such an objective we employ the auxiliary concept of confirmation-status. Similarly in scientific inquiry the necessity or sufficiency of an antecedent for a consequent may be unknown at one point in time and known at another. It seems desirable to allow that such a progression of knowledge be cumulative.

Some readers may wonder why the alternatives

"neither necessary nor sufficient" and "necessity and sufficiency indeterminate" are excluded as possible antecedent-values. The reason is that it is difficult to argue that such statements constitute knowledge claims, at least as conditional statements. If X is neither necessary nor sufficient for Y , that is equivalent to the absence of an implication between X and Y , that is, the absence of a conditionally formulated statement (Suppes, 1957; Tarski, 1965). If both the necessity and sufficiency of an antecedent for a consequent are indeterminate, that is a simple statement of ignorance, not of knowledge. For knowledge claims to accumulate in the form of conditional statements, the antecedent must be asserted to be *at least* necessary or *at least* sufficient or necessary *and* sufficient.

⁴ The concept of explanation as deduction is as old as Aristotle; and although other models for explanation have been proffered, deduction is the most widely applied model in scientific inquiry (Nagel, 1961). Though the utility of the deductive model for sociological explanation may arouse

to refer to such general principles as theoretical generalizations.

The following theoretical generalization can be used to explain differences in employment opportunities between ghetto blacks and suburban whites: "Persons or groups of relatively low status have fewer opportunities to earn socially distributed rewards than persons or groups of relatively high status." Contemporary ghetto blacks are only one of many groups having relatively low status. Being employed is only one method among many by which social systems distribute rewards. Our theoretical generalization, then, applies to our empirical generalization as well as to many other empirical generalizations. Thus if we are given the specific initial conditions that contemporary ghetto blacks have *in fact* a lower status than suburban whites and that the opportunity to work is a socially distributed reward, then we can use those initial conditions in conjunction with our theoretical generalization to logically deduce the empirical generalization that ghetto blacks have fewer employment opportunities than suburban whites. We have thereby explained the empirical generalization by showing how its referent is an instance of a more general phenomenon described by the theoretical generalization.

The foregoing of course is not proposed as a serious sociological explanation, and it is not intended to suggest that deductive sociological explanations can be so simple or superficial. Its only purpose is to illustrate, in a deliberately skeletal fashion, the subsumption of one proposition under a more inclusive proposition. The point is if we can thus derive one generalization from others (whether it be empirical or theoretical) is it not reasonable to argue that generalizations so related are cumulative? The empirical generalization in our example is cumulative *under* the theoretical generalization because it has been explained by the theoretical generalization; the theoretical

generalization is cumulative *over* the empirical generalization because it has explained it. Any substantive generalization within a deductive chain for which there is at least a modicum of empirical confirmation will be said to be cumulative within the chain. Since deduction is asymmetrical, however, it is important to distinguish between a premise and a derivation.

Condition 3: *Premise in a Deductive Chain.*

A generalization G_1 is cumulative over a generalization G_2 if G_1 is an empirically confirmed premise in a deductive chain in which G_2 is a derivation.

Condition 4: *Derivation in a Deductive Chain.*

A generalization G_1 is cumulative under a generalization G_2 if G_1 is an empirically confirmed derivation from a deductive chain in which G_2 is a premise.

Conditions 3 and 4 do not discriminate whether the generalizations in the deductive chain are empirical or theoretical. Either type of generalization in any combination will satisfy the conditions except one: a theoretical generalization can hardly be said to be derived from (though it might be inspired by) an empirical generalization. There are important reasons for this which we must now examine.

Though we often discriminate, as I have here, between empirical and theoretical generalizations, the implications of this distinction for our investigations are profound and not always recognized. The line of demarcation is provided by a concept emphasized in modern times in the scientific philosophy of Karl Popper (1964) and recently analyzed in the sociological literature in a provocative essay by Willer and Webster (1970). It will be instructive to develop this concept first with an example.

One of the most famous empirical generalizations in a generation of sociology has been Gunnar Myrdal's assertion about the American Dilemma. He claimed that a gaping inconsistency was apparent in the American ideology of equal opportunity and

suspicion in some quarters it does not, as Hempel (1965) has shown, preclude hypotheses of the self-regulation of social systems, the hallmark of structural-functional theories. It is also worth remarking that, according to Hempel (as well as other philosophers), causal explanations are a special case of deductive-nomological explanation.

the discriminatory treatment of Negro Americans by white Americans (Myrdal, 1944). When this generalization was first stated in 1944 its truth value was not doubted by any fair-minded sociologist. What of its truth value in 1972? No one would argue that the generalization is false, but it certainly does not describe reality as accurately as it did in 1944. Reality has changed considerably since then. In the intervening twenty-eight years blacks have become increasingly discontented with the discrepancy Myrdal wrote of, and the decade of the 1960's in particular produced an upheaval that has resulted in considerable change in both the behavior and attitudes of whites. To be sure, the American dilemma still persists, but to a much lesser degree. If current trends continue, by the turn of the century the discrepancy between our values and our interracial behavior may disappear altogether, in which case Myrdal's generalization will be false and the American dilemma will be of historical interest only.

But the fact that Myrdal will ultimately be proved wrong is no reflection on him. He was right for a while. But he could not possibly have been right in the long run. He stated an empirical generalization, and it is in the nature of any empirical generalization that, sooner or later, it will become false. It will become false because the empirical conditions it refers to will change. Any generalization consisting of concepts that refer to specific phenomena in particular societies or historical periods is valid so long as those events are not transformed by processes of social change or the sheer passage of time. And of course, though the rate may vary from one society to another, all social facts, even if recurrent and complex, are constantly changing. Knowledge claims with specific spatio-temporal referents then, are not likely to be long-lived.

Theoretical generalizations, however, are more stable commodities. A theoretical generalization stating a relationship between status levels and opportunities for rewards is an example of such a knowledge claim. It will not always be true in all cultures nor will it always be true in the same society, because the necessary conditions for its application may not always be real-

ized. If those conditions are not realized in a given social system it is neither true nor false for that system. But when the conditions are realized—in the past, present, or future, in any social system—the knowledge claim may validly be applied to explain the particular, unique circumstances of the situation. Its truth or falsity depends on its *consistency* with facts it is intended to explain, not with whether or not such facts are permanent, recurrent, universal, or even presently identifiable. Facts which it explains today may be gone tomorrow, but the principle remains valid for explanatory purposes whenever a set of facts emerges for which the generalization is relevant. Unlike empirical generalizations, theoretical generalizations do not have to be continually modified to account for the ever-changing facts but only for the inconsistent and contrary facts that might be observed when they are tested.

Empirical generalizations, then, depend on time and space for their interpretation in that the concepts of which the generalizations consist refer to social action in particular cultures, societies, or historical periods. Theoretical generalizations, however, consist of concepts whose meanings are independent of time and space; that is, they refer to social action in no particular society, culture, or historical period but apply to any social system within which the abstract conditions of the generalization are realized.

In our previous conditions for cumulative knowledge we made no reference to whether cumulative generalizations had to be empirical or theoretical. We should like both to be cumulative, of course. But how can empirical generalizations be cumulative unless they are linked to theoretical generalizations or general principles having a law-like character? Empirical conditions change, and as they change so do the truth values of empirical generalizations. In Western industrial societies—to which most existing sociological generalizations are restricted—the rate of change is rather rapid. Statements true today are false tomorrow sometimes before one can record them in a textbook. Knowledge can hardly accumulate if it disappears as fast as it is discovered. What sense does it make to say that one generali-

zation was cumulative with another during the 1940's but both are now false and irrelevant because the truth of each depended on cultural conditions which have long since ceased to exist? Cumulative sociological knowledge cannot be conceived of as the multiplication of interesting but entirely historical facts. Yet empirical generalizations provide us with no more than a history of contemporary society. Because we live in contemporary society such knowledge has momentous import. For it to have cumulative import requires general principles for its interpretation which are valid even when contemporary societies cease to exist.

Put it another way. Scientific knowledge cannot be cumulative today and gone tomorrow. Therefore, the only way empirical sociological generalizations can be cumulative is if they are related to a generalization whose truth or falsity is not tied to specific cultures and historical periods. Because theoretical generalizations have meaning independent of any spatio-temporal reference, we stipulate as a necessary condition for cumulative knowledge that a theoretical generalization be part of any cumulative chain.

Condition 5: *Space-Time Independence.*

A generalization G_1 is cumulative with, under, or over a generalization G_2 only if either G_1 or G_2 is a theoretical generalization.

We have, then, five conditions for cumulative knowledge, one of which—Condition 5—is asserted to be necessary. If Condition 5 is satisfied, and if any one of the other conditions is satisfied, then a generalization is cumulative; otherwise it is not. But, it should be clear, these conditions permit no generalization to be cumulative in isolation. A generalization is cumulative only within a system of knowledge claims structured deductively or structured to permit a systematic and critical comparison of alternatives.

Within such a context it is reasonable to think of any given generalization as being more or less cumulative according to how many of the five accumulation conditions are satisfied. A generalization that is at once theoretical, a premise, a derivation, and a

modification of an earlier alternative knowledge claim stands at the center of an interlocking chain, well established in a network of knowledge claims. We need not ask what are its implications and its explanation need not give us pause. Its evolution and role in a chain of knowledge claims can be traced and its significance assessed. The same may be said for an entire body of knowledge: To the extent that its component knowledge claims are cumulative, the entire system of knowledge may be said to be cumulative. To the extent that a system of knowledge is cumulative, the relevance of new discoveries can be immediately apprehended and their role in that body of knowledge can be fixed. We may speculate that perhaps only within such a system can a genuine discovery occur, for it is difficult to attach significance and meaning to a newly developed proposition or finding which has no anchor.

CONCLUSIONS

The number of current sociological propositions and findings which have no anchor is, to say the least, professionally embarrassing. The relatively recent emphasis in sociology on theory formation and theory testing is a necessary first step toward anchoring our knowledge, but it is hardly sufficient. Whereas in the past we may have developed theory and research independently of each other, will we be better off in the future if we develop *tested* theories independently of each other? I think not, unless they have a common theoretical denominator which crystallizes their mutual relevance.

That is why Parsons' (1954) call for a working theoretical tradition is still relevant. He knew full well that a perfect marriage between theory and research was no guarantee that the end result would be cumulative knowledge. Only if theory and its research were anchored in a common, progressive tradition defined by a set of shared general principles could the end result hope to be cumulative. We may stumble across a working theoretical tradition accidentally, but I think our progress toward that goal would be facilitated if we had a better idea of how to get there. Parsons seemed to leave the impression that getting there depended on choosing research prob-

lems instinctively on the basis of something bred into the bones of researchers. All that really meant, of course, was that his standards for assessing our progress were primarily intuitive.

No one of the standards developed in the present analysis is novel, but none is intuitive either. The combination of these criteria explicitly applied at decision stages in the investigative process should increase the probability that any given investigation, empirical or theoretical or both, will eventually become part of an integrated system of knowledge, a working theoretical tradition. The probability should be increased because such a tradition requires that knowledge be cumulative. For a body of knowledge to accumulate its component knowledge claims must be related. For knowledge claims (or statements of any kind) to be related they must adhere to the rules of a logic appropriate to those kinds of statements. Since the rules of the logic define what relations are permissible, and what conclusions can legitimately be drawn, failure to adhere to them—or *some* coherent calculus—results in non-sensical or unrelated statements and conclusions.

We shall not have a working theoretical tradition, then, without applying formal rules for systematizing verbal propositions since most sociological knowledge is currently expressed in that form. Application of even the simple ideas proposed here, however, suggests that despite our continuing advances the unity of sociological science is an issue we might profitably address. If sociological knowledge were a puzzle we would have to lean pretty hard on some of the pieces to make them fit. That is why we still read Durkheim. We do not really know how the entire puzzle fits together, so at first we concentrate on the biggest pieces. Until we do learn how to fit that puzzle together—naturally, without undue influence—we shall forever be reading Durkheim.

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COMMENTS

WHY SOCIOLOGICAL KNOWLEDGE IS NOT CUMULATIVE: A REPLY TO PROFESSOR FREESE

Scientific knowledge consists of a combination of empirical and theoretical knowledge. The former is the result of research or the collection of sense data, while the latter is concerned with the mental relations of non-observable constructs (Frank, 1957; Toulmin, 1953; J. Willer, 1971). Cumulation in science therefore may be either empirical or theoretical. Empirical cumulation is much in evidence in sociological literature, but the question of theoretical cumulation is problematic. Professor Freese has maintained that the gap between theory and research has narrowed significantly recently. But, a narrowed gap is, scientifically speaking, a cause for concern rather than satisfaction, for a gap between theory and research is *necessary* in science. Empirical knowledge cannot be added up to reach the level of theoretical knowledge if they are cognitively different.

Facts do not add up to constructs or establish relations between them. Facts get their meaning from our powers of observation, but constructs get their meaning from their mentally established relations to each other. Relations between constructs, however, may be used to explain observed relations between facts. Cumulation, therefore, has a different meaning for each of the two types of knowledge necessary for the development of science. Because the relation of theory to research is isomorphic rather than logically dependent (Toulmin, 1953; D. Willer, 1967), the cumulation of one may progress independently of the other; however, only when empirical and theoretical knowledge are used conjointly (establishing an abstractive, isomorphic connection) can we claim to have produced a scientific explanation.

If science is a knowledge system in which facts are explained in terms of nonfactual constructs, Professor Freese's proposal that we attempt to arrive at some sociological scientific principles through establishing truth values for propositions may be seriously questioned. Philosophers of science do not always agree with Popper's notion that scientific laws are established by falsification (see Ayer, 1946; Kuhn, 1962; and Toulmin, 1953). Indeed, if

scientific laws are universal relational statements, it is logically impossible to establish their truth or falsity empirically.

We cannot logically or inductively arrive at true (or false) universal generalizations because we cannot observe a potentially infinite universe (or even a finite universe larger than we can observe) to determine the truth value of those unobserved instances of the phenomenon in question. The observation of one contrary case, however, is sufficient to falsify an observation. Even "probable" generalizations raise the same problem: we cannot know that the proportion of *a* to *b* is universally precisely (or even roughly) two-thirds. Far from being able to state that "All A's are B's," we cannot even state with certainty that most A's are B's."

If the truth of theoretical generalizations cannot be established by induction, perhaps they can be constructed to be formally true, i.e., true by definition. In such a case "All A's are B's" because B defines the meaning of A. Such statements are termed "analytic" as opposed to "synthetic" observed factual statements. They are true independent of experience, and therefore, experience does not contribute to their truth or falsity (Ayer, 1946). If the meaning of these theoretical generalizations is independent of experience, it is consequently impossible to deductively explain empirical generalizations by them. If "Persons or groups of relatively low status" means having "fewer opportunities to earn socially distributed rewards," then we can deduce nothing about an observed empirical category, "ghetto Blacks," from that definition. The more general theoretical generalization is analytic while the less general empirical generalization is synthetic, meaning that no deduction can be made. Russell, in fact defined a synthetic statement as "any proposition which is not . . . deducible from any proposition of mathematics or deductive logic" (1948: 497).

The "law-like general principles" to which Professor Freese refers have been determined to be both inductively and deductively impossible. If they are empirically derived by induction, they cannot be true or universal; and if they are definitionally constructed, no empirical deductions can be made to observed facts for explanatory purposes. Unfortunately,

because of the popularity among some social scientists of the writings of such philosophers of science as Popper and Hempel, books on theory construction in sociology often compound this error (see Glaser and Strauss, 1967; Greer, 1969; Kaplan, 1964; and Reynolds, 1971).

If generalizations can neither be established by facts nor deductively explain facts, it is not surprising that scientific theories take neither of these forms. So-called "law-like statements" are not like laws at all. They are clearly unlike the mathematically stated physical laws which Professor Freese summarily dismisses in his paper, but they also cannot be claimed to be like the yet to be formulated "genuine" sociological laws which he thinks will follow from these law-like assertions. Perhaps the problem of theoretical cumulation is a direct result of the fact that we attempt to state our theories in "natural language."

An analysis of why the methods of a field fail to produce cumulative theoretical knowledge cannot be expected to succeed if carried out from the viewpoint of those same methods. To be effective, such an analysis must be open to the possibility that it is not simply the way that the methods are used that is at fault (after all, much sociological research is carefully carried out by conscientious, intelligent individuals) but that the problem may lie in the logic of the methods themselves. In other words, the initial consideration is whether the knowledge generated by these methods has the characteristics of scientific knowledge. If not, it cannot be expected to be cumulative.

The use of abstractively interpreted rational mathematical systems is what gives theory its cumulative nature. A rational system of concepts because it deals with experience only through abstraction (i.e., only indirectly through dealing with models isomorphic to experience), cannot be generalized from observations. Consequently theories are not limited in application to denumerable set of observed cases; but, as Einstein pointed out (1950), are instead invented and therefore subject to rigorous empirical interpretation through their measures. Mathematical exactness characterizes theories simply because they are invented; the observed world is not mathematical or exact. In order to treat it as exact we must consequently subject it to some measurement process which enables us to treat it as if it were characterized by units, by mathematical exactness.

Cumulation of theoretical knowledge may consequently mean: (1) that the theory had been elaborated by rational mathematical ex-

tension (mentally extending it to embrace a new concept or new concepts); (2) that a new theory had been invented which either supplemented or displaced an existing theory by being more inclusive (by being more applicable to a broader scope of observed phenomena). The fact that neither of these events occurs in sociology is obviously a consequence of the fact that we have no theory in the sense of rational mathematical systems of concepts which can be abstractively interpreted for explaining individual events. Most empirical methodologists, in fact, agree with this viewpoint, at least rhetorically, but feel that a rational mathematical system of concepts and their derived means of measurement will be the eventual result of the methods presently in use. (Some instead believe that we can never achieve such exactness because social phenomena are too complex.) Measurement is expected to grow out of the refinement of scaling, while exact laws follow from the careful refinement of empirical generalizations. Cumulative science is expected to be a consequence of such procedures.

Measurement cannot result from refining scaling procedures because scaling procedures are not measurement procedures. In fact, they are not even unrefined measurement procedures; they are logically different. The characteristics of ordering observation by measurement (precedence, transitivity, etc.) are theoretically determined and thus independent of particular observations. A yardstick can be used to interpret the theoretical idea of extension and, when so used, establishes a numerical order which can be attributed to the phenomena to which that idea is being applied. Measurement begins by assuming a rational system of numbers (a mental construct) and proceeds by imposing that system on empirical objects. The result of a measurement therefore is not a quality of the measured phenomenon but of the numbers system. A numerical value is *imposed* on the phenomenon by the measurement process. Scaling procedures, on the other hand, do not order data but are ways of summarizing observations by which observable qualities of phenomena are assigned non-numerical but partially ordered values. The "order" of those values is called a "scale." Since the order is entirely dependent upon the unique set of observations which were made, then each set of observations will provide a unique scale. Instead of *ordering observations* and attributing a numerical quality to them (a quality which is not a characteristic of the observed phenomena), as in measurement, scales are *ordered* by observed qualities of phenomena and thus are logically the opposite of measures. It is evident, then,

that no amount of refinement can transform scales to measures.

The fact that our data collection procedures utilize scaling techniques rather than the metric-type measurement of the cumulative sciences has been attributed to the belief that our science is in its early stages. Contrary to the claims of S. S. Stevens (1959), measurement is not just refined scaling, but is logically different; and this difference is crucial to the development of cumulative science. Scaling procedures cannot be refined to give exact measurement because no matter how carefully we observe natural phenomena, we cannot derive numbers from them. Numbers are not part of nature. The unit, *one*, the basis of the rational mathematical system clearly has no empirical existence. This number was not achieved through the refinement of "natural" inexact numbers (such as $\frac{3}{4}$, $\frac{1}{2}$, 2 and $1\frac{1}{2}$) into a unit according to the logic of the procedure used in scaling in order to devise exact measurement. (In fact, in order to express a meaning for other numbers we must have an idea of "one.") Measurement in the exact cumulative sciences begins with the assumption of a rational mathematical system of numbers and forms its measures by imposing that system on some empirical object which is then used as a standard. Measurement, therefore, consists of treating some observable as if it were a unit. Scaling, however, assumes that observables are units (or at least have units as qualities). It cannot, therefore, get beyond classification.

Just as measures do not arise out of the observation of order in the world, theory does not emerge from the careful observation of relationships in our experience. Scientific laws and theories relate nonobservable concepts so that observations may be ordered and made comprehensible through comparison with isomorphic theoretical models. Empirical findings, however, are summary statements for observations. Since all human collectivities are unique, findings which correspond to them are also unique. Because findings are bound to particular populations and have no value for inference in universes of nonlimited scope, they cannot be refined into laws. Nonlimited scope is a result of the rational character of laws as constructs. Findings summarize what *was* true for some observed cases or for the average observed cases and thus cannot be used to refer to as yet unobserved cases. They are nevertheless thought to be statements of relationship whose terms are related probabilistically. Refinement of such statements is supposed to result in absolute connection such as

that found in scientific laws. But since that probability is merely a statement of an observed proportion, it is ordered by unique observations and inseparable from them. If findings are *ordered by data*, they *cannot order them*. They are therefore the logical opposite of laws.

The use of a rationally ordered system of mathematically related concepts is the basis of the cumulative nature of theory. Constructs can be determinatively and exactly related to other constructs through the use of such systems. In other words, mathematics (or other logically possible relational systems) provides a framework by which constructs (nonobservables) can be equated, multiplied, divided, etc. and thus treated in a relational manner not possible for observed phenomena. Observed phenomena which are unique (are not units) can not be multiplied with each other, but they may be explained and simplified for our understanding if they may be abstractively related to systems of constructs (theories) which *can be* mathematically treated. Constructs which are theoretically related are thus elements of cumulative mathematical systems. This cumulative nature is a result of the ability to integrate new constructs (or alter old ones) to extend explanatory power.

A collection of observations, a set of findings, by itself is no more than an accumulation of individual pieces of knowledge characterized by phenomena-determined uniqueness which prevents them from achieving the mathematical simplicity of cumulative theoretical knowledge. Such facts can be *explained* by scientific laws, but they cannot be the basis of laws.

DAVID WILLER
University of Kansas
and
JUDITH WILLER

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CUMULATIVE SOCIOLOGICAL KNOWLEDGE: AN ADDENDUM

The comment by Willer and Willer addresses not only the substance of my paper but also develops some of their own notions about the problem of accumulation. They discuss several issues, such as measurement for example, which my proposal ignored. I shall here confine my remarks mostly to their criticisms of my proposal.

We evidently agree that accumulation is not equivalent to the simple expansion of knowledge but rather depends on its systematic expansion. We evidently disagree on the kinds of systems in which sociological knowledge is capable of expanding. Willer and Willer, if I interpret them correctly, argue that sociological knowledge cannot accumulate if expressed in a natural language. Instead the artificial language systems of mathematics are said to be necessary. I think they are wrong. Accumulation may be less perfect for knowledge expressed in natural languages, but it is not precluded by using those languages.

I take it to be the case that scientific knowledge consists of systems of statements expressed in some language. Otherwise what we claim to know cannot be communicated nor independently evaluated by a relevant public. Now, if we claim to know something, then we are claiming that something is true. But our claim could be wrong. If we are wrong, our so-called knowledge will be unreliable. If we

wish our knowledge claims to generate reliable predictions, then we shall be prudent if we evaluate our claims as right or wrong, as true or false. If we do not take that minimum measure on knowledge claims, then our systems of knowledge can also countenance contradictions. These provide us with no new information and are therefore useless. So if only what is said is what is known, and if what is known may be true or false, then we profit by discriminating what is true from what is not. Our profit consists of reliable information about the world of our experience.

These considerations apply not only to knowledge claims in natural languages but also to any "abstractively interpreted rational mathematical system," because whatever that is, it is certainly a system of statements in one or more languages. Willer and Willer leave the impression that the evaluation of knowledge claims as true or false is irrelevant when those claims are expressed mathematically. Such is hardly the case. The familiar Law of Falling Bodies expresses mathematically a relationship that may be true or false, not analytically but synthetically—when the law is given an empirical interpretation. It is therefore incumbent upon us to adduce evidence in its favor, to judge the conditions for its truth or falsity, lest we send a man to the moon and miss our target. Clearly we do not judge the truth of such a principle by examining all the cases to which it applies, since the number of those cases is infinite. Our limited examination of a finite number of instances gives us, not a final determination of its truth value, but a provisional judgment based on the successful predictions it generates. If contrary evidence appears, we revise our judgment. Since we can never know if it is true but we must nevertheless make a decision, we accept it *as if* it were true until a better alternative is available to supplant it. Having decided to accept one alternative and reject others on the basis of evidence, we say our claim is confirmed and hope for the best; but guarantees have we none. That scientific knowledge claims are statements having truth values, and that working scientists must make provisional judgments about the truth values of alternatives by comparing their predictive success is not new or controversial, it is the name of the game—including the game played mathematically. Such being the case we should not, therefore, be prone to judge as cumulative anything whose truth value we do not also judge. Since confirmation is never final for either verbal or mathematical formulations of laws, one cannot reject only the former because logical systems applicable to natural

languages are mute on the confirmation issue: so is any deductive calculus.

I suggested that lawlike sociological propositions—or generalizations as I called them—may legitimately be expressed in the form of conditional statements. Willer and Willer argue that such statements are not lawlike, which is equivalent to claiming that the logical model for a universal conditional cannot serve as the syntactical expression for scientific laws. Maybe. It depends on what properties you want scientific laws to exhibit. The universal conditional is perfectly capable of reflecting at least one of the properties that Willer and Willer would require of scientific laws. Such a statement may be interpreted to describe a domain of events which is either finite or infinite. It may be interpreted to refer to something which has nothing to do with events or sense data at all. It may be interpreted in the subjunctive mood. It need not necessarily describe anything. It is a form for sentence construction, and it can accommodate the Willers' non-observable constructs quite comfortably. It was my specific suggestion that a theoretical generalization did precisely that: It should consist of concepts that are independent of time and space; in other words, independent of sense data; that is, defined not by facts or observables but by other non-observable constructs and their abstract relations. If a generalization is thus abstract, as well as universal and conditional, it need only have empirical consequences to make it scientifically lawlike.

Willer and Willer evidently feel that the problems of constructing and justifying universal generalizations can be summarily dismissed if we eschew a natural language in favor of, presumably, a language of mathematical functions. But a statement of a mathematical function is itself a universal generalization. It is simply stated in the framework of a different calculus. Because it can be given an empirical interpretation, it too can be evaluated as true or false. Because it is universal it too cannot be conclusively verified nor can it be the product of induction by enumeration. If we therefore adopt functional language as the model for sociological laws, we do not resolve these difficulties which the Professors Willer take to inhibit the accumulation of knowledge.

Why can't sociological knowledge be cumulative in a natural language? It is certainly possible for verbal propositions to adhere within a system of logic like the sentential calculus. This calculus is primitive compared with other systems of logic and mathematics, but so too is much of sociological knowledge. Can we not profit by consolidating what knowl-

edge we have even if the end result is not as impressive, aesthetic and precise as celestial mechanics? I agree that a sophisticated and perfectly cumulative science is a mathematical science because of the greater precision and more powerful deductions that mathematics makes possible. But before we begin describing our ideas in terms of mathematical functions, it might help if we first conceptualize—in a natural language—what those ideas refer to, how they are abstractly related, and what evidence may be brought to bear upon them. Constructing conditional statements whose reference is independent of time and space, comparing the assumed truth values of alternatives, modifying the antecedents of those statements through a comparison of alternatives, and formulating them deductively where possible is a reasonable place to start. The construction of knowledge claims in a natural language does not preclude subsequently formalizing those claims in a mathematical language—unless we are too sloppy with our natural language to begin with. Then of course our knowledge will not be cumulative. But neither will it be cumulative if we employ a mathematical language when we don't have anything to formalize.

LEE FREESE

Washington State University

COMMENT ON GOVE'S EVALUATION OF SOCIETAL REACTION AS AN EXPLANATION OF MENTAL ILLNESS

Gove's article, "Societal Reaction as an Explanation of Mental Illness: An Evaluation," (*ASR*, October, 1970:873-884) is a welcome addition to the growing list of publications which take exception to notions that have emerged from the "labelling" approach to deviance. Gove's analysis argues that the individual's behavior is not irrelevant to the social reaction and labelling and that the deviance-reinforcing impact of formal social control is neither as certain or strong as the labellers seem to think. His analysis of the extent to which the labelling perspective fits the data on societal reaction to "mental illness" is a substantial contribution. Nevertheless, it contains misconceptions and errors which must not go unchallenged.

First, Gove gives evidence from other studies that the usual initial reaction to inappropriate behavior is to deny it. (p. 877) But what Gove fails to admit is that this is one of Scheff's major propositions. The question is not whether others stand ready to pounce on

the first sign of supposedly aberrant behavior; but once applied, does the label stabilize rule-breaking into a deviant role as Scheff hypothesizes. Gove's later data suggest that it does not do so as strongly as Scheff posits. But Gove's data on initial denial are irrelevant to the question, and his discussion misleads by implying that Scheff does not recognize the usual denial.

Second, Gove questions the notion that psychiatric examinations are arbitrary and unsystematic. His conclusion is that "The public officials who perform the major screening role . . . screen out a large portion." (p. 879) Yet Gove's own evidence in his Chart 1 is that only a small minority are judged competent and released through the hearings, not the "large portion" he contends, unless one defines 4.8%, 7.7%, 8.5%, and 35.7% as large portions. (None of this discussion, of course, speaks to the very real civil rights problem of the assembly-line judgements about a "condition" for which one may be forcibly incarcerated.)

Third, Gove's most convincing data show that the stigma of mental hospitalization does not lead to a prolonged career of mental illness, and that it is not nearly the problem for ex-patients that Scheff and others have believed. However, Gove makes the somewhat different point that hospitalization does have a negative, deviance-supporting impact which is lessened by rapid treatment and release. (p. 880) Therefore, the less the hospitalization, the less firmly entrenched in the deviant role one becomes, and hence the less negative the impact. Although I must admit not ever seeing this exact proposition in the labelling literature, it coincides with the societal reaction position. There may be other aspects of the studies which Gove reviews which are supportive, directly or indirectly, of the labelling perspective; and his evaluation is less than balanced by not showing what they are.

Fourth, Gove accepts uncritically the medical analogy of "mental illness" and "psychiatric disturbance" for this form of deviant behavior.

For instance, in outlining the "restitutive processes" of hospitalization, Gove cites evidence that legal and psychiatric ratification of the illness label blunts the negative aspects of hospitalization and helps to restore disrupted family life. (p. 880) Hospitalization may support the family by removing a troublesome member and supporting others' conception of him as ill. But this in no way proves that the disruptive family member is "mentally ill." Indeed, the hospital's restitutive effects may more readily be attributed to the family's redefinition of the member owing to his release

than to the fact that an "illness" as been cured. Again, his contention is that "rehospitalization does not typically occur because of the expectations of others but because of the manifestation of severe psychiatric symptoms which have become impossible to handle in the community." (p. 877) However, Gove presents no evidence that the persons' behavior was disturbed. He shows only that one factor in rehospitalization is the individual's troublesome behavior.

Other statements underline Gove's assumption that persons labelled "mentally ill" are so, in fact. "In sum, the evidence strongly suggests that persons, typically are hospitalized because they have an active psychiatric disturbance which is extremely difficult for themselves and/or others to handle." (p. 877) (See similar statements on p. 879, p. 882, and elsewhere in the article.) Gove's evidence suggests no such thing. He offers no supporting data that the "vast majority" of those labelled mentally ill are so, unless one tautologically makes commitment the criterion for illness.

Instead, the data suggest that people are reluctant to stigmatize those close to them with the mental illness label, especially when it means forcible incarceration, and therefore do so, and seek sanction for doing so, only after the behavior has become too troublesome to handle. To say that the behavior derives from an underlying "active psychiatric disorder" is to offer the standard medical-psychiatric explanation. The scientific inadequacy of this explanation is becoming increasingly apparent. Gove presents no data on the causes of mental illness, psychiatric or otherwise. His analysis suffers from not presenting an adequate alternative view to labelling which might incorporate valid aspects of it.

RONALD AKERS

University of Washington

REPLY TO AKERS

Although Akers found my article a "welcome addition" to the literature and a "substantial contribution," his comments are highly critical; and I welcome this opportunity to reply to them. In doing so I will follow the order of Akers' points.

First, the question is not solely whether labeling stabilizes deviant behavior, but also the nature (or seriousness) of initial deviance. As Lemert (1967:17) indicates, a basic assumption of the societal reaction position is that "primary deviation is assumed to arise in a wide variety of social, cultural and psycho-

logical contexts, and at best has only marginal implications for the psychic structure of the individual" (italics added). As someone who is explicitly in that tradition, Scheff (1966:40 and *passim*) indicates that initial deviance "arises from a fundamentally diverse" and "unlimited number" of sources. Nowhere does he indicate that this initial deviance has serious implications for the individual, and much of his book is spent developing a very ingenious and plausible formulation of how persons without a serious disturbance could easily be channelled (not railroaded) into the mentally ill role. Akers does Scheff a disservice by ignoring the existence of this formulation, a formulation which includes the premise that denial is the typical response to residual rule-breaking. Furthermore, Akers does me a disservice by stating I "fail to admit" that Scheff said denial was the typical response, for I explicitly stated "he [Scheff] notes a general tendency to deny that an act of residual rule-breaking is an indication of mental illness" (Gove 1970a:876). One of the key differences between Scheff and me is that I believe persons tend to deny the implications of behavior indicative of a serious disorder; whereas in Scheff's formulation, denial deals with behavior of little intrinsic significance.

Second. In this section Akers completely misrepresents what I said. On page 878 I explicitly distinguish between the initial screening processes "where the police or some other screening agency make the decision to hold or not to hold the person for examination and possible commitment" and the official psychiatric examination and court hearing. I note that a "large number" of persons are screened out during the initial phase. Regarding the examination and court hearing I state that "if a person passes this initial screening, he will probably be committed and there is reason to believe that the process at this point frequently becomes somewhat ritualized" (Gove 1970a:879).

Third. I do not accept Akers' formulation of what I said; for I distinguish between the stigma of hospitalization, which I see as being largely unrelated to length of hospitalization, and the degree of socialization into a total institution, which I see as being possibly strongly related to length of hospitalization. But let that pass for another matter, namely Akers last sentence in this section. Here Akers states that my "evaluation is less than balanced," not because I have ignored data, but because he *thinks* I may have. Almost ironically some aspects of some of the studies I cite do superficially appear to support the societal reaction perspective. I have dealt with such studies in

a separate paper (Gove, 1970b), where I show that they have serious methodological flaws and that their data in fact tend to be open to interpretations that run counter to the societal reaction perspective. This other paper is cited in the one Akers is criticizing (p. 788), in footnote 1, where I specifically deal with the issue which Akers *thinks* I may have ignored (i.e., those aspects of a study being cited which would appear to support the societal reaction perspective).

Fourth. I plead guilty to presenting "the standard medical-psychiatric explanation" as the alternative to the societal reaction explanation. However, in spite of what Akers may think, the scientific *adequacy* of this *general approach* is becoming increasingly apparent even though many basic issues remain unresolved. I suggest that Akers look at the very extensive literature on mental illness, paying particular attention to etiological factors, both environmental and genetic, to the physiological processes associated with mental illness, (e.g. see Weil-Malherbe and Szara, 1971) and to recent advances in the medical treatment of the mentally ill.

Let me now turn briefly to the more specific issues of restitutive processes and rehospitalization. Regarding restitutive processes, one of the points I attempted to make was that hospitalization pressured family members to view the patient as ill and to act toward him in a sympathetic manner, whereas formerly they had simply viewed him as obnoxious and troublesome and had dealt with him in a hostile fashion. Thus hospitalization did not, as Akers suggests, support the conception of others but instead changed their conception in a positive manner. As I noted in my paper regarding rehospitalization, the evidence (which I cite) strongly indicates that the main reason persons are rehospitalized is the occurrence of a serious psychiatric disorder. Akers has not only refused to take my word for what these studies found but he apparently has studiously avoided looking at the original studies or even at the passages I quote from them. Let me present yet another quotation from these studies. According to Angrist *et al.* (1968:162) "virtually all readmissions were found to be precipitated mainly by continued or recurrent psychiatric difficulties. Unexpectedly, the social factors originally viewed as significantly related to readmission were not important."

To conclude, when I wrote my paper I recognized that it would be controversial and anticipated considerable debate. Unfortunately, as almost all the issues raised by Akers were dealt with more thoroughly in the original article than space has allowed me to deal with

them here, the present exchange has done relatively little to advance our knowledge.

Walter R. Gove
Vanderbilt University

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THE CAUSES OF BLACK RIOTS¹

The increased use of sophisticated mathematics in sociology appears to have two disadvantages. First, mathematical reasoning *per se* seems to be accepted as more legitimate, or more scientific, than verbal reasoning. Second, the mathematical analyst necessarily works with a very simplified model of the real world, and the shortcomings of this sort of simplification may not be recognized by many sociologists with limited mathematical facility. This paper will illustrate how an elegant mathematical analysis may obscure the real world phenomenon being investigated, through oversimplification.

Spilerman, in an excellent and provocative pair of articles on the location of black urban riots in the 1960s (1970, 1971), evaluates four assumptions: (1) riots are random occurrences, and all cities have an identical probability of experiencing a riot; (2) cities are heterogeneous in their underlying riot propensities; (3) the occurrence of one riot increases the likelihood of subsequent riots; (4) geographic contagion among cities contributes to the distribution of

riots. His analysis of probability models and his empirical data support only the heterogeneity assumption. Furthermore, he argues that the riot distribution is not consistent with an explanation which locates the causes of rioting in community conditions, and that outside of the South, the only important factor determining a city's riot-proneness is the size of its black population.

This paper takes issue with Spilerman's elegant, but perhaps over-simplified, explanation. We believe that available evidence supports all four of the above assumptions. Furthermore there is little reason to believe that Negro population size is the only, or even major, factor differentiating non-Southern cities in riot proneness.

Spilerman demonstrates that probability models based on assumptions two, three and four are consistent with the distribution of riots during 1961-1963; but he argues that the Poisson distribution, representing assumption one, does not fit the data, and therefore he rejects the assumption. Table 1, which is a modified form of Spilerman's Table 3 (1970: 631), clearly indicates that the incidence of rioting increased over the time period 1961-1968. This increasing riot rate may be a national characteristic, independent of any specific community characteristic. Therefore we may test assumption one in the context of a changing national situation by applying the Poisson on a year-by-year basis. Before applying this test, we should note the unfeasibility of having a black riot in a city with virtually no blacks. Spilerman's sample of 673 cities (all with population over 25,000 in 1960) includes 261 cities with zero to 1,000 blacks, none of which had a riot during 1961-1968 (1971: Table 1). In our Table 1, we have eliminated these cities from the distribution since they obviously cannot have the same probability of a disorder as a city with a significant black population. We estimated the Poisson λ_t for each year by dividing the number of cities experiencing a riot that year by the total number of cities (412).² The Poisson distribution for each year closely fits the actual distribution

¹ I wish to acknowledge the advice of Norman Balbanian, Eugene Drucker, William Gifford, Phil Rice, John Schenck, and Harry Schwartzlander.

² Although this is not the customary method of estimating λ_t , the essential point is that we have a consistent method which provides a Poisson distribution for each year that is reasonably accurate. Note that λ_t measures national riot-proneness and is independent of the characteristics of specific cities. The increase in λ_t —over the period 1961-68 may be considered a measure of the nation's systematic change in riot-proneness during that time.

Table 1. The Yearly Distribution of Racial Disorders Together with Predicted Values from the Poisson Distribution*

(k)	Number of Cities with k Disorders									
	1961-4		1965		Year 1966		1967		1968	
Number of Disorders	Ac-tual	Pre-dicted ($\lambda t = .036$)	Ac-tual	Pre-dicted ($\lambda t = .019$)	Ac-tual	Pre-dicted ($\lambda t = .068$)	Ac-tual	Pre-dicted ($\lambda t = .260$)	Ac-tual	Pre-dicted ($\lambda t = .252$)
0	397	397	404	404	384	385	305	318	308	320
1	10	14	8	8	23	26	85	83	78	81
2	3	0			4	1	18	11	18	10
3	2	0			0	0	3	1	6	1
4					0	0	1	0	1	0
5					1	0			1	0
Totals	412	411	412	412	412	412	412	413	412	412

*Includes only instances of black aggression, spontaneous origin. Data portions of this table are derived from Spilerman (1970:631).

of riots except for those cities with the highest number of riots.

Riots typically occur during a spring-summer "season." By stringing all years together, Spilerman imposed a periodicity which contradicts the Poisson independence assumption. In taking one year at a time, we removed this periodicity. While the simple Poisson does not give a perfect fit (since there is certainly a population-related bias operating), it is close enough to suggest that a large part of the phenomenon may indeed be explained by assumption one.

In order to reject assumption three (reinforcement), Spilerman notes that "cities with no prior history of racial turmoil were experiencing an increase in disorder-proneness during 1965-1967. This finding is clearly inconsistent with the reinforcement thesis." (1970:637) But the fact that reinforcement cannot explain that particular increase does not imply that the process is not acting. It simply proves that reinforcement is not a sufficient explanation for rioting. Reinforcement may be one of several causes. The reinforcement assumption argues that a city's future riot probability increases as a function of its previous number of riots. On a yearly basis, this means that the probability of having one or more riots in a year, $P(\text{riot})$, is less than the conditional probability of having two or more riots in a year if there has already been one riot that year, $P(\text{two or more riots}|\text{one riot})$. This probability, in turn, is less than the conditional probability of having three or more riots in a year if there have already been two riots in the city that year. This latter proba-

bility is denoted $P(\text{three or more riots}|\text{two riots})$.³ Table 2 shows the above three probabilities calculated on a yearly basis for the 412 cities with black population over 1,000. The reinforcement assumption is supported in the majority of cases, though 1967 is a striking exception. If we use the full sample of 673 cities to calculate $P(\text{riot})$, the reinforcement assumption is supported in almost all cases (not shown).

Spilerman empirically demonstrates that the larger a city's Negro population, the higher its incidence of riots; and he uses this result to reject assumption four on geographic contagion. The best evidence in favor of the geographic contagion hypothesis is the striking clustering of riots in two states, New Jersey and Michigan, during the two-week period following the Newark riot of 1967. Spilerman accounts for the high incidence of riots in these two states by the fact that they have very

³ To calculate $P(\text{three or more riots}|\text{two riots})$ for a given year, say 1967, we divide the number of cities with three or more riots that year (four cities) by the number of cities having at least two riots (twenty-two cities) and arrive at the quotient .18. The sequence of conditional probabilities could presumably continue if the yearly riot season were infinite in length, and if no other factors brought on a saturation effect.

Spilerman's rejoinder asserts that we should use $E(k)$ here. Our test conforms to his original (1970) interpretation of reinforcement as an increase in probability of riot. Furthermore, given heterogeneity, our test could fail to support reinforcement; whereas, an $E(k)$ test cannot.

Table 2. Conditional Probabilities of Rioting on a Year-by-year Basis Assuming that Reinforcement is in Effect.*

Probability	Year				
	1961-4	1965	1966	1967	1968
P(riot)	.04	.02	.07	.26	.25
P(2 or more riots 1 riot)	.33	.00	.18	.21	.25
P(3 or more riots 2 riots)	.40	--	.20	.18	.31

*Based on 412 cities with a black population over 1000.

large black communities (and large black communities have a higher likelihood of riot than small ones.) By comparing the incidence of post-Newark riots in non-Southern cities outside of New Jersey and Michigan, with riot incidence within New Jersey and Michigan, while controlling for size of the city's Negro population, he was able to reduce substantially the apparent effect of geographic contagion. He concludes that geographic contagion is at most a minor factor (1970:638-639). But the argument rests on an assumption that large Negro communities are no closer to sources of contagion than small Negro communities. This assumption may be questioned since the largest Negro cities tend to be located in the northeast quadrant of the country, a largely contiguous megalopolis. A rigorous test of this assumption requires cities as the basis of comparison, but it would be laborious to calculate the contiguous Negro population for a large number of cities. Therefore we moved to a rough, but easier test based on state-size units. Table 3 shows the thirty-two non-Southern states ordered by size of Negro population (mainly urban in these states.)

The size of each state's proximate black population was calculated by summing the Negro populations of all states which touch its border (excluding Canada and Mexico). The resulting table clearly indicates a relationship between size of Negro population and proximity to other black communities. Thus, when Spilerman controlled for Negro population size, he also (inadvertently) controlled for exposure to contagion; hence the apparent reduction in contagion effect becomes trivial. Therefore the geographic contagion assumption remains a viable hypothesis.

Spilerman supports the assumption that cities are heterogeneous in their underlying riot propensities; and he uses for evidence his discovery of the empirical relationship between city Negro population size and incidence of riots. His two papers present several feasibility

arguments why Negro population size may be the only relevant heterogeneity factor for non-Southern cities, but the direct evidence for this view comes from a multiple regression analysis which compares the power of several alternate explanations (1970:639-645). Many of the indicators used are extremely crude. For exam-

Table 3. 1960 Negro Population of the Non-South States and Their Border States (Excluding Alaska & Hawaii)

State	1960 Negro Population (thousands)	1960 Negro Population of Border States (thousands)
New York	1,418	1,587
Illinois	1,037	976
California	384	75
Pennsylvania	353	3,387
Ohio	786	2,145
Michigan	718	1,130
New Jersey	315	2,331
Missouri	391	2,528
Indiana	269	2,757
Massachusetts	112	1,546
Connecticut	107	1,548
Kansas	91	613
Wisconsin	75	1,803
Washington	49	20
Arizona	43	959
Colorado	40	341
Nebraska	29	551
Iowa	25	1,556
Minnesota	22	102
Rhode Island	18	219
Oregon	18	948
New Mexico	17	1,428
Nevada	13	951
Utah	4	118
Maine	3	2
Wyoming	2	77
New Hampshire	2	116
Idaho	2	88
Montana	1	6
South Dakota	1	81
North Dakota	1	25
Vermont	1	1,531

ple, percentage changes in total and nonwhite population from 1950 to 1960 are taken to indicate "social disorganization" (1970:639); population per councilman is taken to indicate relevant "political structure" (1970:641). These sorts of approximations cannot easily be avoided when one must use readily available data, but one must recognize that such crude indicators might explain very little variance *even when the explanations which they represent are substantially correct*. Negro population size, on the other hand, accurately indicates the size of a black community. Therefore the fact that Negro population size is the best explainer of variance may mean nothing more than that it is the least crudely measured of a series of equally important explanatory variables.⁴ Also, considering that Negro population size is correlated with proximity of contagion sources, some of the variance "explained" by it may be due to contagion effects.

In sum, one could assert the following about black urban riots in the 1960s without being grossly inconsistent with available evidence: National disorder-proneness increased from the early 1960s to the late 1960s and then decreased markedly by 1971. The location of riots within any one year may largely have been a matter of chance; however, there was an additional systematic tendency for cities with large Negro populations to have a high number of riots. This may be explained by the fact that these large cities are close to sources of riot contagion and/or by their large Negro population size per se. There is some evidence that the occurrence of rioting in a city increases (reinforces) the probability of a future riot there. Riots may be caused, in part, by underlying community conditions. In short, the causes of black riots are complex and not well understood.

ALLAN MAZUR
Syracuse University

⁴The incidence of several types of disruptive social protest (beside black riots) increase as a function of population size. Student riots were more likely to occur at larger than smaller universities (Scott and El-Assal, 1969); bombing incidents in the United States occur disproportionately in the larger cities (Stout, 1972); fluoridation referenda (a sign of protest) and referendum defeats are both more likely to occur in larger cities (Crain *et al.*, 1969:217); and the likelihood that a nuclear power plant will have a siting conflict increases with the population of its nearest metropolitan area (Mazur, 1972). This uniformity of results suggests that the effect of a large population is simply to increase opportunities and resources for the outbreak of protest.

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STRATEGIC CONSIDERATIONS IN ANALYZING THE DISTRIBUTION OF RACIAL DISTURBANCES*

Allan Mazur has prepared a thoughtful comment on two papers of mine which investigate the locations of racial disorders. While I believe his criticisms to be in error, the mistakes represent common confusions concerning procedure in model construction and verification. For this reason, in addition to replying to his specific allegations, I will discuss some general aspects of strategy, and cautions which must be observed, in attempting to comprehend social phenomena by mathematical and statistical means.

Mazur makes four points. He argues that if the annual disorder distributions are analyzed separately, in place of being aggregated, the simple Poisson process (which postulates an identical riot-proneness value for all cities) cannot be rejected. Second, Mazur contends that "the reinforcement assumption is supported in almost all cases"¹ and, as a consequence, a disorder outbreak raised the proba-

* This paper was written while the author was supported by the Russell Sage Foundation. Comments by John Kelley of Columbia University on an earlier draft are gratefully acknowledged.

¹ Quotations without a reference are from Mazur's paper.

bility of subsequent turmoil in the same city.² Third, he believes that riot propagation by geographic contagion remains a viable hypothesis. "When Spilerman controlled for Negro population size, he also (inadvertently) controlled for exposure to contagion" since large ghettos tend to be concentrated in the northeast, in proximity to one another. Fourth, Mazur suggests that the insignificance of the community characteristics may have resulted from the use of crude indicators. "The fact that Negro population size is the best explainer of variance may mean nothing more than it is the least crudely measured [among] . . . equally important explanatory variables."

THE INADEQUACY OF AN IDENTICAL RIOT-PRONENESS VALUE (λ) FOR ALL CITIES

Mazur argues that since the increasing rate of disturbance during 1961-68 represented a national trend, the thesis that λ is the same for all cities should be tested by applying the Poisson distribution to data from each year separately. For the moment we will accept this suggestion to disaggregate the disorder distribution. Also, I have no objection to Mazur's removal of the 261 cities with Negro populations below 1,000 although, as he notes, this constitutes recognition that (at least between the deleted and retained cities) differences in λ -values do exist.³

Yet, even following Mazur's procedures for investigating community differences in disorder-proneness, the Poisson model must be rejected on the basis of a χ^2 test⁴ in four of the five periods presented in his Table 1. Moreover, in each of these time intervals, the variance of the observed distribution exceeds its mean,⁵ a condition which indicates either hetero-

geneity or reinforcement (Spilerman, 1970: 632-635).

From the perspective of analytic strategy, a more crucial matter concerns the inadvisability of Mazur's suggestion to disaggregate the disorder distribution. Disaggregation is unnecessary for examining the constancy of λ , and it is also unwise. It is unnecessary because, were the cities characterized by a common λ -value which followed a national trend, as Mazur believes, *the aggregate disorder distribution for 1961-1968 would be Poisson*. Consequently, a test of Mazur's contention does not require examination of the separate annual distributions. Under the section title, "The Time Dependent Poisson Process" (Spilerman, 1970:632), the thesis of a common λ -value which varied over time was treated at length and rejected. While that test was performed on all 673 cities, reestimating the Poisson distribution using only the 412 communities with Negro populations in excess of 1,000, leaves my original conclusion unaltered (Table 1). The assumptions underlying the Poisson process simply are not satisfied by the disorder data. In particular, the homogeneity requirement for the λ -values is violated, as argued in Spilerman (1970:632-639).

Not only is disaggregation unnecessary for testing Mazur's contention, it is unwise because of the toll it takes on degrees of freedom, weakening the power of a statistical test, and should be avoided where alternative procedures are available. In clarification of this point, note that had Mazur argued for examination of the disorder distribution by month, rather than by year, there would have been too few outbreaks in most time periods to permit rejection of the Poisson model. Indeed, I suggest this is why the Poisson specification cannot be rejected for 1965—only eight disorders occurred in that year.

At times there will be compelling substantive reasons for paying the price of disaggregation. For instance, one might hypothesize that the factors which made for disorder-proneness changed suddenly in 1967, in the aftermath of the Newark riot. Prior to that outbreak (according to the argument), the racial disorders reflected deprivations and tensions peculiar to the stricken communities. However, as a result of the extensive publicity given the Newark outburst, a climate conducive to social imitation developed which re-

² Mazur's discussion of reinforcement is restricted to the possibility of positive reinforcement.

³ The notion of a threshold, below which outbreaks are extremely unlikely, is a reasonable assumption. Indeed, this very suggestion was made in Spilerman (1971:429). The disagreement is over whether Negro population size influences disorder-proneness once the threshold value is exceeded. Our position is that it does, and the measure of its effect was reported in Spilerman (1971:Table 1, line 4).

⁴ The test performed is the variance test for a Poisson distribution (Maxwell, 1961:112). This test is more sensitive than a goodness of fit test, and is particularly useful where entries in several cells are small. For the periods 1961-1964, 1966, 1967, and 1968, $\chi^2(412)$ equals 728, 697, 480, and 575 respectively. All values are significant at the .01 level.

⁵ The mean and variance for the years 1961-1964, 1966, 1967, and 1968 are respectively, (.053,

.094), (.087, .148), (.325, .380), (.342, .478). Indeed, a cursory inspection of the annual distributions reveals the tendency for the departures from the predicted distributions to be systematic, with too many cities experiencing several disorders.

TABLE 1. DISTRIBUTION OF RACIAL DISORDERS FOR 1961-68, TOGETHER WITH PREDICTED VALUES FROM POISSON*

(k) Number of disorders	(n_k) Number of cities with k disorders (actual)	($\hat{n}_k$) Number of cities with k disorders (Poisson, $\lambda = .828$)
0	243	180
1	93	148
2	36	62
3	19	17
4	10	4
5	4	1
6	4	
6	2	
7	1	
8	0	
9	1	
10	2	
11	1	
Σn_k	412	412
$\bar{k}$	.828	$\chi^2(3) = 105^{**}$ $P < .0001$
s_k^2	2.211	

*Data from Spilerman (1970: Table 2). Cities with Negro population below 1,000 persons deleted from analysis.

**Values in cells 4-11 were combined.

placed local causes of discontent as primary instigators of collective violence. Disaggregation would be necessary for examining this contention, or similar questions; but Mazur does not raise substantive issues.

Mazur concludes this portion of his comment with the following assertion: "While the simple Poisson does not give a perfect fit, it is close enough to suggest that a large part of the phenomenon may indeed be explained by assumption one, i.e. randomness." I have already indicated the procedural inadequacies with his analysis, and the rather clear evidence for rejecting the Poisson model. Nonetheless, there are two further problems with this assessment. First, randomness is *not* at issue; the compound Poisson (which permits heterogeneity) also requires the randomness (in time) assumption. The disagreement is over whether we can reject a hypothesis that the cities were characterized by an identical λ -value.

Second, his assertion is inappropriate to distributional models. The statistical tests we commonly perform on these models refer to the presence or absence of an effect; they are not partialing procedures. It may be the case

that an equal λ -value assumption is appropriate for some subset of the cities or, more generally, that they can be grouped according to common values of disorder-proneness. The intellectual exercise is to then ascertain which community characteristics provide an efficient and theoretically meaningful grouping. My own analysis suggested precisely this situation: Cities with equal sized Negro populations had equivalent disorder propensities during the 1960's. Indeed, I indicated that if the cities are grouped on Negro population size, the assumptions of the Poisson model become applicable to each subcategory (Spilerman, 1970:646-648).

THE INADEQUACY OF A REINFORCEMENT THESIS

Mazur's argument in behalf of reinforcement rests on his belief that because $P(n+1 \text{ riots} | n \text{ riots})$ typically exceeds $P(n \text{ riots} | n-1 \text{ riots})$, "the reinforcement assumption is supported in the majority of cases."

Let us first note that Mazur's formulation in terms of conditional probabilities leads to peculiar results. For instance, calculating these probabilities for the *predicted* Poisson distribution for 1967 (from Mazur's Table 1), we obtain

$$P(1 \text{ or more riots}) = 95/413 = .23$$

$$P(2 \text{ or more} | \text{one}) = 12/95 = .13$$

$$P(3 \text{ or more} | \text{two}) = 1/12 = .08$$

These values are most interesting. They suggest the operation of negative reinforcement, yet we know that the Poisson process incorporates no such assumption!

Mazur's error was in using conditional probabilities when he should have computed a sequence of estimates of $E(k)$, successively deleting observations at low values of k . That is, $E(k)$ is estimated from the full distribution, and from observations satisfying $k \geq 1$, $k \geq 2$, etc. Carrying out these calculations with the predicted 1967 distribution, we find

$$E(k) = 108/413 = .26$$

$$E(k; k \geq 1) = 12/95 = .13$$

$$E(k; k \geq 2) = 3/12 = .25$$

where $E(k; k \geq n)$ is the estimate of $E(k)$ obtained from observations satisfying $k \geq n$.⁶ Thus, except for rounding error, the expected number of disorders is constant, irrespective of the prior requirement. This is the conditional statement which should have been made, and the above result for the Poisson is meaningful.

In Table 2 we present estimates of $E(k)$

⁶ The successive estimates of $E(k)$ were calculated using incomplete moments:

$$E(k; k \geq n) = \frac{\sum k P_k}{\sum P_k} \quad k = n+1 \quad k = n$$

for $n = 0, 1, 2$.

computed from the observed distributions in Mazur's Table 1. Not surprisingly, these values are more supportive of Mazur's position than his own results. In particular, the embarrassing exception of 1967 is reversed. But do the results constitute evidence for a reinforcement thesis *as opposed to heterogeneity*? Not at all; they are consistent with either explanation. If there is heterogeneity, the successive estimates of $E(k)$ will increase because of the sorting out of cities by their disorder propensities. The communities which are able to satisfy each more restrictive prior requirement become, increasingly, ones with high disorder-proneness values. Indeed, it is well known that reinforcement and heterogeneity cannot be distinguished by comparisons such as Mazur proposes.⁷ For example, in a discussion of this matter, Arbous and Kerrich (1951:412) conclude, "if the observations are arranged as a univariate distribution, it is impossible to decide whether the compound Poisson [(heterogeneity)] or [positive reinforcement] is the more plausible." This very point was also made in Spilerman (1970:635).

If we cannot choose between these hypotheses by such statistical calculations, how do we decide in favor of one? I have suggested that substantive considerations must be brought to bear, and that they provide overwhelming evidence for heterogeneity, but no comparable support for reinforcement. The argument on behalf of heterogeneity rests upon the empirical finding that Negro population size made an enormous difference for disorder-proneness. During 1961-1968, a city with a Negro population in excess of 100,000 was twelve times as likely, on the average, to experience a disturbance than one with a Negro population in

the range 2,500-5,000. Using multivariate methods, among the 413 communities⁸ with more than 1,000 Negro residents, the correlation between number of disorders and Negro population size (controlling for region) is .668 (Spilerman, 1971:429). To me, these facts speak persuasively in support of heterogeneity.

We can also attempt a more direct test of the reinforcement thesis. Were this mechanism operative then, net of the determinants of heterogeneity, knowledge of disorder locations in one time interval should increase their predictability in the succeeding time period. To test this possibility the disorder locations in 1968 were analyzed in terms of the outbreak distribution one year earlier. Two dummy variables, D68 and D67, were defined, each assuming the value one for a city if it experienced racial turmoil in the requisite year. D68 was then regressed against D67, with controls present for Negro population (N_c) and region (a dummy variable, $S=1$ for South). In the resulting equation,

$$D_{68} = -1.182 - .155S + .167\log N_c - .014D_{67} \\ (-8.77)(-3.50) (10.32) \quad (-.27) \\ R^2 = .253$$

the insignificance⁹ of the coefficient for D67 indicates that knowledge of the 1967 disorder locations is redundant once the determinants of heterogeneity have been controlled.

It is conceivable that reinforcement operates within a riot season (Spring to the Fall months) but not across years. This possibility

⁷ Mazur appears to have confused an individual level process (reinforcement) with observations on a population. There is nothing wrong with the conditional argument as it pertains to a single city. His error was in identifying proportions of cities with conditional probabilities for a city.

⁸ While there were 412 communities in 1960 with Negro populations exceeding 1,000 persons, the 1960 Census of Population (while presumably presenting Negro population characteristics for only these communities) included data for Great Falls, Montana, which had 904 Negroes. All 413 communities were used in the regressions.

⁹ T-values are in parentheses; $|t| > 2.00$ is significant at the .05 level.

TABLE 2. SUCCESSIVE ESTIMATES OF EXPECTED NUMBER OF DISORDERS, BY YEAR*

	Year				
	1961-64	1965	1966	1967	1968
$E(k)$	.05	.02	.09	.32	.34
$E(k; k \geq 1 \text{ riot})$	.80	--	.46	.42	.61
$E(k; k \geq 2 \text{ riots})$	1.20	--	1.00	.59	1.04

*Calculated from observed data in Mazur's Table 1.

can be investigated for 1968.¹⁰ Let D68A and D68B be dummy variables, the first taking the value one for a city if it experienced a disorder prior to May, the second taking this value if the city experienced a disturbance in subsequent months.¹¹ Regressing D68B against D68A, and controlling for the determinants of heterogeneity, we obtain

$$D_{68B} = -.621 - .150S + .088\log N_c + .067D_{68A} \\ (-5.76) (-4.69) (7.05) (1.54) \\ R^2 = .170$$

Again, the insignificance of the coefficient of D68A indicates that knowledge about prior disturbances does not improve predictability. We therefore fail to find evidence to support reinforcement. It should be noted, in passing, that while we have not yet considered Mazur's contention regarding a possible confounding of geographic contagion with Negro population size, that claim cannot salvage the reinforcement thesis. The fact that community characteristics make a difference with regard to disorder-proneness is evidence for heterogeneity. Why they matter is another issue.

A comment on analytic strategy. It is apparent that Mazur does not consider parsimony a virtue. He writes that although "reinforcement is not a sufficient explanation for rioting [it] may be one of several causes." True, but so might be divine guidance. The essential point is that we have no reason for invoking either mechanism. With respect to reinforcement, (a) there is no evidence to support its operation as a systemic factor, and (b) we do not require the reinforcement assumption to explain any aspect of disorder generation. The heterogeneity thesis is quite adequate for coping with this process and the evidence in its behalf is substantial.

As a final consideration, it should be noted that Mazur's first and second contentions are inconsistent. If he believes that a common λ -value characterizes the cities, his argument on behalf of reinforcement cannot be correct. Conversely, if he believes that reinforcement was operative, then his first contention must be false. The two specifications are contradictory.

¹⁰ A similar analysis cannot be carried out for 1967. Approximately two-thirds of the riots in this year occurred in July, following the major Newark outburst. Irrespective of how this year is subdivided by grouping months, one of the intervals will contain too few incidents to permit a comparison.

¹¹ This division corresponds basically to comparing the post-Martin Luther King-assassination disorders in April with the summer riots.

THE INADEQUACY OF A GEOGRAPHIC CONTAGION THESIS

Mazur argues that the reported correlation between number of disorders and Negro population size may be masking the operation of geographic contagion because large Negro population centers tend to be concentrated in the northeast, in close proximity. "Therefore," according to Mazur, "the geographic contagion assumption remains a viable hypothesis."

His evidence for this confounding consists of an association between Negro population size within a state and Negro population in the neighboring states (Mazur's Table 3). Before the geographic contagion thesis is examined, it should be noted that what is shown by this table is far from evident. While each state is listed once, the units we have been regarding as susceptible to disorder are *communities*; in particular, ones with 1,000 or more Negro inhabitants. New York State, alone, contains twenty-five cities which satisfy this criterion while the last ten states on Mazur's list, together, include only five such communities. For this reason, it is unclear just what can be learned from a comparison of the columns.

Leaving aside this difficulty, we will undertake several analyses to determine the merit of the geographic contagion thesis. These examine the disorder experience of a community as a function of the size of its neighboring Negro population, or the prevalence of disturbances in nearby cities, net the effect from its own Negro population.

(a) The most direct test of Mazur's assertion can be obtained by regressing the number of disorders in a city during 1961-1968 (D) against Negro population in the city (N_c), and a contextual variable which equals the Negro population in states contiguous to the one in which the city is located (N_{cs}):¹²

$$D = a + b_1 \log N_c + b_2 \log N_{cs} \quad (1)$$

Support for geographic contagion would be indicated by a significant (positive) coefficient corresponding to $\log N_{cs}$.

(b) The above test assumes that Negro population in the border states *makes an additive contribution* to community disorder-proneness. Instead, it may be the case that the relationship between D and N_c differs according to whether the community is located in

¹² The N_{cs} values are from column 2 of Mazur's Table 3. In the analysis which follows only cities with Negro populations greater than 1,000, and in states on Mazur's list, were included. A dummy variable for region is absent because the analysis is restricted to the non-South.

a state having a large or a small Negro population in the contiguous states. To test this possibility a dummy variable (V) was defined which equals one if Negro population in the border states exceeds 1.5 million, and is zero otherwise. In the regression model,

$$D = a + b_1 \log N_c + b_2 V + b_3 (\log N_c) \bar{x} V \quad (2)$$

support for geographic contagion would be indicated by a significant (positive) value of either b_2 or b_3 .

(c) The operation of geographic contagion in facilitating the spread of disorder stems, presumably, from the greater responsiveness of Negroes to outbreaks in neighboring ghettos than to ones in distant locales. Individuals are likely to have acquaintances in nearby cities, to be in communication with them by telephone during a crisis (National Advisory Commission, 1968:38) and, generally, to empathize with events in proximate and familiar settings. Cities closest to a stricken community therefore should be the most prone to incur sympathetic disturbances. Indeed, if geographic contagion were operative we would expect a community's disorder propensity to reflect the extent of racial violence *elsewhere in that state*. This is because within-state communities, on the average, are likely to be located nearby, and because state boundaries tend to define conceptual units for reference group behavior.

Formalizing the above argument we conclude that, controlling on Negro population in a community, the greater the number of outbreaks elsewhere in the state, the higher should be the number of its own disorders. Two regression models were used to test this proposition,

$$D = a + b_1 \log N_c + b_2 D_s \quad (3)$$

and

$$D = a + b_1 \log N_c + b_3 R_s \quad (4)$$

where D_s equals the number of disorders which have occurred elsewhere in the state during 1961-1968, and R_s is this figure standardized by the number of disorder-susceptible communities in the state. A significant positive coefficient for D_s in (3), or for R_s in (4), would constitute evidence for geographic contagion.

The results from these four regressions are presented in Table 3. It is apparent from the t -values (in parenthesis) that none of the coefficients which could be interpreted as support for geographic contagion is significant at the .05 level. Nor, as I have argued (Spilerman, 1970:645-646), is this surprising. The 1960's was a period of emerging racial solidarity among black persons, and there is no evidence that this development was hindered by city or state boundaries. Moreover, the exten-

sive coverage given the disorders by the television networks acquainted black persons *throughout the country* with this manner of expressing discontent and frustration, as well as elicited sympathetic disturbances.¹³

RESEARCH WITH CRUDE INDICATORS

Mazur recognizes that one often has available only crude indicators, particularly when comparable information is required for a large number of communities. The relevant questions concern how to proceed in this circumstance, and what can be learned with crude measures.

(a) Several indicators should be used for each construct. While this can create multicollinearity problems when they are included simultaneously in an equation, this issue is of no consequence if each is separately insignificant, net of the controls, which was the case in the disorder regressions. On this matter, referring to my analysis, Mazur writes, "population per councilman is taken to indicate relevant 'political structure.'" This statement is misleading; population per councilman was one among four structural variables used to measure political responsiveness. Likewise, no other construct was represented by fewer than three indicators.

(b) One would expect more refined measures to be highly correlated with the indicators used in this study since they tap major aspects of Negro opportunity. More refined measures of community-based discontent would surely be associated with factors such as housing quality, income, and occupational status. As a result, if other measures were related to disorder-proneness, some of our sixteen indicators should convey their effects.

(c) While it is impossible to prove the absence of a relation between unexamined community characteristics and disorder-proneness, we can entertain strategies which attempt to ascertain the necessity for introducing additional variables. This was the intent of the section titled, "What about the Unexplained Variation?" (Spilerman, 1971:433-435). An estimate was calculated of the amount of explained variation which would be produced by the variable Negro population size *were the proposed explanation correct*. For the non-South, this calculation is consistent with the

¹³ In the case of incidents which were reported on local news programs but were too minor for network coverage, sympathetic disturbances may have been induced disproportionately in nearby communities. Our analysis suggests that there were few instances of this sort of propagation.

TABLE 3. REGRESSION EQUATIONS FOR TESTING GEOGRAPHIC CONTAGION THESIS*

Equation	Independent variables**							R ²
	Const.	log N _C	log N _{Ce}	V	(log N _C)xV	D _S	R _S	
(1)	-6.314 (-11.43)	.860 (17.10)	-.024 (-.47)					.540
(2)	-7.313 (-10.72)	.967 (12.43)		1.418 (1.59)	-.181 (-1.78)			.548
(3)	-6.414 (-13.89)	.858 (17.06)				-.003 (-.44)		.540
(4)	-6.418 (-12.95)	.858 (16.89)					-.043 (-.24)	.540

*Entries are unstandardized regression coefficients. T-values are shown in parentheses; $|t| > 2.00$ is significant at the .05 level.

**Observations are all cities with Negro populations in excess of 1000 persons, which are also in states in Mazur's list. N = 252.

thesis that the disorder locations do not reflect variations in the community situation of Negroes.

(d) The plausibility of the findings should be evaluated in light of other knowledge we have about the phenomenon. In the disorder study the analytic findings were reviewed in the context of other developments during the 1960's (Spilerman, 1970:645-646). Several factors were enumerated which, it was suggested, operated to reduce the salience of community conditions as a primary cause of aggression, and elevate in their place geographically diffuse stimuli.

CONCLUSIONS

My assessment of the evidence concerning the issues raised by Mazur is that there is no reason for altering my original argument. In summary, (a) the equal proneness thesis is incompatible with the disorder distribution; (b) heterogeneity was a factor of massive importance, but no remotely comparable claim can be made for reinforcement; (c) the geographic contagion thesis cannot be supported; and (d) the sole explanation which is consistent with both the formal analysis and with our informed understanding of the period is one which argues that the causes of the rioting are not to be found in conditions of deprivation specific to the stricken communities. What we have witnessed was a national phenomenon.

As a final point, it should be noted that our understanding of the causes of the outbreaks does not rest upon the high correlation with Negro population size; nor is it surprising

that the incidence of other kinds of disruptive protest are a function of population size (Mazur's footnote 4). The fact that a large population "increases opportunities and resources for the outbreak of protest" simply means that one must control on this variable to assess the importance of correlated factors, which bear greater theoretical significance. Indeed, the thrust of the investigation was to ascertain which indicators of community-based discontent are related to disorder frequency once Negro population is held constant (Spilerman, 1970:643-645). The analytic underpinnings of our conclusion derive from the insignificance of these community characteristics.

SEYMOUR SPILERMAN

University of Wisconsin

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A COMMENT ON "ORGANIZATION STRUCTURE AND COMMUNICATIONS"

The article "Organization Structure and Communications" by Jerald Hage, Michael Aiken, and Cora Bagley Marrett (ASR, October 1971:860-871) contains some serious deficiencies of definition and measurement. The unfortunate definition of *feedback* as "a high volume of communication of information relevant to the work of the organization" (Hage, Aiken and Marrett: 863) neglects several features ordinarily associated with that venerable concept, such as reciprocal causation, cybernetic control, and equilibrium. One is free to choose one's own definition, but the difficulty in defining feedback in terms of the sheer volume of communication is that careless readers may assume that the usual feedback mechanisms of system theory are present in a situation where they have not been demonstrated. Given the meager data of this article it is not at all clear that "communication" entails feedback to a higher degree than does "planning or programming." If one assumes, as the authors do, that "... coordination through planning or programming is more likely to imply use of sanctions to achieve social control" (Hage, Aiken and Marrett, 860), and if one also assumes that sanction systems often provide examples *par excellence* of feedback involving reciprocal causation and cybernetic control (not to mention communication), then it becomes even more untenable to make a facile association of "communication" with "feedback" and "planning or programming" with "sanctions."

One of the hypotheses of this study states that "the greater the degree of complexity" of an organization, "the greater the proportion of horizontal task communications" (Hage, Aiken and Marrett, 863). While the theoretical rationale for the distinction between horizontal and vertical communications is obscure at best, it is important to note that in assessing the prevailing direction of communication in a formal hierarchy one encounters a finite number of potential horizontal links and a finite number of potential vertical links; and *the ratio of potential horizontal links to potential vertical links varies substantially and systematically according to the size and shape of the hierarchy*. Table 2 of the Hage-Aiken-Marrett article shows a high correlation (.67) between a measure of horizontal communication and a measure of complexity; but if the "size" and "shape" of an organization are related to both

horizontal communication and complexity, then the effects of size and shape must be eliminated if one wishes to understand the independent interaction of communication and complexity, as this article attempts to do. Perhaps an example of the importance of "size" and "shape" (or "span") will suffice.

Consider a formal hierarchy in which (1) there is a single unit at the apex, (2) each additional level (or rank) has a number of units which is twice that of the level immediately above it, (3) all interaction is dyadic, and (4) a given unit can only interact with those units that are *directly* above, *directly* below, or on the same level as itself. Let L_i represent the number of units at a given level, while n is the number of levels. The vertical interaction potential VIP, defined as the maximum possible number of vertical dyads, is

$$\sum_{i=1}^n (L_i L_{i+1}) = (L_1 L_2) + (L_2 L_3) + (L_3 L_4) + \dots + (L_{n-1} L_n)$$

The horizontal interaction potential HIP, defined as the maximum possible number of horizontal dyads, is

$$\sum_{i=1}^n \frac{L_i(L_i-1)}{2} = \frac{L_1(L_1-1)}{2} + \frac{L_2(L_2-1)}{2} + \dots + \frac{L_n(L_n-1)}{2}$$

If HI is defined, in rough conformity with Hage, Aiken, and Marrett, as the observed number of horizontal communication dyads in a formal hierarchy, and VI as the observed number of vertical communication dyads in such a hierarchy, then a more accurate measurement of the preponderance of horizontal interaction could be obtained by standardizing the ratio HI/VI against the ratio HIP/VIP, e.g., as in

$$\frac{HI \cdot VIP}{VI \cdot HIP}$$

The necessity for such standardization arises from the fact that HIP/VIP is not a constant. The vertical interaction potential for n -level hierarchies, VIP_n , is a geometrical series with a sum $S_n = a(1-r^N)/(1-r)$. In this case, $a = 2$, $r = 2^2$, and $N = n - 1$. The vertical interaction potential for a given hierarchy, then, is given by the formula

$$VIP_n = \frac{2[1 - (2^2)^{n-1}]}{1 - 2^2} = \frac{2(2^{2n-2} - 1)}{3} \quad (1)$$

In the formula

$$\begin{aligned} \text{HIP}_n = & \frac{1}{2}[L_1(L_1 - 1) + L_2(L_2 - 1) + \\ & \dots + L_n(L_n - 1)] = \\ & \frac{1}{2}[L_1^2 + L_2^2 + \dots + L_n^2 - \\ & (L_1 + L_2 + \dots + L_n)], \end{aligned}$$

the squared terms form a geometric series with the sum

$$A_n = \frac{2^{2n} - 1}{3} = \frac{(2^n - 1)(2^n + 1)}{3}$$

and the remaining terms have the sum

$$B_n = \frac{1 - 2^n}{1 - 2} = 2^n - 1.$$

Therefore, the horizontal interaction potential for our simple hierarchy is

$$\begin{aligned} \text{HIP}_n = & \frac{1}{2} \left[\frac{(2^n - 1)(2^n + 1)}{3} - (2^n - 1) \right] \\ = & \frac{1}{2}(2^n - 1) \left(\frac{2^n + 1}{3} - 1 \right) = \\ & \frac{(2^n - 1)(2^n - 2)}{6}. \end{aligned} \quad (2)$$

By formulas (1) and (2),

$$\begin{aligned} \frac{\text{HIP}_n}{\text{VIP}_n} = & \frac{(2^n - 1)(2^n - 2)/6}{2(2^{2n-2} - 1)/3} = \\ & \frac{(2^n - 1)(2^n - 2)}{4(2^{2n-2} - 1)}, \end{aligned}$$

so that for small hierarchies there is a preponderance of potential horizontal links over potential vertical links. For hierarchies of increasing size,

$$\begin{aligned} \lim_{n \rightarrow \infty} \frac{\text{HIP}_n}{\text{VIP}_n} = & \lim_{n \rightarrow \infty} \frac{(2^n - 1)(2^n - 2)}{4(2^{2n-2} - 1)} = \\ & \lim_{n \rightarrow \infty} \frac{2^{2n} - 3(2^n) + 2}{4(2^{2n-2} - 1)} = \\ & \lim_{n \rightarrow \infty} \frac{1 - \frac{3}{2^n} + \frac{2}{2^{2n}}}{4 \left(\frac{1}{2^2} - \frac{1}{2^{2n}} \right)} = \\ & \frac{1 - 0 + 0}{4(\frac{1}{4} - 0)} = 1. \end{aligned}$$

In a large hierarchy, then, HIP and VIP tend to be equal.

For such a simple hierarchy as that dealt with in this example, the ratio HIP/VIP ranges from .5 to 1.0, allowing a variation of substantial significance when used as a basis for standardizing another variable. If one entertains more complex (and more realistic) assumptions about the size and shape of

hierarchies, the assessment of HIP/VIP becomes commensurately more difficult and more important.

MICHAEL A. FAIA

College of William and Mary

REPLY TO FAIA *

We are pleased for this opportunity to pursue in detail some points discussed in our paper (Hage, Aiken and Marrett, 1971). The original manuscript was sixty pages long, but, because of space limitations, almost half the original manuscript was sixty pages long; but, version and with it the more detailed discussion of Faia's points.

One issue raised by Faia concerns the meaning of the concept of feedback. From our perspective feedback has two possible meanings: (1) information flow about what occurs in an organization and (2) information flow that changes some aspect of an organization. We find it difficult to separate these. The more information one receives about an organization's operations, the more likely this information will contain reports about malfunctions, errors, or difficulties that in turn will result in action being taken to remedy the situation. The second meaning here, i.e. information that creates change, is the same as the idea of a signal from a "rate variable" to a "state variable" (Forrester, 1969) and is, of course, cybernetic control. A "rate variable" is one that is monitored; while a "state variable" is one that is altered to modify one of the "rate variables." Some simple examples of state and rate variables in organizations are increases in deviance or conflict (rate variables) followed by increases in socialization or sanctions (state variables). Alterations in these state variables are likely to result in a decrease in the deviance or conflict. Or consider the example of students on strike against the university; the police are likely to be called and the strike broken. This is a familiar illustration of a cybernetic control system. The key point is that rate variables are monitored, and state variables are those responding to changes in the rate variables. Our theory suggests that high feedback with socialization as well as the use of sanctions or coercion in conjunction with programming are different responses with the same consequence for the rate variables.

* We wish to express our appreciation to Jerry Marwell, Ken McNeil, and Richard Schoenherr for helpful comments on an earlier version of this reply.

We meant feedback in both senses, and we believe that March and Simon (1958) probably did as well. In using March and Simon's concepts, James Thompson (1967) interprets programming to mean standardization, and feedback to mean mutual adjustment. Likewise, Burns and Stalker (1961), contrast a hierarchical system of authority, communications, and control with a network system of authority, communications, and control. Regardless of the terms, most of these authors see cybernetic control (and coordination) being exerted. In one system, the need for feedback, danger signals, or reports on malfunctions is largely eliminated by standardization. In the other system, continuous adjustment occurs because the system *cannot* be standardized or programmed.

As feedback increases in volume, more information comes from all parts of the organization. For example, instead of receiving only reports about the volume of production, the entire process of production or of treatment in a people processing organization is monitored. Likewise, as the volume of feedback increases, more and more aspects of the managerial process are monitored. Not only do subordinates give and request information from their superiors within departments, but department heads confer with workers in other departments and vice versa. Clearly, these interchanges of information can function both as a coordinating mechanism and as a socialization mechanism at the same time. This is not to say that there is no information flow in a programmed organization but that its volume is less; and therefore, it is likely to consist of subordinates reporting to their superiors. The simplest and easiest things to monitor under this system are various measures of quantity production, as for example in Blau's case study of a welfare agency (1956).

The important point is that as feedback increases, there is an increasing probability of reports requiring changes, and thus there is likely to be *more* control with high feedback and socialization. From this flow several implications: (1) a programmed system has only a few reports from subordinates and these are likely to concern outputs, (2) a high feedback system has many reports and these are likely to concern the production process. Given much more information from all parts of the organization, there is naturally more detailed knowledge and thus tighter control. The increase in volume means tapping in at many more time-points in the production process. We might call the high feedback coordination system a "quality control system" and the programmed

one a "quantity control system." Both are cybernetic control systems; but they differ vastly in the volume of information, its detail, and the number of parts of the organization originating information.

Again, most organizations can be conceived of as being on some continuum, in terms of emphasis on programming and feedback as well as on sanctions and socialization, and thus between quantity and quality control. These phenomena are, however, not mutually exclusive. Most organizations have some of each. It is the relative emphasis on one or the other which concerns us here.

Since we cannot claim to have measured feedback directly, especially in the sense of reports that may precipitate corrective action, we greatly stressed both the volume of communication, scheduled or unscheduled, and its direction, vertical or horizontal. This line of reasoning makes clear why volume is important; the greater the volume of communication (which implies the volume of information), the greater the probability of feedback occurring. Likewise, horizontal communication is most likely to contain this kind of information. Others have noted the difference in content and meaning of horizontal and vertical communication (Landsberger, 1961; Guetzkow, 1965; Burns and Stalker, 1961; Katz and Kahn, 1966); and, therefore, we did not feel extended discussion was necessary.

Insofar as feedback is an alternative mechanism of coordination to programming, the direction of communications is critical. In a system with high feedback, information should move between departments rather than merely within. As department heads confer with subordinates in other departments and workers confer with department heads of other departments, we can more reasonably infer that these conferences concern coordination of the departments, though all such communications need not concern such objectives. Horizontal communication flows are more likely to involve such content than vertical ones.

A careful examination of the correlations makes clear that for both scheduled horizontal communications organization-wide committees) and unscheduled communications (same status levels) the magnitude of the correlation is greater than for vertical flows. This means, of course, that not all communication is feedback for coordination and control; and, therefore, a concern about volume and direction is necessary.

Faia's second point implies that our results may simply be a function of the organization's "size" and "shape" (though his imprecision in

the use of the latter term is not helpful) and that "... the effects of both must be removed if one wishes to understand the independent interaction of communication and complexity. . . ." We were aware that these might be potentially important factors; and in the prepublication version of our paper, we included controls for both organizational size and number of departments, but these pages fell victim to the editor's requirement for brevity. As we will show when these factors, as well as the number of levels, are considered our observed relationships remain.

But, we do not mean to imply that we are taking Faia's comments at face value. On the contrary, in his example, he makes unrealistic assumptions about organizations; he fails to show how departures from his simple example (which is unrealistic for most organizations, and certainly for those in our study) would affect the number of potential dyads; and he misunderstood that his type and our type of data differ completely.

Let us examine Faia's assumptions. "Consider a formal hierarchy in which (1) there is a single unit at the apex, (2) each additional level (or rank) has a number of units which is twice that of the level immediately above it, (3) all interaction is dyadic, and (4) a given unit can only interact with those units that are *directly* above and *directly* below, or on the same level as itself." First his use of the term "unit" is ambiguous. Evidently, he means a given position filled by a given individual, since if he meant a work unit of several persons, then (a) a given supervisor would have two separate work units reporting to him, neither of which had an intermediate supervisor (at least not in his model) and (b) communications would always be constant between the various positions in any dyad of two work units, an unrealistic assumption. Second, in Faia's example, the supervisor's control span is two positions, i.e. every supervisor has two and only two subordinates. Third, the organization is completely symmetrical. Fourth, any given person has equal access to any person on a level immediately above him, immediately below him, and on the same level, however low his level. This latter qualification is sociologically and organizationally naive. Fifth, no person can interact with persons two levels removed, another untenable assumption for most organizations. Finally, if we work out the size of Faia's organization for organizations with given levels, we get the following:

Number of Levels	Size of the Organization
1	1
2	3
3	7
4	15
5	31
6	63
7	127
8	255
9	511
10	1,023

As this table suggests, Faia presents a model of an extreme and seldom observed type of organizational structure. Faia has made an interesting point by showing that the number of levels might affect the possible *number of dyads* in an organization, given his restrictions, but he failed to go beyond this simplistic example and demonstrate what happens when his assumptions are not met. For example, what would happen under the following circumstances: (1) the span of control varies for supervisors at various levels and on the same level from only one person in some circumstances to many persons in others; (2) the organization is asymmetrical, meaning (a) that if we count from the top we may find only two levels in some branches, three in others, and still six in others; and, of course, the number of units in an organization varies if one counts from the top down or the bottom up. (Cf. Kaufman and Seidman, 1971); and (b) that some lower level persons have one or more subordinates while others on the same level have none; and (3) there is no formal restriction on the number of levels an incumbent of a given position can cross in establishing communications. Such restriction might occur in some organizations, but certainly not in ours. What this means is that Faia's point about the number of potential dyads in an organization is interesting, but his formulas apply to such highly restrictive situations that they are virtually useless. This does not mean, however, that he could not have developed more realistic assumptions and showed us just how the number of dyads are affected when some key parameters are altered.

Of course, all the foregoing assumes that Faia's type of potential communications are "in rough conformity" with our data. This assumption, however, is not correct, further bringing into question Faia's point. Faia's whole discussion is based on comparisons of the *number of pairs* of people (dyads), given the restrictions he has outlined. Our data are based

on the *frequency of communication* among members. Once we had determined the names of all persons with whom a given respondent communicated, we asked:

"How many times in a typical week do you confer with each of these persons?" (p. 865)

and further:

"Two types of information were obtained from these questions: (1) the number of different persons in the organization with whom communications occur and, (2) the frequency of these contacts. To simplify the presentation of the data, *we only include here the latter which is a measure of the intensity of information flow among members of the organization.*" (p. 865; italics added for emphasis).

This information was used to compute standardized rates for each type of communication, i.e. interdepartmental at a higher level, interdepartmental at the same level, etc. That is, we took the total frequency of unscheduled communication for each of these types of communication and for the overall volume of unscheduled communication in the organization and divided the totals by the number of respondents for whom that type of communication was possible. Thus, as our article observed, *the unscheduled communication scores are standardized scores or rates of communication per respondent, meaning that size was taken into consideration in computing the unscheduled communication scores.* However, these scores could logically be affected by the unavailability of some types of dyads.

For example, if a given respondent had no superior, no one on the same level, or no one at a lower level, he could obviously not have such communications. On the other hand, if for each type of communication considered in our article, a respondent had available at least one dyad of a given type, the frequency of communication score for that respondent could vary from zero to infinity, though in reality there would obviously be an upper limit on that frequency.

Now this raises some interesting methodological questions which concerned us in our research. What would happen if no respondent in the organization had the opportunity to have communication of a given type? Then, the organizational score for that type of communication would necessarily be zero. This was never the case, however, since each organization had at least one person for whom communication with someone in each of the six types of communication was possible, though there may still have been no communication for other reasons. Such was not the case in computing level-specific rates, how-

ever, a procedure briefly alluded to in our article. In that case, we computed correlation coefficients only for an organization for which a given type of communication pattern by level was structurally possible. But there is another aspect to this question. What happens when, for example, the lowest-level professional workers in a given work unit have no one below them, while in another work unit the incumbent of a comparable position does? Or, what happens when a division head has no one above him either in the same or another department (since he reports to the organizational head), and, therefore, no intra-departmental or interdepartmental communications with someone at a higher level? If such persons were counted in calculating the frequency of communications, it could artificially deflate an organization's score, especially if a larger number of persons were in such circumstances. For this reason we assigned missing data codes for that specific type of communication pattern and then computed organizational scores on available information.

To repeat, the use of these procedures means that the size of the organization was considered in computing these organizational scores; the unscheduled communications scores are therefore rates of communication of a given type per respondent for whom that communication was possible. Thus, the size of the organization in no way logically affected the organizational scores. This is not to deny, however, that organizational size, as well as configurational characteristics of organization, might have some *sociological* relevance for the rates of communications within organizations. For example, one could hypothesize that having more people in an organization could increase communications per member. Durkheim (1933) argued that increasing size should result in greater interaction, that is, higher social and moral density. Durkheim alleged that size and hence dynamic density were the cause of the progressive movement toward structural differentiation, that is, increased complexity; and one might wonder from this perspective if an organization's increased size might not lead to greater rates of unscheduled communication among organizational members. One could perhaps deduce other hypotheses linking organizational characteristics like height (number of levels in the longest chain) or width (number of departments) to communications. Assuming there is some relationship, we could examine the effects of these variables on the relationships reported in our article.

We did examine the effect of the organization's size (both the raw data which was highly skewed as well as the rank order of or-

ganization size), height (number of levels in the longest chain from the head of the organization to the lowest level staff member), and width on the rate of organizational communications. There was some tendency for larger organizations, those with more levels and those with more departments to have higher rates of communications; but only one of these relationships attained statistical significance; and in some cases the relationships were negative, especially for intradepartmental communication with persons at a lower level. Nevertheless, we computed partial correlations for each relationship reported in our paper controlling one at a time for organizational size, height, and width. The result of this analysis was that almost none of the statistically significant relationships reported in our article was altered; and, in fact, some actually increased with the effects of size and these configurational characteristics statistically removed. Space limitations preclude a discussion of these findings here, but we will supply the tables for them on request. Our substantive conclusions are that organizational size, width, and height do not account for our findings, since our findings are unaltered with the effects of these variables statistically removed.

In summary, we did not ignore the effect of size and configuration on our findings, since we standardized our communication rates by the number of respondents who could possibly have communications of a given type. More important, we computed intensity or frequency of communication measures, a variable which is less affected by the number of dyads. In addition, we statistically controlled for various measures of size and configuration, but found that they had little or no effect on the conclusions we had drawn. The size of an organization and the aspects of configuration presented here are not nearly as important as structural arrangements in determining the intensity of organizational communications.

JERALD HAGE

University of Wisconsin

MICHAEL AIKEN

University of Wisconsin

CORA BAGLEY MARRETT

Western Michigan University

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REPLY TO PROCTOR

Mr. Proctor's reanalysis (December, 1971) of the data from my article on the Srole items (April, 1971) contributes to recognizing the importance of the operation of acquiescence in the items. His treatment shows that the race of the interviewer significantly affects the degree of acquiescence in the interview situation. His analysis was based on an analysis of variance and produced a significance probability of about 1%. When I analyzed this relationship I used chi square which was significant at about the .2 level. Feeling that this result was too weak to state a case for interviewer effect, the table was not included in the article. The fact that Mr. Proctor found a significant relationship and I did not, is an artifact of the type of analysis used. Given the nature of the data, it is more appropriate to use an analysis of variance rather than chi square.

Unfortunately, Mr. Proctor went on to imply that the effect of the interviewer's race was the only aspect of acquiescence demonstrated by the data. He dismissed the effect of wording (original versus obverse) by ignoring two features of my article. First, he seemed not to

notice Table 1 which showed that the differences between these two ways of wording the items was significant at the .001 level (using chi square) *with race of interviewer held constant*. Secondly, he focused on my discussion of percent of variance explained by acquiescence. In order to make a case here he ignored my statement on this type of analysis. I explained that while this type of analysis had been used in similar studies, my data did not meet the requirements for such analysis. Specifically, I said:

The effect of acquiescence was stated (in studies by Bass and Couch and Keniston) in terms of the amount of variance explained in the positively scored scale relative to the negatively scored scale. *This procedure cannot be replicated here* since both scales were not administered to the same individuals. . . . (Carr, 1971:291; emphasis added).

Thus, this aspect of Mr. Proctor's comment is an attack on a "straw man." My analysis of the relationship of the positively scored scale to the negatively scored scale was based on Table 1 (chi square) and not on my incidental discussion of percent variance explained. Furthermore, I clearly acknowledged that my data did not meet the requirements for a statement on percent variance explained.

It would be most unfortunate if readers of Mr. Proctor's comment get the impression that acquiescence is a problem that can be handled by controlling the race of the interviewer. While this is a useful step, the wording direction problem is much more important and more difficult to solve.

LESLIE G. CARR

Guilford College

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ERRATUM

Two errors occurred in Ms. Mary Jackman's rejoinder to Joseph Lopreato and Janet Saltzman Chafetz on page 226 of the April issue. First, Ms. Jackman is affiliated with the University of Wisconsin and not the University of Chicago. Second, Ms. Jackman's reference to Arthur Goldberger should appear at the end of paragraph three. The text should read as follows: ". . . my paper . . . would yield a model with five parameters but only four cells, for which there would be no solution (for further discussion see Goldberger, 1968:112-118)."

ITEMS (Continued)

■ **Lee Freese** is Assistant Professor of Sociology and Director of the Sociological Research Laboratories at Washington State University. His primary interest, reflected in his paper published herein, is the application of scientific philosophy to sociological inquiry. He is also currently working on theoretical and experimental investigations of distributive justice. His paper is commented on by **David**

and **Judith Willer** each of whom has authored a book on the general topic under consideration.

The issue concludes with exchanges concerning three previously published papers and a reply to a comment on a fourth. The debates are lively and doubtless will continue.

J.F.S.



AMERICAN SOCIOLOGICAL REVIEW
Volume 37 Number 5

Goode Force in Human Society

Myrdal Hilly	Hardship and Collective Violence	Yuchtman	Reward and Work-Role Attractiveness
Mark Idrich	Vandalism and Civil Disorders	Tudor	Job Complexity and Powerlessness
Monacich	Theory of Ethnic Antagonism	Thomas et al.	Role-Taking and Power
Isen Hilly	Status Inconsistency and Political Change	Sly	Migration and the Ecological Complex
Osner	The Alien as a Servant of Power	Mayhew et al.	Size and Structural Differentiation

Comments

Notice to Contributors

Preparation of Copy

Manuscripts are evaluated by the editors and other referees. To permit anonymity, attach a cover page giving authorship and institutional affiliation, but provide only the title as means of identification on the manuscript itself. Submit at least two copies, and retain a copy for your own files. Manuscripts are accepted subject to non-substantive editing. Prepare copy as follows:

1. Type all copy—including indented matter, footnotes and references—double-spaced on white standard paper. Lines should not exceed six inches.
2. Type each table on a separate page. Insert a location note, e.g., "Table 2 about here," at the appropriate place in the text.
3. Draw figures on white paper with India ink. Retain the original drawings for direct transmission to the printer, but send copies with the manuscript.
4. Clarify all symbols with words in the margin of the manuscript. Encircle these and other explanatory notes not intended for printing.
5. Include an abstract of 100–150 words.

Format of References in Text

All references to monographs, articles and statistical sources are to be identified at an appropriate point in the text by last name of author, year of publication, and pagination where appropriate, all within parentheses. Footnotes are to be used only for substantive observations, and not for purpose of citation. There is no need for "*Ibid.*," "*op. cit.*," or "*loc. cit.*"; specify subsequent citations of the same source in the same way as the first citation. Examples follow:

1. If author's name is in the text, follow it with year in parentheses. ["... Duncan (1959) has proven that ..."] If author's name is not in the text, insert at an appropriate point the last name and year, separated by comma. ["... some have claimed (cf. Gouldner, 1963) that ..."]
2. Pagination (without "p." or "pp.") follows year of publication, separated by colon. ["... it has been noted (Lipset, 1964:61–64) that ..."] Incorporate within parentheses any brief phrase associated with reference. ["... have claimed that this is so (but see Jones, 1952:99 for a conflicting view.)"]
3. With dual authorship, give both last names; for more than two, use "et al." For institutional authorship, supply minimum identification from the beginning of the complete citation. ["... occupational data (U.S. Bureau of the Census, 1963:117) reveal ..."]
4. If there is more than one reference to the same author and year, distinguish them by use of letters (a, b) attached to year of publication, in text and in reference appendix. ["... as was previously suggested (Levy, 1965a:331) ..."]
5. Enclose a series of references within a single pair of parentheses and separate by semicolons. ["... as many have noted (Johnson, 1942; Perry, 1947; Lindquist, 1948) ..."]

Format of References in Appendix

List all items alphabetically by author and, within author, by year of publication, in an appendix, titled "REFERENCES." Use no italics and no abbreviations. For typing format, see the following examples:

Davis, K.

1963a "The theory of change and response in modern demographic history." *Population Index* 29(October):345–366.

1963b "Social demography." Pp. 204–221 in Bernard Berelson (ed.), *The Behavioral Sciences Today*. New York: Basic Books.

Goode, W. J.

1967 "The protection of the inept." *American Sociological Review* 32(February): 5–19.

Moore, Wilbert E., and Arnold S. Feldman.

1960 *Labor Commitment and Social Change in Developing Areas*. New York: Social Science Research Council.

Sanford, Nevitt (ed.)

1962 *The American College*. New York: Wiley.

PRESIDENTIAL ADDRESS: THE PLACE OF FORCE IN HUMAN SOCIETY *

WILLIAM J. GOODE

*Columbia University**American Sociological Review* 1972, Vol. 37 (October):507-519

HUMAN beings learn early that almost everything they want can be obtained only through each other's cooperation, whether their need or goal is a mother's breast or a love potion, a slave uprising or a new colony, a Nobel Prize or protection against a bullying policeman. Everyone learns, consciously or not, accurately or not, various ways of controlling others for one's own ends. Few people spend much time calculating how to control others, but everyone does so part of the time.

Whatever people do, for whatever goals, will be evaluated by others, who will become thereby more or less willing to cooperate, more or less likely to resist, more or less likely to continue in the relationship. We all interact with others, engage in a flow of transactions with others; and whatever we do has some effect on others, to our advantage or not, in conformity with our wishes or not, and whether or not we will it so.

Consequently, however else we might classify the wide range of human acts, we can also view them as samples of social control processes, i.e., the ways by which people shape the flow of each other's behavior. Almost all can be grouped into a small number of types of social control

forces. One of these is force and force-threat, which I shall henceforth simply call "force"; when I mean only "overt force," I shall so label it.¹

The use of force—and I repeat, I mean by this term both force-threat and overt force—is as ubiquitous as the preachments against it. Whatever else social systems are, they are also force systems. Force constitutes one of the major foundations of all social structures. The processes by which the command of force is expended, exchanged, accumulated, or lost, are universal in social interaction, because force is one of the fundamental resources people and groups need to elicit cooperation, help, and conformity from one another.

Force constitutes one of the four great social control systems in all societies. For two millennia, social analysts have given much attention to three of these—which they have variously termed force, prestige, and wealth—because these underlie all stratification systems.² The fourth, which

¹ For related definitions, see Bierstedt (1950), Etzioni (1961), French and Raven (1959), Parsons (1967), Dahl (1957), Cartwright (1959), Lasswell and Kaplan (1950), and Russell (1938).

² Plato, it will be remembered, distinguished among these three reward systems. Gold people, for example, were not to have wealth, but high prestige; the silver people were to apply force; but the brass and iron people were permitted material wealth. Hobbes' definitions of the bases of power also include these and others (1935:54-62). Cf. also Sumner's (1959:33-34) "hunger, love, vanity, and fear"; and the more widely used Weberian categories of class, status, and party.

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may be called love, friendship, or personal attraction, has less often appeared in grand theory, more often being viewed as an accidental or intrusive set of forces; and no one has ascertained whether its impact is as systematic as the other three.³

Every person and group participates in these control systems, gaining or losing in one or more, investing wisely or foolishly, with few or great results. Over time, some accumulate far more resources, and are better able to obtain whatever pleasures and comforts, dignities and treasures, may be available. Both individuals and groups, families, and corporations, social strata and nations, may enjoy this fate; although of course most do not.⁴

It is necessary to separate each of these four systems from the other. Force, prestige, wealth and love should not be confused under the slippery term, "power," because they differ both concretely and theoretically. That they differ concretely is proved by our own acts in the course of the day: in order to persuade others to do something, we may here decide to threaten a lawsuit, and there praise another for a fine performance; or in still another situation simply offer money. Theoretical analysis will reveal many parallel patterns partly because at the psychological level these factors are experienced as rewards and punishments. A full exposition of their differences would take us far afield, but let me note such theoretically important differences as the following:

1. It is possible to expand almost indefinitely

³ Both Plato and Hobbes took note of this set of forces, as have many others, but none has analyzed it systematically. My colleague Allan A. Silver has for some years been engaged in such a study.

⁴ Talcott Parsons has eloquently put forward the view, in *The Structure of Social Action* and again in briefer form in *The Social System*, that we may conceive of the broad area of human behavior as being shaped by, or analyzable through, the biological, personality, social, and cultural systems. Within the social, we can distinguish several analytically distinct, qualitatively different levels of analysis: political science, dealing with power relations; economics, dealing with the problems of allocating scarce resources to alternative ends; and sociology, dealing with social integration and normative consensus, the integration of social and cultural goals. All three may be viewed as action systems.

the number of one's economic customers, as well as the number of people one rules by force; but one cannot do so for friendship and love.

2. One can make explicit contracts in economic exchanges, enforceable by law, and explicit contracts or exchanges to gain a command of force, often not enforceable by law; but one cannot by contract obtain another's love or friendship, respect or esteem.
3. One can retire from production for the economic market, but one's actions continue to be evaluated by prestige criteria as long as one lives.
4. A social organization that relies on overt force (e.g., a slave system, a prison) differs substantially from one that relies on affection or esteem, with reference to such characteristics as its members', normative commitment to collective goals, their belief that the system is just, their willingness to leave the organization, how well they work if not supervised, and so on.
5. Overt force can expropriate whatever wealth is available, but it cannot extort love; for we cannot love another or cause another to love us by willing it.
6. Overt force can command the external trappings of honor and deference, encomiastic speeches, and the kowtows and obeisances of even unwilling subjects; but it cannot by fiat elicit respect in those subjects' minds.

Although these differences are not here presented systemically, and their implications are not all self-evident, they suggest that it might be theoretically profitable to examine each of these four realms or processes separately. They may well be viewed as distinct elementary processes (Homans, 1961:3-7), which in interaction shape much of our social behavior, but whose patterns and characteristics are best understood if we see them as analytically separable. They may be viewed, in fact, as crude parallels to different types of physical forces whose analysis yields relationships that parallel one another in some ways, forces that cannot yet, even in that most advanced of fields, be analyzed by an integrated field theory.

The systematic study of force as a distinct phenomenon or set of processes has been singularly neglected in the history of social thought, partly because its use has been viewed as deplorable, and therefore occurring mainly in unusual, deviant, criminal, or evil circumstances.

Philosophers and rulers have joined in

deploring force, the former because they correctly perceived that its use implies an unjust demand, the latter because they were convinced that their just reign required no force. After all, subjects, happy with their government, obey because they want to do so. Doubtless, rulers have also guessed that subjects who are persuaded violence is wrong are easier to command.

Against the view that political or social systems do not rest on force except in deviant circumstances is a recurring theme, largely expressed (though not often publicly) by dissidents, women, slaves, children, and the lower orders generally, that force is the foundation of rule. This may seem to be a self-serving view, since they are precisely the most likely victims of force and force-threat. Nevertheless, since neither of these opposing views has been investigated systematically, we should now examine both.

We sociologists have neglected such an examination because we share a long humanistic tradition whose biases deny the ultimate importance of force. We have been taught to believe in man's unconquerable soul, his often mute but always staunch resistance to tyranny. We can observe over the panorama of the past five hundred years in Western nations, and more recently in others, the slow advance of human rights and freedom against monarchy and imperialism, dictatorship and despotism, propaganda and news control. Consequently, we continue to feel, whatever the evidence before our eyes, that somehow human beings do resist force and force threat over time, and that man's will to freedom not only endures, but prevails.

In addition, as sociologists and culturalists, we harbor the bias that force will have little effect on a cultural or social system, that organic life somehow resists or survives the rude threats of a mortar gun, or the blazing pain of napalm. After all, the Chinese swallowed their Mongol and Manchu rulers, the Indonesians pushed out the Dutch, the Indians did not become English, and so on.

However, that sanguine view of history should not be allowed to obscure other facts. For example, Rome did impose its cultural patterns on a wide area; indeed we are all

moulded to some degree by Roman conquests. The Islamic conquest not only shaped the Arab world, but its heritage spread far beyond to Spain and Portugal in the West, and Indonesia in the East. The Iberian conquerors of the New World imposed their culture and social system on that area, undermining or destroying most of the native cultures encountered along the way (Goode, 1961). Both they and the Anglo invaders of the New World imposed their culture and social system on the slaves they imported from Africa.

But leaving aside such grand examples of cultural clash, that sanguine view overlooks the cruel fact that a human being lives but a short time, while such conquests and tyrannies may last for centuries. To say that they are ineffective because they will ultimately fail to impose their social and cultural systems, ignores the truth that temporarily they do so; and "temporarily" may well outlast the life of an individual.

Sociologists have always agreed that all societies must be able to command enough force to kill or restrain those who cannot be controlled by other means. For anyone can at least be stopped by overt force from doing something (Parsons, 1967:265-71). However, sociologists have mainly focused, for the past half-century, on the other means of control. They have explored mainly the impact of the normative order, explaining social action by reference to values and value consensus. They have shown that people obey the rules of the society, group, or family because they have been socialized to believe the rules are right. For example, it is not merely unprofitable to defraud, steal, or kill; it is morally repugnant.

Sociologists have urged, correctly I think, that if people and groups act only with a rational view to their personal profit and with no inner normative controls, then group structures, goals, and controls would be unsupported and a war of all against all would result (Parsons, 1937:89-102).

On the other hand, as I and others have noted, this essentially Lintonian view is partly incorrect, since the observable consensus and normative commitment are not total enough to create the necessary social controls (Goode, 1960a, 1960b). Some opinion is set against almost any normative

rule; people alter their faith when they change social positions; beliefs vary among social strata; and so on. Such observations suggest, at a minimum, that if social control systems are to succeed, they require far more support than internal moral commitment can yield. That support may come from gains and losses in wealth, or from affection or force, but surely not alone from normative commitment.

With that bias toward moral commitment as an explanation, sociologists have felt, along with most social analysts, that force was a weak reed to lean on, and that no regime or society can rest on force alone. Indeed it cannot; but neither must it do so. None ever need try; rulers and conquerors, like teachers and husbands, army officers and employers, always command other resources. We have rejected an empty claim. On the other hand, no sensible person would argue that any society, organization, or group has tried to operate without that most potent weapon.

If this is properly the territory of our sister discipline, political science, they have not bothered to cultivate it; and we shall not be usurpers. Indeed, force has been analyzed mainly as overt force, in criminal violence or the repression of criminal violence, or in war and war games of various kinds.

Much more important, force processes may be so general that we can view their analysis as a distinct task, like that of market processes in economics. That is, they are to be observed or guessed at in all social systems and pervade all continuing social relations to some degree. Thus, no existing intellectual field can claim any better right to study them than any other.

We shall understand these phenomena more clearly if we stop thinking of them as occurring only in despotisms and tyrannies, among criminals, or in war, and instead begin to observe that in any civil society, as e.g., England and Scandinavia, everyone is subject to force. All are engaged in it daily, not alone as victims but as perpetrators as well. Everyone controls some force, and offers some force-threat to others. We are all potentially dangerous to one another. Once we see that, and the pervasiveness of such dynamics in our social life, we

shall be better able to observe their regularities and to measure their effects.

In this view, and by this definition, force is very human, not simply an animal heritage to be eventually overcome. Indeed, force exhibits a very different structure in other animal societies, even among predators, whose killing propensities seem so like our own. The differences can be briefly summarized:⁵

1. Within such societies, animals will not fight to the death, and will not kill when they have vanquished another.
2. The leader or dominant animal does not give orders to subordinates to use force on recalcitrant members.
3. Members do not organize to support or to kill leaders.
4. Weaker animals cannot kill dominant animals.

Given these structural differences, it seems plausible to view human force systems as worthy of study in their own right, not as a leftover from our animal past, or processes that occur when a social system or subsystem has broken down into a jungle chaos.

Concepts and Measurement. Not all the conceptual and methodological problems of this challenging proposal can be adequately discussed here; and some central ones cannot, I believe, be solved in this decade. To touch on a few will, however, clarify the theoretical orientation I urge.

As a first step in conceptual clarification, I suggest eliminating the term "power" from sociological analyses for the next decade. We have too long avoided dealing seriously with force by shoving it under that grand label. The term is both too broad and too narrow, even in its most popular, Weberian definition: i.e., one has power to the degree that one can impose his will even against re-

⁵ The first statement is drawn from ethological reports (Lorenz, 1963; Mowat, 1963; Ardrey, 1966). The second and third are essentially deductions from sociological theory. Of course, animal groups will sometimes kill "strangers," and when caged may become very homicidal. Under some conditions, it is believed, rats will wage "war" in the same building. Elephants are reported to drive out rogue elephants collectively. However, the only animals that typically violate the first rule are special stocks, created by man for killing: fighting bulls, game cocks, pit bulldogs.

sistance. It is too narrow, because even if one *can* impose one's will, the other person or group commands power too. As Machiavelli wisely remarked over four centuries ago, even a victory is not absolute (Machiavelli, 1935:180).

It is too broad because one can impose one's will through all sorts of resources, from bribery or love to the thrust of an army corps. A child may impose its will by a smile, or a respected colleague by a lifted eyebrow.

Still more important, because this allusive, adumbrative label conveys a penumbra of domination by bayonets and coercion, we feel we have dealt with force and force-threat without examining the processes by which, for example, money can purchase a force-threat, or civil societies or economic or family systems may at various points be supported by force.

For similar reasons, we should no longer use the terms "forced compliance" or "coercion" to mean overt or physical force. We feel coerced or forced to comply when we do not wish to do something but feel the other person can make our resistance too costly to bear. Such a lopsided bargaining situation can occur when we are frightened by the anger of our beloved or the possible contempt of our friend, and not alone when we face a hoodlum's knife or a despot's gun.

On the other hand, we may not feel coerced even when we do confront overt force, simply because we believe we can overcome it, or because we are willing to pay that price. Thus, it is not overt force that defines a situation of coercion, but our desire not to obey the command, and our perception that the cost of resistance is too high, whether in physical punishment, the loss of esteem, money, or affection.

Next, we should note that force is empirically and definitionally not limited to punishment or deterrence. We can reward another by backing that person in a conflict with whatever forces we command. Force protects each individual, as e.g., various of our civil rights are, or can be, backed by a court order enforceable by a marshal; or as a wife can be protected by force against the threats of her husband. Force can persuade others to *do* something, not merely to stop doing something. Physical force and its threat can alter another's cost reward

calculations, and thus move him or her toward the action we desire.

All this is true for other resources as well. Love or affection can turn into hate. A salary can be reduced or raised, and profit turned into loss. One can be hired or fired. We can be esteemed for our action, or denigrated. We can experience force, then, as punishment or reward, as we do other important resources that shape our behavior. It is a philosophical bias, not empirical observation, that views force as only punishment and deterrence.

The problem of observation in this realm is acute, because the influence of force is shown precisely by the difficulty of observing it in ordinary civil life. It is not visible because it has been effective. Indeed, a fruitful working hypothesis about social and biological life is that some forces are not less powerful for being less visible, not unimportant because they are called into play less often; some phenomena are striking for their absence. With reference to such factors, we can guess that if they are rare, or rarely visible, it is because other complex and efficacious processes reduce their frequency of appearance, or change them into less threatening forms. We live in a sea of deadly germs, yet remain healthy; our heart rarely fails to adjust to extreme variations in demand for blood flow; killer animals almost never kill members of their own species when they fight. In these and similar cases, it is profitable to ask why an apparently likely outcome does not occur frequently.

In social life, all of us have felt murderous rages, but few of us have murdered (Goode 1969). I have previously argued that love is kept under control, is prevented from appearing as often or as devastatingly as it might by a wide range of social patterns (Goode, 1959). We might be fooled into supposing that the imperious drive of sex is weak, since no society we know was a hotbed of fleshly pleasure; but we know, from our own lives, that numerous controls bar the creation of so happy a state.

Following this working guess, I assert that the importance of force and force-threat in human behavior is richly demonstrated by the rarity of its use. Living as we do in protected environments, we rarely

see anyone manhandled, or hear an overt threat—except from fellow drivers. Nevertheless, every social system is a force system. The two statements do not make a paradox, because every social system contains mechanisms, processes, and patterns whose result and often intention is to prevent the outbreak of overt force. Even in systems with a high overt force component, such as a prison, such outbreaks are infrequent (Sykes, 1958; Cressey, 1966; Glaser, 1967).

Thus a U.S. businessman does not ship arms to a belligerent country because he knows they will be confiscated. Nor does a divorced husband remove his children from the custody of his wife. He knows that she could haul him into court. Indeed, the use of physical force betokens a failure of the force-control processes.

Partly for that reason, we cannot solve our methodological problem by gathering a sample of violent episodes on a frontier. The problem is not merely that cases of overt physical force are relatively rare. It is rather that short of actual killing or jailing, *most cases of even overt force* are instances of force-threat, promises of more to come. In few robberies is the victim physically subdued. Instead, he is threatened. If a bullying policeman shoves a citizen, the latter correctly perceives the action less as force than as a threat of more force, a possible escalation to death. In a country with a settled electoral and judicial tradition, a vote count almost always signals that the loser must step down, under a threat of overt force rarely expressed; and a court order to vacate the office is a command backed by overt force. So well is this understood that in the United States, rife at times with corruption and often violent in its reactions, no defeated major office-holder, with whatever police forces at his or her command, has ever denied his or her office to the winner of an election.

The cases of the business man who obeys government regulations, the husband who accepts a custody arrangement, or the defeated office-holder who vacates his or her office, sharpen the observational problem, while showing how overt force shades into force-threat, and most often is felt as a threat of more force. In instances of force *not* im-

mediate and physical, the individual may be unaware that he or she has been moved by such a threat and in fact not even calculate how far he or she could resist without overt force being applied. Much force appears to people not as a threat of violence, but simply the rules of the game, the obvious reality of the cosmos, to which one must bow unthinkingly.⁶

Moreover, where the use of overt force is almost certain, those who conform may do so for other reasons. For example, we do not walk into an office of General Motors and begin using an idle calculating machine because we believe it is morally wrong, or even because we would be quickly ejected, but more immediately and consciously because our friends would consider us foolish or bizarre.⁷ Or, again, we may refuse to race on the highway, saying we could be carted off to a village jail, when in fact we are much more fearful of a wreck.

The methodological problem is, in short, how to measure the variance accounted for by force and force-threat, in an action or set of actions in which the participant may not have an accurate idea of its impact, and may not have considered force a possibility; or, if aware, may be unwilling to confess it.

But though I do not yet see how such problems of observation and measurement can be solved at present, our theoretical explorations should not await their resolution. Rather, they are more likely to be worked out if we can offer interesting or fruitful propositions that can be tested only when those problems have been solved.

I suggest, therefore, that inquiry into the control processes of force might be worthwhile. It follows from this orientation that all highly industrialized societies are high force systems, by comparison with almost all societies that have gone before them. No dissident groups of substantial size can hide out and engage in armed resistance,

⁶ Holmes' classic comment is relevant to these cases: Law is "... a statement of the circumstances in which the public force will be brought to bear upon men through the courts." (Howe, 1961: vol 2:212).

⁷ I.e., we conform because of the threat of prestige-loss. The case should, however, suggest to the more daring readers a rich field for natural field experimentation.

set up a competing regime, or impose a radically different system on these nations. There are no earls or dukes whose armed and loyal retainers might be a threat to the social or political system.

More significant, their citizens support the social and political systems, and would back them with force were they attacked by dissident groups. This is as true of Holland and Sweden, with their low homicide and crime rates and low frequency of army attacks against civilians, as it is of the United States, with its somewhat higher rates.

By contrast, some Latin American countries, like Bolivia and Colombia, whose governments over the past decade have had to muster soldiers to protect themselves against rebels or competing subgovernments, would be placed much lower on this scale. Some African countries with a high use of overt governmental force, and some countries in the east of India would fall still lower on such a scale. Most bloody despotisms of the past would rank only modestly high on this scale, supported as they were by only a palace guard and their own soldiers. A neat demonstration of the low position of some Latin American countries is the fact that only rarely have the masses risen in armed support of an overthrown regime.⁸

In general, the widespread use of physical force by the regime or dominant groups in a society is probably negatively correlated with that country's position on the scale of the force it commands, because the application of overt force tells us that many people in the society oppose the political or social system, and are unwilling to back it by force, i.e., by informal controls over dissidents.

However, the negative correlation may be a modest one because (1) dominant groups are not necessarily wise, and may use more or less than the optimum amount of physical force for the control they seek, just as parents may use more or less force on their children than would be most effective or rational. Perhaps, for historical reasons, rulers of contemporary Russia use more overt force than necessary; (2)

leaders may command much physical force and force-threat, yet use their overt force to increase still more control; and (3) rulers may, knowing their support or force resources are modest, wisely accept the situation, and refuse to risk their positions by embarking on a course of armed repression. Despite these qualifications, the general negative correlation between a regime's command of force and use of physical force seems evident.

Since this view insists that everyone commands some force, if only because others may protect him or her, and everyone is potentially a threat or a support, we need ask not only how much force the regime or the dominant strata enjoys, but how much counterforce the citizenry can muster. We may imagine a low force regime with a high force citizenry (although I believe this unlikely), or a high force regime with a high-force citizenry. Let us consider the latter.

Although there is some truth in Casanova's comment (which he attributed to a Jesuit) that no nation suffers heavy taxation where its citizenry is armed, and in Machiavelli's comment that a wise man who becomes ruler of a city will arm its citizens to gain their support in exchange for the trust and respect he has thus given, there is little truth in the parallel insight that an armed citizenry is free, or will not tolerate despots.

Harsh and even terrorist rulers have abounded in West and East African kingdoms (Walter, 1969) where almost every adult male was a warrior. The Germans were both free and armed when they clashed with Rome, if Tacitus reported correctly; but the Asiatic steppes and the Islamic countries from the Punjab westerly to Africa have been both armed and victims of autocrats for a thousand years of recorded history. Switzerland and Iceland were both armed and free; but during much of Latin America's chaotic political and social life, its populace has been armed and unfree.

All this follows from our theoretical conception; for a society's total force is not a summation of the individual capacity to kill, but a function of the social organization of force. Consequently, armed but unorganized citizens may be, and indeed

⁸ Walter Goldfrank reminds me of at least one case, that of Benito Juarez in Mexico.

usually are, helpless before the organized might of a tyrant's army and police, even when these are few in number.

The force of a free citizenry is not, then, determined by how many guns they possess, but by their collective determination to resist. This in turn is primarily a function of their faith that their fellow human beings will not let them stand alone against the physical force of a ruler, but will rather risk individual injury to prevent collective injury. Thus, in ranking a citizenry by the force it commands, the question is not so much whether it owns more guns than the government, but whether its members can count on each other for support against encroachments on their freedom. That is the measure of its force. With that capacity, guns can be obtained; without it, guns have historically been of little use.

Thus, we can argue that political and social systems like the Scandinavian and Anglo countries, Iceland, Switzerland, or Holland do command high force since they would be militarily supported by their citizenry against any physical force that sought to impose radical changes against their will, and because technically they command a vast quantity of military and police equipment. But they are also systems in which the citizens themselves command high force, although in most the homicide and crime rates, and the amount of violence used by citizens and the government is low.

The citizens of such countries can and do reject leaders, in the strict sense that they can and do vote them out; and their verdict is backed by force. Again, the verdict is not settled by armed conflict, because those concerned are certain of the outcome.

Our own country falls into this category too, but since some will reject this description, let me be more specific. In face of the cynicism or despair about the trend of our time, feelings which I often share, I assert that: (1) certainly in this country we have suffered encroachments on our liberties over the past three years; (2) but, over a longer span, perhaps a generation or more, our freedoms have been strengthened; (3) whether we look at the past few years or the past generation, the world as a whole has witnessed an expansion of civil freedoms and rights unparalleled throughout history;

(4) in any event, as my analysis shows, freedom is never absolute, to be acquired once and for all. Precisely because it is a relationship, based on collective trust and buttressed by one's fellow citizens' willingness to back it with force, it varies over time, forever being lost or regenerated. What we have lost in the U.S. in the past three years, we shall regain.

The foregoing analysis contains several concrete and theoretical hypotheses, and leads to a technical formulation, which I shall briefly expound. First, Americans will in fact resist with overt force large encroachments on their freedom; and this fact will weigh heavily in the calculations of those who might wish to embark on a program of repression.

Second, whether a businessman, a potential criminal, a pupil, a colony, will risk death, jail, or other forms of physical violence—in short, will violate a rule, or refuse to accept social control—is not a function alone of the firepower or physical strength of such controls and of the visibility of the dereliction, but of whether in either side's evaluations the cost of submission outweighs the threat of force. In short, how much are the alternative actions worth as goals? And their worth is a function of their values.

In older terminology, military analysts spoke of "the will to battle," an imponderable that has more than once outweighed firepower.⁹ In less disciplined form, this differential evaluation of submission can appear as a culture's joy in violence, a theme observable in Plains Indian cultures, Norse sagas, the Song of Roland, and Arab conquests in the early Mohammedan period. Such an evaluation makes the threat of violence less costly, and the cost of submission higher. It therefore affects the calculation of alternative costs in a wide range of control decisions, not alone those affecting liberty.

Thus, any calculation of how successful a control effort by force will be, or how

⁹ Hackett refers to a General Mike West who was in Washington as part of the British government's Liaison Staff, and asked the official in charge of a U.S. Army war game how many men ran away on either side. "None," was the surprised reply. "In that case," said the general, "the result was worthless." All battles are decided by the numbers who run away and by nothing else. (Hackett, 1969:167).

likely a violation of a rule will be, must include the following variables, which have more often been stated in analyses of criminal violations:¹⁰

(1) How observable is the person or group violating the rules?

In civil liberties actions, this visibility is extremely high; but of course in criminal behavior it is likely to be low. In concrete terms, the question is, how likely is the person to be caught?

(2) How much force can the violator command, relative to the amount commanded by those who seek control?

In criminal activities, the police and citizenry command overwhelming force in a public confrontation, and over a longer run, though of course in a specific confrontation the criminal is likely to use counterforce where he believes he can get away with it over a short run, or because other people are not around. With reference to many business regulations, the business man knows he cannot command much force if matters proceed to a genuine confrontation; but he may hope by persuasion, bribery, or friendship to avoid such a conflict.

(3) How high is the evaluation set by either party on winning?

Policemen do not set a high evaluation on stopping gambling or prostitution, but care greatly about punishing anyone who kills a policeman. The South African government sets a high evaluation on repressing blacks; while American Colonials were willing to risk the punishment for treason, for in their evaluation the cost of submitting to the British government was too high.

Intuitively we would guess that the likelihood of resistance, whether in the form of criminal activity, peaceable business calculations about obeying a tax or import law, or a battle in war, is a function of the forces perceived to be available, multiplied by the contingency of being observed (which may be nearly 100% in battle, but .001% in a crime), multiplied by their evaluation of the goal.

Next, from this result must be subtracted the opposing group's or person's weighting of

their forces, multiplied by their evaluation of the goal. Then, the greater the first figure is than the second, the greater the likelihood of disobedience or violation.

Such a formal, hypothetical calculation is of course a statement at the beginning of an action sequence; and the calculations are based on the realities as each perceives them. One or both may be incorrect in these estimates, but they are constantly recalculated at each succeeding step. Thus, a businessman may decide to violate a governmental regulation in the belief that ultimately he can win a court case, or that the government has no great interest in the matter, but change his decision when his lawyer tells him he is wrong. Such a formal calculation then may be stated as follows:

$$(\text{vis})(f)(\text{val}) \stackrel{?}{>} (\text{val})(f)$$

where vis = visibility
 f = perceived available forces, including those which may be elicited from other groups or people
 val = evaluation of the goal

In addition, time discounts may be entered into such formulations, since the amount of force we can muster immediately may be low, as when we are arrested illegally, but ultimately high, as when we obtain an effective lawyer and bring a suit against the offender. People in groups vary substantially in their evaluations of immediate risks versus ultimate victory; and indeed social stability rests to some extent on the fact that most people discount heavily a future victory in a force conflict, and thus will not continue to resist control for long.

In the foregoing analysis, we have concentrated on the types of force structures in which overt force is *less* frequent, i.e., where the steps to be taken before overt force is used are many, and rarely all taken, because one or the other party understands what the eventual outcome will be, and perceives that the patterns protected by potential force are viewed as simply a reflection of the structure of social reality, rather than as an implicit threat. That analysis can also be turned to the other end of the scale, where overt force is more common, and the steps taken before its use are few. An extreme case is of course the political system of terror, whose control patterns differ slightly in structure but greatly in the values of the parameters. Although I have given some attention to this process in unpublished work, time does not permit me to discuss it (Kogon, 1949; Moore, 1954; Walter, 1969; Mendelstam, 1970).

¹⁰ Such a general formulation points up the quasi rationality of the decision process in criminality. With some obvious modifications, and again with fair accurate knowledge on the part of the participant, it can be applied to the dropping out of alum youngsters. On the "rationality" of violence, see Goode (1969) and Stroudbeck and Short (1964). We ignore here the still more fundamental processes, most often treated in radical critiques, by which certain types of acts are defined as crime and thus their perpetrators as criminal.

In the remainder of the paper, I should, however, like to consider a set of cases between these two extremes, by presenting some hypotheses that focus on overt force. These are drawn from more extended analyses, but I shall try to make them as clear as I can in briefer form. That is, I will be laconic, but I hope not cryptic.

1. The less the value consensus within a group, the more efficacious is overt force, as against other resources of reward and punishment, especially prestige or friendship. On the other hand, the less the consensus, the less stable is the rule or command likely to be, if the physical force exercised by a commander or ruler weakens temporarily. Force is more useful, since the group lacks direction, until it can agree on some set of goals; and it will therefore not agree on the goals set by the ruler. On the other hand, because they agree on no alternative set of goals, they will show less resistance to force.

Few will back the ruler's authority if it is challenged, because they do not share his goals or values; and of course no organization of subordinates would arise to support him. On the other hand, it is less likely that subordinates can organize, since they do not have collective goals or shared values, except in their desire to kill or depose the leader. This situation has roughly characterized Latin America, where the masses were typically not united in values, and could only unite to unseat a leader or simply watch while a set of opposing forces engaged in battle, participating as little as possible (Goode, 1973).

2. The greater the value discrepancy between the rulers and the ruled—note that the previous proposition simply referred to a general lack of consensus in a group—the less effort the leaders will invest in a program to socialize the ruled or to use other social control variables such as friendship, money, or prestige. Instead, they will use overt force and strong threats of it. Consequently, in a conquest it is not likely that the ruled will ever willingly be incorporated into the conqueror's system. One of the few exceptions is Rome, but note that Rome gradually extended the advantages of Roman citizenship to an ever increasing set of groups and tribes. By contrast, even over

long periods of time most conquests have failed; in that the conquerors were eventually thrown out, and the conquered not assimilated into their system of values (Goode, 1961).

3. The higher the level of management tasks to be accomplished, the less efficacious is a strong component of overt force. It is less efficient because it requires, relative to the output, a greater or closer supervision if the worker is unwilling; and in turn the worker is almost always unwilling if much overt force is used. There are too many junctures and phases where slackness and sabotage are not visible, in contrast with simpler tasks. Of course, as we know, scientists can be made to work at modest task levels under threat of overt force; but I remind you that even the scientists depicted in Solzhenitzin's *The Inner Circle* were given far greater privileges than ordinary prisoners, and viewed themselves as lucky. Correspondingly a strong component of overt force is more efficient when the task is simpler, and there is no need for long periods of nonsupervision.

The above relationship has been noted by many analysts; but we may now consider an unnoted corollary, that the greater the component of overt force, the less the managerial costs in dollars and time, even though the task must be less sophisticated. In concrete terms, eight or ten guards can be used to supervise the work of a hundred or more agricultural workers. Note in this connection that had the feudal lord of the manor tried to elicit maximum productivity from his serfs, he would also have had to spend far more time and energy in management. He would have had to plan more, and he could not have devoted as much time to falconry, stag hunting, and other courtly pleasures.

4. Because the use of force is generally deprecated, the greater the approximation to using overt force on others, the more likely that those who do so will develop and offer moral justifications for using it. Moreover, experimentation in social psychology has reported that when observers see that an individual has suffered, or has been made the victim in an experiment, they are more likely to deprecate him or her, or feel that he or she has deserved the mis-

fortune (Berkowitz, 1969:98 *et passim*; Berscheid and Walter, 1967).

5. The greater the likelihood that force is used overtly by authorities, the more likely it will be viewed by those authorities and outsiders as necessary and therefore approved. Note that this view is taken when force is used by stronger on weaker parties, as by parents on children; policemen on adolescents; on prisoners as against workers; Marine trainees as against Navy recruits; felons as against misdemeanants; lower classes as against upper classes; blacks as against whites; and so on. It is, then, approved more when used on those who command less overt force or force threat and on those who are thought to require it more.¹¹

6. Those segments of the population on whom the use of overt force by authorities is more approved because other means are thought not to be available or efficacious, may in fact respond to other means; but the society or its ruling groups typically disapprove of such de-escalation. They view the costs of getting conformity as too high to them in prestige, money, or affection, compared with the cost of using force. They may even refuse to consider that their policies are unwise. They believe the cost of using overt force on such groups is relatively low, since they doubt they can gain their allegiance. Presumably, public opinion surveys might test this hypothesis. I am of course suggesting that this widespread "folk sociological belief" may be wrong, but people do act in this fashion.

7. When an individual or group uses overt force on the more advantaged segments of the population, who are generally thought to require no force since they usually cooperate anyway, others will disapprove and withdraw their force-threat backing unless it is shown that the group's violation (typically, some protest) was greater than the conflict situation justified. When authorities use overt force on more advantaged people, they must argue persuasively in its favor, or they will suffer a range of reprisals from

social disapproval to the loss of their jobs.

8. Let me offer a final proposition, focused again on overt force. People lose prestige, i.e., gain disapproval, the more they apply force in situations where acts are thought to be properly guided by values, norms, or shared goals: e.g., sit-ins or robberies. By contrast, those who use force in situations already defined as potentially violent will not lose as much esteem. Thus, one loses more prestige by fighting in a business office than in a lower class bar or a slum. The slave overseer lost more respect for brutality on a long-settled or conquered plantation than on one newly carved from the wilderness. A peace officer in a middle class suburb loses more respect for using violence, than did the frontier sheriff.

This relationship has a corollary: social anger, hostility, or deprecation directed against an individual increases the more that individual proves by resistance that conformity and stability in a social institution are maintained by overt force. That is, by compelling others to use overt force to gain compliance the resisters prove that some people feel little moral commitment to the system, or feel that it is very unjust. An extreme case of course is the violent reaction of slave owners to slave rebellions, but note recent student riots and sit-ins.

FINAL REMARKS

In proposing the systematic study of force as a set of control processes, I have moved from considering large-scale structures to smaller units, and from situations in which overt force is very much in the background, to those where it plays a prominent part. I have done so not merely to meet in part the challenge I have offered, to share the task, but also to show that this general theoretical orientation can generate testable hypotheses that will further our understanding of social structures.

I have offered several conceptual clarifications, notably abandoning the loose term "power," and analytically separating the main realms or types of control processes, i.e., force, prestige, wealth, and affection.

I have pointed out some methodological problems, most importantly, that those who are moved by force may not know that they are. However, not only is that problem al-

¹¹ This attitude has been typical of the "native" Americans toward the latest wave of immigrants, and toward the blacks at all periods of U.S. history.

ready widespread in existing analyses of "power," but we are more likely to try to solve these problems when we see that this approach helps in developing interesting propositions. In accord with that hope, I have tried to develop several such hypotheses.

I should like to close with a few, by no means all, of my prejudices about social structures. Like other people's they may be empirically incorrect in part, yet they may lead to uncovering relationships we might otherwise overlook.

I have already referred to one of these, the working guess that a phenomenon may have important consequences, or may be a crucial element in the social structure, though rarely observed; and indeed its rarity may suggest how important it is that the social processes keep it in check.

Let me suggest a few more. Although I applaud the kindly bias of modern social analysts who argue that rewards are powerful in eliciting desirable behavior from others, while punishments confuse or hinder one's efforts (Church, 1963:369-402; Solomon, 1964:239-53; Boe and Church, 1968; Campbell and Church, 1969), I believe that research will increasingly disclose the powerful effect of punishment in inducing conformity, and even change in values. Correspondingly, although I too deprecate the use of overt force, the historical record shows that regimes with even a fairly high component of overt force can be stable for a long period; and when force patterns are embedded in the system of understandings about reality that people share, overt force can be efficacious indeed. This embeddedness need not be based on moral consensus, or a belief that overt force is just. However, precisely how we come to accept that embeddedness should be a focus of our inquiries.

Next, people on whom overt force is used, or who feel its threat severely, can better judge its efficacy than those whose privileges are supported by it. In line with this prejudice, I suggest that although the imposition of an unwanted system, regime, or control pattern on a population almost always requires overt force, we cannot deduce from observing the use of a low amount of force that all or most subordinates approve the

system, or share its values. Correspondingly, those who are disadvantaged, whose traits or talents are little rewarded, do not and historically have not accepted this arrangement as just and right. If they accept it at times, they do not affirm it, and feel little commitment to it.

Finally, the past decade exhibits a worldwide revolutionary temper, in which the disadvantaged have increasingly come to perceive, and to articulate, that much of the social structure is not only unjust and can be changed, but is also held in place by force, wielded by human beings. Thus, they have come to be less willing to comply with its rules. One might, then, as a challenge to rulers everywhere, point out that we can in fact test their claim that their system is based on justice and on people's allegiance to it, by reducing the use of physical force to buttress it. Thus my final prejudice: that system which requires the least physical force would more closely approximate justice than any we now know.

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HARDSHIP AND COLLECTIVE VIOLENCE IN FRANCE, 1830 TO 1960

DAVID SNYDER AND CHARLES TILLY

University of Michigan

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We challenge the standard argument which treats collective violence as an expression of the dissatisfactions felt by populations experiencing hardship after periods of relative well-being. We propose an alternative explanation in which struggles for political power are the central features. Time-series analyses of year-to-year fluctuations of collective violence in France from 1830 through 1960 fail to yield significant results for a variety of models designed to represent major arguments in the recent literature stressing the effects of short-term hardship. Similar analyses representing the effects of governmental repression and of national political activity yield results corresponding to our expectations. So far we have not been able to incorporate adequate measurements of the other major power-struggle variables into the time-series analysis. But we take the results of this preliminary investigation as a warrant to continue in that direction.

CONSIDERING the scattered, unsystematic and contradictory character of the available evidence, the idea that hardship causes collective violence has gained surprising currency. In recent years few scholars have propounded a simple mechanical relationship between the two. Yet at least one variant of the notion has actually gained adherents; that is the explanation of collective violence (and other forms of protest or rebellion, whether violent or not) as a response to a gap between expectations and achievements. That explanation can easily be made true by definition—for example, by letting the violence itself stand as the evidence of unrealized expectations. It can also be made irrefutable but trivial, simply by authorizing an eternal search for one more gap to account for the violence at hand. There is, however, a credible, weighty and sometimes testable form of the argument which reasons from short-run hardship to protest via the accumulation of individual dissatisfactions.

We challenge the entire line of argument. Men do, indeed, often become angry when other people violate their expectations. Under some conditions short-run hardship does, we concede, precipitate rebellion. But we do not think there is any general connection between collective violence and hardship such that an observer could predict one from the other. We doubt that the diverse events

which go by the names of protest, collective behavior, rebellion and violence have anything more in common than the fact that authorities disapprove of them. And we suppose that the principal, immediate causes of collective violence are political: collective violence results from changes in the relations between groups of men and the major concentrations of coercive power in their environments.

This paper says little about the political analysis of collective violence, and much about hardship. Here we seek merely to show that plausible versions of the expectation-achievement argument fail to explain the year-to-year fluctuation in collective violence over an important span of one country's history, while two eminently political variables—the extent of governmental repression and the degree of national political activity—do provide a partial explanation of that fluctuation. Other reports of our work lay out the political analysis more fully, provide some evidence of its validity, and treat a number of alternative arguments not mentioned here.¹ We do not

¹ This paper reports one part of a continuing study of the effects of large-scale structural change on the character of political conflict in western Europe. The study is being carried on in loose collaboration by a number of scholars at the University of Michigan, the University of Toronto, the Westfälische Wilhelms-Universität (Münster),

think for a moment that this particular investigation disposes of all possible relationships between collective violence and hardship, or that it comes close to establishing the priority of politics. At most we claim no more than to have lodged enough doubts against the expectation-achievement theories of collective violence to recommend a moratorium on their use as explanations until they have received further tests, and to have provided enough support for a political-process theory to justify investing new efforts in its elaboration and verification.

Lest we be suspected of battling straw men, let us mention a few oft-cited statements which follow the line of argument we reject. James C. Davies begins by speaking about revolutions, but soon extends his formulation to a wide variety of violent events: "... revolution is most likely to take place when a prolonged period of rising expectations and rising gratifications is followed by a short period of sharp reversal, during which the gap between expectations and gratifications quickly widens and becomes intolerable. The frustration that develops, when it is intense and widespread in the society, seeks outlets in violent action." (Davies, 1969:547; see also Davies, 1962, 1971). In addition to revolutions in a strict sense of the term, Davies explicitly applies the scheme to draft riots, student protests, the "Black Rebellion of the 1960s" and the Nazi seizure of power. Despite his insistence that the definitive evidence for this argument must come from observations of attitudes, he is willing to use changes in income, education, economic growth, farm productivity and civil rights as indicators of expectations and gratifications. More important

for present purposes, in analyzing the Nazis and several other cases, he offers evidence of rapid economic decline after long expansion to substantiate his argument.

Ivo and Rosalind Feierabend (1966) offer two formulations which are germane to the relationship between hardship and collective violence. First, they argue in essence that the higher the ratio of want formation to want satisfaction, the greater a country's propensity to "instability." In one study, literacy and urbanization represented want formation, GNP, caloric intake, physicians, telephones, newspapers and radios represented want satisfaction, and thirty different domestic conflict measures for 1955 to 1961—ten of them explicitly involving damage to persons or objects, and a number of others implying it—went into the index of instability. Second, they propose that "the faster (the slower) the rate of change in the modernization process within any given society, the higher (the lower) the level of political instability within that society." (Feierabend and Feierabend, 1966:263). In this case, the yearly percent rate of change from 1935 through 1962 in caloric intake, literacy, primary and postprimary education, national income, cost of living, infant mortality, urbanization and radios per thousand population served as indicators of the rapidity of modernization. This time there were two measures of instability: (a) the aggregate index mentioned earlier; (b) the variance of that index over single years from 1955 through 1961. Their formulation differs from Davies', but it clearly permits predictions from fluctuations in economic well-being to levels of collective violence.

Finally, Ted Gurr proposes that "... a psychological variable, relative deprivation, is the basic precondition for civil strife of any kind, and that the more widespread and intense deprivation is among members of a population, the greater is the magnitude of strife in one or another form." (Gurr, 1968: 1104; see also Gurr, 1969, 1970). Gurr's models and measurements are more elaborate than those of Davies or the Feierabends. For present purposes, the essential argument is that both persisting and short-term deprivation have direct, positive effects on the magnitude of civil strife, with

and elsewhere. National Science Foundation grant GS-2674 currently provides the principal financial support for the study. Grants from the Canada Council made earlier phases of the work possible. Recent statements and reports of findings appear in Lees and Tilly, 1972; Lodhi, 1971; Rule and Tilly, 1971; Shorter and Tilly, 1971a, 1971b and 1971c; C. Tilly, 1970a, 1970b, 1972; L. Tilly, 1971a and 1971b; R. Tilly, 1970; R. Tilly and C. Tilly, 1971. We are grateful to Paul Siegel for criticism of an earlier paper by Snyder on the same subject, as well as for spotting a serious error in an earlier draft of this paper, and to Priscilla Cheever, Freddi Greenberg and Glen Jones for assistance in assembling the data.

allowance for the effects of legitimacy, coercive potential and social-structural facilitation. "Persisting deprivation" combines weighted measures of economic discrimination, political discrimination, potential separatism, dependence on private foreign capital, religious cleavages and lack of educational opportunity. "Short-term deprivation" combines declines in foreign trade, inflation, declining rates of growth in GNP, qualitative reports of adverse economic conditions, new restrictions on political participation and representation and new "value-depriving policies of governments." "Magnitude of civil strife" accumulates and weights information about individual conflicts, most of them involving attacks on persons or objects. (We will neglect the complicated measurements of legitimacy, coercive potential and social-structural facilitation, although they raise intriguing and serious methodological problems.) Gurr, too, reasons from short-run hardship to protest via the accumulation of individual dissatisfactions.

Although these investigations are open to serious attack on theoretical, technical and substantive grounds, we will not offer a critical assessment of them here.² Our purpose in sketching the three arguments and their implementation is to provide a rationale for our own choice of models and indexes to represent the line of reasoning we wish to challenge. We have taken one critical segment of the expectation-achievement argument, sought to represent it in terms faithful to the usual formulation of that argument, and tried to test it thoroughly against excellent data concerning year-to-year fluctuations in collective violence within one country over a long period of time. In the research reported here, we have not represented "expectations" in any direct or convincing way. We have, instead, inferred changing expectations from fluctuating "achievements" in a manner similar to that sometimes employed by Gurr, Davies and many other advocates of expectation-

achievement explanations of collective violence.

All the data are yearly aggregate measures for France during the period from 1830 through 1960. Our measure of collective violence is the estimated number of participants in disturbances in continental France as a whole. "Disturbances" are continuous interactions involving at least one group of fifty or more persons in the course of which someone seized or damaged persons or objects over resistance.³ They exclude acts of international war. The disturbances studied consist of every event meeting our criteria detected by trained readers of two national newspapers for each day from 1830 through 1860 and 1930 through 1960, plus each day of a randomly-selected three months per year from 1861 through 1929. Once events qualified in this way, we collected information about them from a wide variety of sources: other newspapers, published court proceedings, annual reviews of politics, French national and departmental archives, secondary historical works, and others. We then recorded a great many characteristics of the disturbances, including estimates of the number of participants, in machine-readable form.⁴

³ We use the commune-day as our building block. France subdivides into about 38,000 communes. If two or more events meeting our criteria occur in the same commune (in Paris, the same quarter) on the same day, with a reasonable presumption of an overlap of at least ten percent of the participants in the smaller event, we treat them as parts of the same disturbance. Similarly, if qualifying events occur in adjacent communes or on consecutive days and there is a presumption of ten percent overlap, they belong to the same disturbance. By these rules, over nine-tenths of all the disturbances fall within a single commune and a single day. Obviously, this procedure fragments large sequences like the revolution of 1848 into a considerable number of disturbances, and excludes the non-violent days of the revolution from consideration.

⁴ We are taking two steps which should make it easier for other scholars to extend, verify and even challenge our own conclusions: (1) extending the time-series files for the 131-year period to include a far larger range of variables; (2) depositing our basic machine-readable files, including the time-series files, with the Inter-University Consortium for Political Research for redistribution; most of the files should be available by early 1973.

² Intelligent reviews of the literature appear in Bienen, 1968; Calhoun, 1970; Lupsha, 1970; Nardin, 1971; Nelson, 1970. Most of the papers cited in note 1 also contain detailed criticisms of different segments of the current literature on political conflict and collective violence.

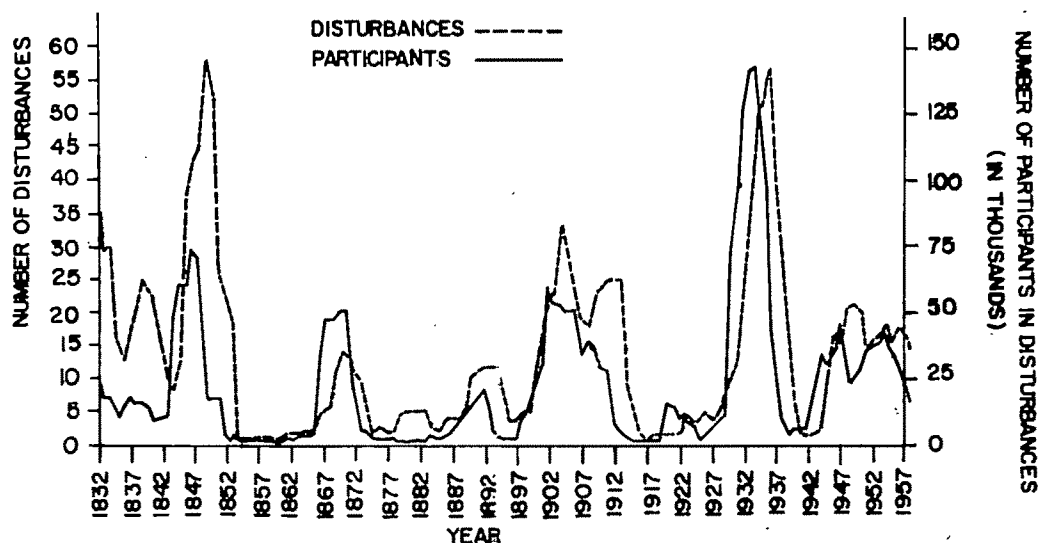


FIGURE 1. DISTURBANCES AND PARTICIPANTS IN DISTURBANCES, 1832-1958 (FIVE-YEAR MOVING AVERAGES).

In order to produce a continuous series over the 131 year period, we have performed two extrapolations which tend to reduce the variance somewhat: (1) we estimated the number of participants in the roughly 6 percent of disturbances where we had insufficient information for a numerical estimate as the mean of all those others in the same year that we were able to estimate numerically; (2) we quadrupled our annual figures for the period from 1861 through 1929, in which we had studied only a quarter of all the months. Altogether, then, we are dealing with 1,989 disturbances and an estimated 3.2 million participants.

As one might expect, the number of disturbances and the number of participants vary greatly from one year to another, but vary closely together.⁵ Figure 1 represents the numbers of disturbances and of participants in five-year moving averages for easy legibility. (The analysis itself, however, uses single-year data.) As the figure shows, very high levels of collective violence came around the revolutions of 1830 and 1848, at the beginning of the twentieth century and in the mid-1930s, while exceptionally low levels prevailed in the 1850s and during the two World Wars. Sometimes the transition came abruptly. In the extreme case,

there were ninety-three disturbances and some 90,600 participants in 1851, followed by two disturbances and an estimated 950 participants in 1852. Without exception the large, abrupt shifts of this kind mark a major rearrangement of the national structure of political power in France. In 1851-1852, the crucial events were Louis Napoleon's *coup d'état*, the widespread but unsuccessful insurrection it incited, and the installation of a police state under the man who was to become Napoleon III.

Our indicators of hardship and well-being are all economic: (1) an index of food prices, (2) an index of prices of manufactured goods, (3) an index of industrial production.⁶ Following the usual practice in expectation-achievement investigations, we take high levels on the first two variables and low levels on the third as indicating

⁵ Over the 131 years, $r=.84$. There were a mean 15.2 disturbances per year, with a standard deviation of 22.3, a mean 24,198 participants per year, with a standard deviation of 45,641.

⁶ The industrial production index grafts the series in the *Annuaire statistique de la France, résumé rétrospectif*, 1966, p. 561 to the series for 1830 to 1898 in Lévy-Leboyer, 1968, thus covering the years 1830-1913, 1918-1938 and 1942-1960. The food index grafts the wholesale price index of the *Annuaire statistique*, p. 373, for 1830-1860 to the retail price index in Singer-Kérel, 1961: 452-453 for the years 1860-1954. The manufactured goods index grafts the *Annuaire statistique* wholesale price index for "industrial products" in 1830-1860 (p. 373) to the retail index for 1860-1940 and 1949-1954 in Singer-Kérel, 1961: 452-453. In each analysis we used the maximum number of years for which there were data for all the variables in the particular model being tested.

hardship for the population as a whole. More precisely, we accept short-run *rises* in prices and *declines* in industrial production as evidence of increasing hardship. The conventional argument, which we adopt for the purposes of this inquiry, is that the population compares current experience with that of the immediate past, and therefore suffers "relative deprivation" when the economy turns down. Davies and Gurr, among many others, use that reasoning explicitly; it also seems consistent with the arguments of the group of cross-sectional studies represented here by the work of the Feierabends.

We make our test of this argument in a roundabout way: not by constructing a single model and rejecting the argument if the model fails, but by testing a *set* of models incorporating the relative deprivation argument. If none of these models fits, we can safely reject (for our data) the usual versions of the argument. Since our data are measured over time, we have employed econometric time-series techniques. Each of these series—the participants in disturbances and the economic indicators—manifests a trend verified by the non-parametric techniques described in Malinvaud (1966: 390–392). We "detrended" the series using the method of first differences ($\Delta X = X_t - X_{t-1}$), for these reasons: (1) only complicated and intuitively meaningless polynomial expressions could account for the trend in these relatively long time-series; (2) detrending using first differences reduces the serial correlation of the residuals; and (3) most importantly, detrending using first differences rather than fitting a polynomial function of time is more faithful to current theories of relative deprivation. By including a polynomial expression for trend, we would in fact be treating as "deprivation years" any years (and only those years) where, for example, observations on the price index were above the predicted value. So, in effect, our deprivation measure would depend on the magnitude (and more importantly, on the sign) of the difference from the trend expression, but not necessarily on the difference from the preceding year (the measure which the theory implies). The method of first differences, by measuring relative deprivation as the change from one year to the next, erases this problem.

Our first model is one which specifies all of the economic deprivation predictors as independent variables, of the following form:

$$\Delta Z_t = b_0 + b_1 \Delta W_t + b_2 \Delta X_t + b_3 \Delta Y_t + u(t);$$

where

$$\Delta Z_t, \dots, \Delta Y_t = Z_t - Z_{t-1}, \dots, Y_t - Y_{t-1}.$$

Z = number of participants in disturbances

W = price of food index

X = price of manufactured goods index

Y = index of industrial production

t = time

u = error or residual term

These letter-variable combinations will remain constant throughout this section.

We compute the regression and correlations:

$$\text{Regression: } \Delta Z_t = 622.5 + 13.09 \Delta W_t + 0.75 \Delta X_t - 626.51 \Delta Y_t + u(t);$$

$$\text{Multiple Correlation Coefficient: } .0270$$

$$F_{8,107} = 0.3539, p < 0.55$$

$$\text{Coefficient of Determination: } .0007$$

$$\text{Standard Error of Estimate: } 57,791.4$$

Neither the analysis of variance for the multiple correlation, nor the coefficients of any of the indicators of deprivation are significantly different from zero. (We use a critical level of $p = .05$ throughout this paper.) Furthermore, the serial correlation of the residuals is quite large (.5304), despite the fact that the first differences method is designed to reduce this correlation. By not being able to account for the serial correlation with this model, we are in effect saying that there are other (non-economic) variables which can explain some of the systematic variation left in the residuals.

A model is more than the sum of its parts. The magnitude (and even the sign) of partial slopes and the significance level often change with the inclusion or exclusion of certain independent variables. Therefore, it is possible that certain of these deprivation indicators could predict collective violence separately. With this in mind, we construct the following models, again using first differences, to ascertain whether or not any single deprivation indicator can predict signifi-

cantly the number of participants in disturbances:

- (1) $\Delta Z_t = b_0' + b_1' \Delta W_t + u'(t)$;
- (2) $\Delta Z_t = b_0'' + b_1'' \Delta X_t + u''(t)$;
- (3) $\Delta Z_t = b_0''' + b_1''' \Delta Y_t + u'''(t)$.

None of the analyses of variance for the multiple correlations in any of these models is significantly different from zero; nor are any of the coefficients significantly different from zero. In all these cases serial correlation of the residuals is relatively high (at least 0.36), indicating once again the existence of variables accounting for systematic variation which we have not yet taken into account.

On another tack, we can learn a great deal about the relationships between the indicators of hardship and our indicator of the magnitude of collective violence by looking at their intercorrelations over time. Based on the writings of the proponents of the hardship-violence linkage, we would predict the following about these relationships: (1) The correlation between changes in the price of food index and changes in the number of participants should be positive and the time lag zero or one year, unless the price of food rises rapidly over a period of years,

in which case there may be a cumulative effect. (2) The lag between changes in the price of manufactured goods and changes in the magnitude of violence might be anywhere from one to five years, since manufactured goods are not such an immediate necessity as food; the expected sign of the correlation is positive. (3) The lag in the correlation between changes in the index of industrial production (as a crude measure of business conditions) and changes in the number of participants can't be predicted in advance.

What in fact do the data look like (see Table 1)?⁷ The correlation between changes in the price of food index and changes in the number of participants in disturbances is virtually zero for all lags/leads within the boundaries considered. Similarly, the correlation between changes in the price of manufactured goods index and changes in the number of participants is close to zero for all lags/leads except the unlagged correlation (lag zero: $r = .1067$).⁸ We have already

⁷ Appendix tables 2 and 3 contain the full set of unlagged correlations for variables used in our analyses.

⁸ Changes in the two price indexes are too closely associated with each other ($r = +.91$) to

Table 1. Intercorrelations Over Time of Economic Deprivation Indicators and Number of Participants in Disturbances.

	Time (Lag/Lead)	ΔW_t Price of Food Index	ΔX_t Price of Man. Goods Index	ΔY_t Index of Indus. Product.
ΔZ_t Number of Participants in Disturbances	8-	.0115	.0016	.0967
	7-	.0008	.0015	.0687
	6-	-.0012	.0005	.0566
	5-	.0066	-.0042	.0292
	4-	-.0160	.0526	.1469
	3-	.0021	-.0211	-.1159
	2-	-.0377	.0014	-.1374
	1-	.0107	-.0496	.0757
	0	-.0270	.1067	-.0703
	1+	-.0165	.0020	-.0103
	2+	-.0640	.0000	.0170
	3+	-.0444	.0019	.0276
	4+	.0164	.0055	-.0348
	5+	.0116	-.0002	-.0433
	6+	-.0043	.0035	.0415
	7+	.0007	-.0012	.0140
	8+	-.0011	.0010	-.0854

Minus signs following entries in the "time" column indicate that the column variables (ΔW_t , ΔX_t , ΔY_t) are lagged on (precede) the participants variable by the designated number of years. A plus sign following the time entry designates a lead.

Data from the years 1830-1912, 1920-1938, 1949-1954: 108 observations.

tested a model which attempted to predict the yearly difference in the number of participants in disturbances from the yearly difference in the price index of manufactured goods at lag zero, and found no significant relationship between the two variables.

The correlation between changes in the index of industrial production and changes in the number of participants is greatest in the expected (negative) direction for a lag of two years ($r = -.1374$). Therefore we construct the following equation, taking account of what appears to be a two year lag in the relationship: $\Delta Z_t = b_0 + b_1 \Delta Y_{t-2} + u(t)$. Even then, neither the analysis of variance for the multiple correlation nor the coefficient of the independent variable is significantly different from zero (for both, $p = .16$).

This last procedure has been quite artificial; where intercorrelations over time seemed to be relatively large and in the direction expected by the relative deprivation theory, we have constructed models which, by choosing the time lag with the highest correlation, have given the economic deprivation indicators the best possible chance of predicting the magnitude of collective violence. And yet, *none* of the models we have tested has yielded a relationship between our economic deprivation indicators and our indicator of the magnitude of collective violence significantly different from zero. The evidence is so clear it hardly needs laying out. The theories of a linkage between relative deprivation and collective violence propounded by Davies, Gurr and many others can safely be rejected for these data.

The alternative theories which we favor treat collective violence as a by-product of struggles for political power. We will not lay out our arguments in detail here, since we are not yet in a position to represent all the variables involved in time-series format. The central ideas are simple. Within any substantial population there is likely to be at least one structure whose members control the major means of coercion effective in that

population; to the extent that the structure is formal and differentiated, we call it a government. Within some specified period, a number of groups varying in coherence and strength collectively apply resources to influence the actions of the government; they are contenders for power.

No group contends for power without having mobilized—having acquired collective control over resources—and mobilization is a relatively rare and difficult process. Some of the contenders have routine means of influencing the government, of influencing each other, and of exerting collective control over which groups belong to their number; we consider them members of the polity. Groups enter and leave a polity through a continuous process of testing: meeting or failing to meet criteria over which the existing members of the polity exercise control. Among those criteria, the ability to mobilize extensive resources—especially manpower and coercive resources—is almost always prominent. Occasionally a revolution fragments the polity for a time; more rarely, the revolution produces a new polity by replacing some or all of the existing members, or by constituting a new government.

Collective violence, then, tends to occur when one group lays claim to a set of resources, and at least one other group resists that claim. Existing members of the polity frequently resist via agencies of the government, especially troops, police and other specialists in coercion. Where governments have substantial force at their disposal, in fact, those specialists ordinarily do the major part of the damaging and seizing which constitutes the collective violence. High levels of governmental repression, however, increase the costs of collective action. They thereby decrease the likelihood that groups will mobilize and make claims which are unacceptable to existing members of the polity. Repression thus reduces the extent of collective violence.

These extremely general statements say nothing about the conditions under which different kinds of groups mobilize and contend for power, what sorts of claims precipitate violent resistance, how the form of government matters, and so on through much of the agenda set for us by the study of struggles for power. They nevertheless

permit us to distinguish between their effects with any confidence. However, dropping one or the other from the equations presented here does not appreciably affect the results of the multiple regressions.

point away from expectation-achievement accounts of collective violence, except to the extent that the gap between expectations and achievements for the population as a whole predicts the extent of mobilization, repression and contention for power. We do not think that extent is great; the analysis we have just reported confirms us in that belief. Our argument gives priority to conditions which facilitate or hinder mobilization, which change the frequency of contested claims, and which govern the extent and character of governmental repression.

In the present analysis, we concentrate on repression and on political activity at the national level. By *repression* we mean governmental activity which raises the cost of collective action by contenders for power. Ideally we should treat repression as a vector summing the effects of all governmental actions over all contenders.⁹ Since many actions (as when one of several competing parties is proscribed) raise the cost of one group's action while lowering the cost for others, we would also want to have a separate vector for each contender.

In general, reinforcements of civil liberties such as assembly, due process and access to the press lower the costs of collective action, and measures like preventive detention or registration of associations raise the costs. In modern European history, one of the more striking facts is the low level of collective violence (as conventionally defined: we are perfectly aware of war and governmental terror) under such repressive regimes as those of the Nazis, the Italian Fascists or Primo de Rivera in Spain. In these cases, it is not so much that repression discourages violence itself, as that repression discourages the collective actions which would, in less repressive times, sometimes end in violent encounters with police, troops or other contenders for power.

⁹ For many purposes, one would want to decompose governmental repression into *capacity* (the facilities for control of collective action available to the government), *propensity* (the extent to which those facilities are used) and *efficiency* (the amount of control achieved per unit employment of those facilities). Here we implicitly multiply the three factors together and look at their product.

If the government permits the organization of public gatherings, gives the press free rein, and guarantees civil liberties, all other things being equal, we expect the magnitude of collective violence should be low. In the extreme case where most forms of association (labor unions, professional organizations, political parties and the like) are outlawed, as was the case during the early part of Louis Napoleon's reign and during the German Occupation in France, then we expect virtually no collective violence. In simple terms, we expect a negative partial relationship between governmental repression and the magnitude of collective violence.

The case for national political activity as a stimulant of collective violence is easier to make. If it were not for the presence of theories emphasizing hardship, relative deprivation and other individual states only tangentially related to politics, we would hardly have to justify the introduction of political struggles into the analysis. According to our argument collective violence should tend to rise, all other things being equal, with the extent of nonviolent struggles for power. (For a given level of mobilization, we would expect a negative relationship between collective actions involving a high risk of violence and those involving a low risk of violence; in the present argument we are lumping the effects of mobilization and contention together, and neglecting the choice among alternative means.) Broadly speaking, collective violence should rise and fall with nonviolent political activity.

We propose a preliminary test of this argument for France by means of a few elementary models encompassing the period from 1830 to 1960. Our indicator of the magnitude of collective violence is again the estimated number of participants in disturbances. We employ three different indicators of repression; each presents some difficulties.

The first is "excess arrests": the deviation of the actual number of arrests of participants in collective violence from the number expected on the basis of the relationship of arrests to participants over the entire 131-year period. We compute it as the five-year moving average of the residuals from the regression of arrests on participants, lagging it so the five years end just prior to the year whose collective violence we are

seeking to explain.¹⁰ Unfortunately, the index only captures the response of the government to actions which produced violence—often as a direct consequence of governmental intervention—and therefore has a degree of circularity built into it.

Our second index is the size of the national budget in the current year, which measures the sheer bulk of government. The trouble with that index, of course, is that while plausibly related to the government's repressive *capacity*, it is quite distant from the governmental action we wish it to represent.

The third measure is more clearly repressive: the man-days of detention in jails during the year. Here one might reasonably object that most of the actions for which people are jailed are not *collective* actions. Man-days of detention is no doubt the best approximation to our fundamental notion of repression, but none of the three measures gets at it very directly.

Our measure of national political activity is elementary. It is a dummy variable (1 = yes, 0 = no) representing the presence or absence of a national election in the year under examination. (We have also worked with the number of cabinet changes per year; there the results are mixed, and the shifts in the form of government make them difficult to interpret.) In a year of national elections, contention for power intensifies, coalitions form or reform, and political organization proliferates. According to our argument such a year, all other things being

equal, should produce relatively high levels of collective violence.

How can we test the argument? Our simplest model would be the following:

$$P_t = b_0 + b_1Q_t + b_2R_t + b_3S_t + b_4T_t + u(t),$$

where:

P = the number of participants per year (square root transformation)

Q = excess arrests (lagged five-year moving average residual of regression: square root of arrests on square root of participants)

R = annual expenditures, national budget, in millions of francs

S = lagged five-year moving average of man-days in jail

T = national election (1 = yes, 0 = no)

u = error term

These letter-variable combinations will remain consistent throughout this section.

Data on man-days in jail are only available for the period 1886–1939. In order to hold on to the years before and after those dates and yet be able to use that relatively good indicator of repression, we carry out two sets of analyses, one covering almost all of the years from 1830 through 1960, the other running from 1886 through 1939. We can detrend both sets of series significantly with a second-order polynomial in time ($t^2 + t$: a simple parabolic function of time). Our full model is therefore the following:

$$P_t = b_0 + b_1Q_t + b_2R_t + b_3S_t + b_4T_t + a_1t + a_2t^2 + u(t).$$

When the regression is computed over the years 1886–1939, we find:

$P_t = 4838.6 + 6.60Q_t - .002R_t - .027S_t + 30.97T_t - 106.58t + .632t^2$, for which $R^2 = .5035$, standard error = 99.065, $F_{6, 38} = 6.4238$ and $p < .0001$. In this case only the coefficients of R (the national budget) and of the two time variables are significant; the coefficient of S (man-days in jail) is in the predicted direction but weak ($p = .175$), that of T (elections) in the predicted direction but weaker still ($p = .395$) and that of Q

¹⁰ At first we simply used the ratio of arrests to participants, and detrended both variables. Paul Siegel pointed out that, even after detrending, participants and arrests/participants are inversely related to each other by definition. Furthermore, since the number of participants is highly auto-correlated from year to year, lagging will not remove the difficulty. Our use of residuals eliminates the first problem, but not the second. Sources for political variables: Arrests come from our own coded descriptions of disturbances. National budget figures: *Annuaire statistique de la France, résumé rétrospectif*, 1966: pp. 484–485. Man-days in jail: *Annuaire statistique de la France, résumé rétrospectif*, 1966: p. 175. National election: compilation from a variety of political sources.

(excess arrests) positive rather than the predicted negative, and weak ($p=.132$). Adding the three economic variables employed earlier—food prices, prices of manufactured goods and industrial production—to the equation produces no significant increase in explanatory power. Dropping excess arrests, on the other hand, reduces the standard error without appreciably affecting the amount of variance explained. The central relationships for the period 1886–1939 appear in the reduced equation:

$$P_t = 6104.3 - .003R_t - .050S_t + 33.3T_t \\ - 126.7t + .728t^2$$

In that equation, $R^2=.4925$, standard error = 95.41, $F_{5,44} = 8.5395$, $p < .0001$. All coefficients except that of T (elections) are highly significant, and the removal of T raises both the standard error and the serial correlation. For this period, in sum, the bulk of the state (as represented by the national budget) and the state's use of its jails appear, as expected, to depress the level of collective violence. The effect of national political activity remains dubious and that of arrests for participation in collective violence quite unreliable.

As we stretch out to the entire period 1830–1960 we lose our measure of detention in jails but hold on to the others. Over that period the basic equation is:

$$P_t = 88.3 + 4.13Q_t - .215R_t + 45.15T_t \\ - 1.92 + .023t^2.$$

Here $R^2=.172$, standard error = 106.22, $F_{5,120} = 4.9729$, $p < .001$. In this case, the one-year serial correlation is .306, a circumstance which lagging the relationships does not affect appreciably; we conclude that there are other variables acting on this system which we have not been able to grasp. In the equation all coefficients are significant save those for t ($p=.14$) and T (elections: $p=.07$). The coefficient of excess arrests is again positive instead of the predicted negative value. This time if we add the three economic variables to the equation we get a slight increase in fit to the observed distribution ($R^2=.211$, S.E. = 104.69, $F_{8,108} = 3.440$, $p < .002$, one-year serial correlation = .292), but only the coefficient of the industrial production variable takes on a significant value ($p=.03$). Even though we appear to have misstated

the effect of arrests for participation in collective violence, we conclude that the basic relationships in this set are the political ones.

To close the circle, let us return to the analysis of year-to-year change with which we began. The first differences method we used in the first part of the analysis makes more sense for the expectation-achievement arguments we were examining there than it does for the political-struggle argument which the later sections of this paper have taken up; we adopt the first differences method here only to permit comparison with the earlier results. The model takes the form:

$$\Delta P_t = b_0 + b_1 \Delta Q_t + b_2 \Delta R_t + b_3 \Delta T_t \\ + b_4 \Delta W_t + b_5 \Delta X_t + b_6 \Delta Y_t + u(t),$$

where:

P = the number of participants per year

Q = excess arrests

R = annual expenditures, national budget

T = national election

W = price of food index

X = price of manufactured goods index

Y = index of industrial production

u = error term

and all changes are measured from the previous year to the current one. Our computations produce the equation:

$$\Delta P_t = -.657 + .656(\Delta Q_t) + .001(\Delta R_t) \\ + 35.4(\Delta T_t) - .028(\Delta W_t) + .086(\Delta X_t) \\ - .004(\Delta Y_t),$$

for which $R^2=.0586$ and $p=.3551$. The only term of the equation which reaches statistical significance is T , which signals the shift to or from a year of national election ($p=.04$); all three political variables have stronger effects than any of the expectation-achievement variables. All things considered, the political explanations of collective violence remain plausible, while the expectation-achievement arguments lose credibility.

We have not, by any means, ruled out all plausible versions of the expectation-achievement explanation. Given the multiplicity and looseness of the arguments scattered through the literature, many further analyses of these and other variables representing expectations and achievements are still possible. We recommend, and intend

to pursue, (1) tests to ascertain whether or not there is a feedback influence from violence to repression and, if so, how it operates; such feedback might account for the ambiguous results of our analysis of "excess arrests"; (2) other, more direct, representations of the "expectations" side of the argument; (3) the study of other variables representing mobilization, repression and the acquisition and loss of power by major contenders; (4) tests of both families of models on other sets of data. Until we test the same models on other times and places, some students of collective violence may prefer to hold on to expectation-achievement reasoning, arguing that France is an exception, that Frenchmen are preternaturally submissive in the face of repression, not to mention insensitive to

hardship. That way of saving the hypothesis would at least be novel. For our part, however, the result of the time-series analysis make us increasingly doubtful that the expectation-achievement arguments concerning collective violence have much explanatory value. The most promising alternatives lie in the analysis of struggles for power.

APPENDIX

TABLES OF UNLAGGED CORRELATIONS

Each coefficient refers to the maximum number of years for which data for both variables were available. The corresponding regression equations were, of course, confined to the years in which data were available for all variables in the equation.

Table 2. Zero-Order Correlations of Indicators of Hardship, Political Activity and Collective Violence, 1930 to 1960.

	Days in Time Jail	Natl. Budget	Elec- tion	Indust. Produc.	Food Prices	Man. Goods Prices	Partics. in Coll. Violence	Arrests in Coll. Violence	Excess Arrests
Time	1.00								
Days in Jail	-.85	1.00							
National Budget	.51	-.58	1.00						
Election	-.17	-.02	-.11	1.00					
Industrial Production	.18	-.19	.02	-.08	1.00				
Food Prices	.20	-.55	.54	-.09	-.04	1.00			
Manufactured Goods Prices	.22	-.52	.49	-.07	-.04	.91	1.00		
Participants in Collective Violence	.15	-.25	.10	.16	-.14	-.14	.09	1.00	
Arrests in Collective Violence	.02	-.28	-.01	.23	-.12	.26	.27	.72	1.00
Excess Arrests	-.12	-.14	-.11	.17	-.04	.24	.30	.00	.69

Table 3. Zero-Order Correlations of One-Year Changes in Indicators

	Food Prices	Man. Goods Prices	Indus. Prod.	Elec- tion	Natl. Budget	Parts. in Viol.	Arrests in Viol.	Days in Jail
Food Prices	1.00							
Manufactured Goods Prices	.91	1.00						
Industrial Production	-.04	-.02	1.00					
Election	.00	.06	.07	1.00				
National Budget	-.11	-.45	.01	-.12	1.00			
Participants in Collective Violence	.04	.07	-.02	.15	.03	1.00		
Arrests in Collective Violence	.12	.21	-.00	.23	-.20	.69	1.00	
Days in Jail	.44	.41	-.39	-.01	-.09	.13	.06	1.00

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PATTERNS OF VANDALISM DURING CIVIL DISORDERS AS AN INDICATOR OF SELECTION OF TARGETS *

RICHARD A. BERK

Northwestern University

HOWARD E. ALDRICH

Cornell University

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Using data from two independent surveys, this paper examines those ghetto retail merchants who are likely to have their business establishments attacked during civil disorders. The findings, unusual in their complementarity, suggest that riot participants select many of their targets and that the pattern of selection reflects a variety of concerns, from personal gain to "pre-political" motivations. Interpretations of these findings undercut theories of collective violence that rest on such concepts as suggestibility, contagion, and "animal spirits."

MOST commentators on recent American civil disorders have observed that ghetto retail establishments were popular targets for rioters. Indeed, some analysts have discussed attacks on ghetto stores as a defining civil disorder characteristic (Quarantelli and Dynes, 1968). Unfortunately, the common focus has not generated common interpretations of the aggression directed at inner city merchants. Some analysts have emphasized exploitative aspects of inner city retailing (Caplovitz, 1967), while others have labeled ghetto merchants as symbols of broader oppression by white society (Carmichael and Hamilton, 1967). Some have seen merchants simply as a convenient target for criminals (Banfield, 1970), and still others have combined several dimensions in more complex approaches (Rossi *et al.*, 1968).

In part such disagreement stems from a lack of information about both the inner city merchants and their customers. Researchers have been trying to understand retailing relationships without data which directly links customers to specific merchants. Thus, inferences about the inter-

actions between merchants and customers typically involve little more than educated speculations.

Data on the relationships between ghetto businessmen and their customers may help resolve the interpretative disputes. For example, Rossi and Berk (1970) found systematic covariation in fifteen large cities between the practices of ghetto retailers and customer criticism of ghetto businesses. Both business practices and customer evaluations are related to the probability that consumer exploitation would become a salient public controversy in a city. The more merchants engage in potentially abrasive practices (e.g., bargaining with customers over prices), the more likely is exploitation to be an issue. The *methodological* point is that when merchants and their customers are matched by city of residence, one can begin to establish links between actions of shopkeepers and actions of consumers.

This paper will examine a type of relationship existing between inner city residents and their local businessmen by linking the attributes, behavior, and attitudes of merchants with the actions of people in the communities they serve. The independent variables are measures of ghetto retailers; the dependent variable is an important gauge of consumer response: attacks directed at local business during civil disorders. Specifically, covariation between those stores which are vandalized and looted and characteristics of businesses will be approached through five kinds of processes that describe different types of merchant-customer interaction leading to riot-associ-

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ated violence. These are "selection" of targets:

1. by the attractiveness of merchandise
2. as retaliation
3. through familiarity with the store
4. through symbolic considerations
5. through proximity to the civil disorder.

The evidence presented will provide some support for all five kinds of violent interactions, but to varying degrees. To the extent that processes 2 and 4 ("retaliation" and "symbolic consideration") are salient, civil disorders are to be interpreted as "pre-political" in nature. (We will elaborate on the meaning of "pre-political" later in the paper.)

Besides providing information on merchant-customer interaction and generally characterizing civil disorders, our approach will have implications for the motives of individual riot participants. Prior analyses, typically lacking in high quality data linking the predispositions of rioters to their actual behavior, have not satisfactorily explained the actions of individuals. (For an excellent review of the literature, see McPhail, 1971.) Case studies of civil disorders, though often rich in detail about rioters (Hayden, 1967; Cohen and Murphy, 1966; Gilbert, 1968), permit cautious generalizations at best. Comparative studies have usually focused on larger units of analysis (Ford and Moore, 1970; Downes, 1970; Spilerman, 1971; Lieberman and Silverman, 1965), using city level inputs on various kinds of relative and absolute deprivations and outputs of occurrence or intensity of rioting. The intervening processes are usually ignored or described through forward and/or backward extrapolation. Studies of riot arrestees and inner city residents (Sears, 1966; Raine, 1967; Fogelson and Hill, 1968; Caplan and Paige, 1968; Paige, 1971), though stressing variables operating on individuals, have been plagued with serious measurement problems, difficulties in establishing causal direction, and an absence of data on how individuals behaved in the group context of a riot.¹

¹ The most recent Paige article (1971) is an instructive example of an attempt to understand the motivations of civil disorder participants where questions about the causal direction of the effects must be raised. Additionally, we question the use

Finally, there will be some implications for collective behavior theory. Although we have no data on crowd process per se, a failing not to be casually dismissed (Berk, 1972), indications that people in violent crowds act instrumentally on a variety of motivations, undermine explanations using "contagion" and "irrationality" (Freud, 1957; Le Bon, 1960), and suggest we re-evaluate the role of "generalized belief" (Smelser, 1962).

COLLECTION OF THE DATA-SAMPLING METHODS

The data in this study are unusual in three respects. First, actions of local residents are linked to the behavior of neighborhood merchants. Second, two studies were conducted with data gathered independently from one another. Thus two *independent* evaluations of the ideas will be presented. Third, Aldrich's data involve measures at two points in time, one before civil disorders struck and one after. The panel approach helps considerably in establishing causal direction and lends unusually strong support to the findings presented.²

THE FIFTEEN CITY DATA

The fifteen city data were collected in the Spring of 1968 as part of a larger study

of an analysis of variance model when the independent variables are not orthogonal.

² At this point some history is in order. Berk's material from fifteen large cities was collected under the auspices of the Kerner Commission and in collaboration with Peter Rossi of Johns Hopkins University and Angus Campbell and Howard Schuman of the Survey Research Center of the University of Michigan. Aldrich's data from three large cities were collected under the auspices of the President's Commission on Law Enforcement and the Administration of Justice in Collaboration with Albert J. Reiss, Jr.

The two sets of data thus were collected completely independently. Berk had completed an analysis similar to that presented in this paper when he met Aldrich, who was working on similar problems (Aldrich, 1969). Aldrich then employed an approach on his data as close to Berk's as feasible, elaborating where he had data on things which Berk did not. Thus, the analyses were not done independently, though the data bases for the analyses were. Additionally, this paper is a product of extensive dialogue, making it a joint enterprise and somewhat different from Berk's earlier work (Berk, 1970).

examining the causes of the different experiences large urban centers were having with civil disorders. Consequently, a sample was chosen that reflected a wide variety of riot events. Five cities (Newark, Detroit, Milwaukee, Boston, Cincinnati) were chosen which had severe civil disorders in 1967. Five cities (Washington, Baltimore, Gary, Pittsburgh, St. Louis) were chosen which had not experienced a serious civil disorder by 1967. Five cities (Brooklyn, Cleveland, Chicago, Philadelphia, San Francisco) were chosen which had experienced serious civil disorder before 1967, but none in 1967.

The sample of retail merchants from the fifteen cities was drawn with the following considerations and limitations: (1) The distribution by kind of store was obtained through quota sampling techniques, with the quotas based on an analysis of regional store distribution data for those regions of the United States in which the cities were located. (2) At least twenty-nine stores were sampled within each city, with the respondent either the owner or manager. (3) There was interest in obtaining a meaningful number of black as well as white merchants. Consequently, a quota of blacks was necessary, because the average number of interviews sought per city was only thirty. The quota for blacks was based on relevant census data and became 26 percent of the sample. (4) The sample was aimed at inner city stores and was drawn from main commercial thoroughfares in neighborhoods at least 60 percent black in the 1960 Census. In most communities the proportion of blacks had increased considerably since 1960. (5) The sampling included those stores in operation during the Spring of 1968, with most of the interviewing completed by April 1. In other words, in cities that had previously had civil disorders, the sample could only include those businesses that had survived, or that had begun operations after the disorders.³

³ It is difficult to estimate what sorts of bias, if any, the civil disorder attrition may introduce. Earlier work by the authors (Berk, 1970; Aldrich and Reiss, 1970) along with comparisons between our samples and those of a sample of Watts merchants suggests that relatively few merchants were put out of business directly because of riot associated vandalism and that our material is reasonably representative.

THE THREE CITY PANEL DATA

The three city data are based on a survey of crime problems of small businesses that was part of a larger study in 1966 of crime and law enforcement in high crime rate areas of Boston, Chicago, and Washington, D.C. Two areas were selected in each city, based on the race and income level of their populations. Only the three areas experiencing civil disorders in 1967-68 are included in this analysis. They are: Roxbury, on the near south side of Boston; Filmore, on the West side of Chicago; and the near Northwest area of Washington, D.C.

Within each of these areas, a probability sample was designed to select one hundred nonresidential establishments with the respondent either the owner or manager. The number of interviews by area in 1966 varied from eighty-six to 102. The total number of interviews was 659. In the summer of 1968, seven of the eight areas were included in a follow-up study. Interviews were obtained for 432 owners or employees of the original 659 establishments in these seven areas, giving a completion rate of 65.5 percent. Excluding the firms no longer in business, however, the completion rate was 80 percent.

In summary, several characteristics of the samples should be emphasized. First, both samples were taken from inner city neighborhoods with predominantly black residents. Second, the fifteen city data are cross-sectional, while the three city are longitudinal. Third, although three cities appear in both studies (Chicago, Boston and Washington, D.C.), interviews in the three city material were conducted in the summers of 1966 and 1968; while in the fifteen city material, interviews were conducted in the early spring of 1968. Consequently the fifteen city merchants spoke about the riots of the summer of 1967, while the three city merchants spoke about the "King" riots of April, 1968. In short, the two sets of data provide two independent opportunities to examine patterns of vandalism.

TYPES OF RIOT-ASSOCIATED VANDALISM

The basic question of patterns of attack during civil disorders can be approached

through two ideal types of processes underlying vandalism.⁴ First, from the level of analysis of the merchant, any one store may have the same probability of being attacked as any other. In other words, the individual and group interaction of potential vandals in relation to a variety of retail operations produce random vandalism. If independent variables emerge that are strongly associated with the propensity to be attacked, the random model becomes implausible. This implies our second kind of underlying process: selection of targets. If one can predict which stores are likely to be victimized choice of targets can be reasonably inferred.

If a pattern of attack emerges, it then becomes appropriate to speculate about variables that might explain the selection of stores. For this analysis, such variables will be broadened into conceptual categories which specify kinds of interaction between merchants and ghetto residents. These five types of "Riot-Associated Vandalism" have important methodological implications. The first four were constructed before the initial wave of data analyses; while the fifth, only relevant to the three city data, was constructed before Aldrich began his analyses. These are not *post hoc* interpretations. We attempt with the data to test various aspects of these "models" and *post hoc* interpretations that do appear are speculations about failures of some indicators to perform as expected.

I. Choice of Targets by Attractiveness of Merchandise

In this kind of interaction store selection is determined by the attractiveness of merchandise. Attractiveness of merchandise is clearly a psychological variable based on individual utilities, but with a reasonable assumption or two about individual motivation, useful generalizations about aggregates can be made. (More about the assumptions later.) We suggest that the more attractive the merchandise, the more likely a store will be vandalized.

⁴ Vandalism is defined broadly to include varied actions: destruction of sales goods, damage to the store, looting, and arson. The reasons for this broad definition will be discussed later.

II. Choice of Targets as Retaliation

Here the store selection is determined by the shopkeeper's actions. If a merchant behaves abrasively, his store will have an increased probability of attack. Abrasive merchant behavior involves not only deceptive selling and merchandising practices (e.g., overcharging and selling inferior merchandise), but discourteous demeanor as well.

There can be little doubt that ghetto residents see the actions of many merchants as distasteful. For example, a survey conducted by Angus Campbell and Howard Schuman in our fifteen cities found large percentages of black ghetto residents with consumer grievances. Fifty-six percent felt that they had been at least sometimes unfairly overcharged in neighborhood stores. Forty-two percent felt that they had at least sometimes been sold spoiled or inferior goods (Campbell and Schuman, 1968:45). These data suggest attitudinal links between abrasive merchant behavior and customer retaliation and consequently make vandalism as retaliation more plausible.

III. Targets as Symbols

This category of interaction involves race *per se*, and suggests that white shopkeepers will have a higher vandalism rate because they symbolize white society. Indeed, Dynes and Quarantelli, after noting that retail stores were one of the major targets during the disorders, argue that "these targets can best be explained by noting that these are the key points of contact between the ghetto and the larger white society. . . . Retail stores symbolize both the lack of economic resources and despised style of life" (Dynes and Quarantelli, 1970:190). From this perspective, the riots are a collective action against the white economic elite. (Of course, white merchants may have a tendency to own stores carrying higher priced merchandise, and they may also have the tendency to act in abrasive ways.)

IV. Choice of Targets by Familiarity

Vandalizing and looting a store requires some knowledge of the target's characteristics. This information may seem especially

important when rioters are worried about the risks involved. Civil disorders are not everyday occurrences, and indeed, may reflect a failure of familiar institutions. Consequently, riot participation involves some anxiety simply because the experience is novel. Additionally, urban riots are clearly dangerous to many participants. If the events surrounding a rioter do not sufficiently suggest risk, the media have typically emphasized the destruction, injury and death. In short, riot participants are exposed to much fear-inducing information.

If some rioters are anxious, one might expect selection of targets where some sense of situational control exists. Part of a feeling of control rests on information. Therefore, we suggest that rioters will be more likely to choose stores with which they are most familiar.

Familiarity may be operating in another manner as well. Though the discussion so far indicates that rational selection of targets will likely occur, the degree of deliberation probably varies greatly. For relatively impulsive rioters, one could expect target choices to reflect stores normally patronized. When the opportunity to vandalize and loot develops, they may immediately select stores that first come to mind, possibly those best known.

V. Choice of Targets by Exposure

A civil disorder is clearly a localized event. Most case studies of mobs emphasize events which draw people to a location and the resulting dense milling. Additionally, geographical movements of rioters are often charted along with occurrences of collective violence at different places over time.⁵ Consequently, we suggest that the chance that a store will be attacked during civil disorders will depend somewhat on its proximity to groups of rioters. The more riot participants nearby, the higher the probability of an assault.

It is important to emphasize that listing the five types of interactions does not imply that one type will be true and the rest false.

⁵ For an excellent example of the use of historical materials to study geographical patterns of collective violence, see Rudé (1964).

Nor does it necessarily mean that any will be completely true or false. Rather, all should be visualized as telling part of the story.⁶ Further, there will be some covariation between types. Black merchants, for example, may often be located in stores carrying undesirable merchandise.⁷

RESULTS AND DISCUSSION

Five of the cities in the fifteen city material were selected for this analysis because they had experienced civil disorders during the summer of 1967. These are labeled "riot cities." In these riot cities, merchants were specifically asked if they had been vandalized during a time period that included a civil disorder.⁸ The other ten cities, those that had had a riot prior to 1967 and those that had not yet had a riot by the summer of 1967, are excluded from our analysis. All cities in the three city data were "riot cities" because merchants were being asked about a time period including April, 1968. Boston, Washington, and Chi-

⁶ Also, there are models that could have some validity but were not included because we lacked data for evaluating them. For example, we postulated a vulnerability model which would predict that those merchants who were seen as "easy marks" would be those vandalized. Unfortunately, we have no measures of vulnerability that could have been sensibly applied. In short, the potential usefulness of other types of interactions was considered in light of available data. If there was no data with which to evaluate a type, it was not included in this paper.

⁷ This raises the question of multicollinearity. Fortunately, all zero order correlations between the independent variables were well below levels at which one begins to worry (Gordon, 1968). Most were around .20 with only one correlation above .50 (.64).

⁸ Potentially problematic was the word "vandalized." We intended it to mean a variety of attacks reflected in looting, burning, and destruction of property. Fortunately, the merchants interpreted the word in this way since follow-up questions indicated that they used the word "vandalized" as a summary concept to include the assortment of events we had in mind. It would have been useful had we been able to disaggregate the components, but that proved impossible. Similar issues were raised with the measurement of which merchants were attacked in the three city data, and they were resolved in the same way. One result of this aggregation is that we are unable to decompose vandalism and examine its components as additional dependent variables.

cago all had civil disorders then, and merchants were specifically asked about riot related damage.

Earlier discussion has implied that the dependent variable, attacks on ghetto retail business, is to be conceptualized as dichotomous. Either a merchant's store is vandalized and/or looted or it is not. Indeed, this is the way the phenomenon will be approached and operationalized. The choice is justified by unsatisfactory data on more elaborate kinds of measures such as the type and amount of damage. We were easily able to ascertain which stores were vandalized, but additional detail proved incomplete and unreliable. Further discussion of methodological problems raised by these potentially useful items and some abbreviated related analysis can be found elsewhere (Berk, 1970; Rossi, Berk, and Eidson, Forthcoming; Aldrich and Reiss, 1969).

Thus our dependent variable is a dichotomy for each data set. For the fifteen city data, the dependent variable indicates whether a merchant answered "yes" when asked if his store had been vandalized during the summer of 1967.⁹ For the three city

data, the dependent variable is slightly more precise, indicating whether a merchant answered "yes" when asked if his business was set on fire, looted, or otherwise damaged.

Results of the regression analysis for the fifteen city data are presented in Table 1. The six independent variables defined in Chart 1 were included because they accounted for statistically significant increments of variance and had a statistically significant raw regression coefficient. A few other variables were used and discarded; some will be discussed. (The number of discarded variables is so small as to virtually eliminate the possibility that our statistically significant findings are a result of the apocryphal five rejections of the null hypothesis at the .05 level out of one hundred tries).

The prediction attempt in the three city data is also successful, as shown in Table 2. Twenty-one percent of the variance is accounted for, indicating that patterns of attack can be discerned in riots occurring in Boston, Chicago, and Washington during

⁹ Berk (1970) has compared patterns of vandalism in the five riot cities with vandalism in the other ten cities. He found that while the variables included in this analysis accounted for a statistically significant and substantially interesting amount of variance in the riot cities, only a small amount of variance is explained by these same

variables in the other cities. Additionally, those few variables that do have some predictive power in the other cities show not only small, but somewhat different effects and raise a number of issues too lengthy to discuss here. The findings show essentially that riot-associated vandalism is qualitatively different from other kinds (cf. Chapter 8 in Rossi *et al.*, Forthcoming).

Table 1. Predictors From Fifteen City Data of Probability of Being Vandalized.

VARIABLE Rank by absolute size of Beta	BETA*	TYPE OF VANDALISM
High average price	.342**	Attractiveness
Awareness of problems Blacks have	.319**	Familiarity and/or retaliation
High Traffic	.283**	Attractiveness and/or familiarity
Offer special services	.171**	Retaliation
Race (White)	.157**	Symbolic
Non-negative attitudes toward Black customers	-.114**	Retaliation
R ² = .23**		
N = 150		

* Beta is standardized regression coefficient.

**P < .05, one-tailed test.

Chart 1. Operationalization of the Variables in the Fifteen City Study.

Race (White) - Race was entered into the regression equations as a dummy variable, zero equal to "black," and one equal to "white."

Awareness - On the questionnaire there were seven items that asked, "Compared to other groups in the city of the same income and education do you think Negroes are about as well off, less well off, or better off with respect to" Seven items were then listed which included such things as "educational opportunities, housing, and treatment by the police." It was felt that, objectively, blacks are, in fact, less well off with regard to these various items and that to say that blacks were "as well off" or "better off" was to be unaware of reality. A higher score indicated that the respondent was more likely to admit that blacks were less well off, and that the respondent, then, was considered more aware.

Special Services - A series of five items asked, "Here are some services that some stores often, sometimes, or never extend to their customers. Do you" The items asked about cashing payroll checks, helping fill out applications and other forms, making contributions to local churches and charities, giving personal advice, and extending credit to people others wouldn't help. A high score indicated that the merchants tended to offer the particular service.

Non-negative Attitudes Towards Black Customers - Four items asked about blacks as customers. The questions began by making a statement about black customers and then asking the merchants how much he agreed with the statement. The statements asked about such characteristics as passing bad checks, shoplifting, and vandalism. Response categories were "agree strongly, agree slightly, disagree slightly, and disagree strongly." The items were scored so that a high score meant a failure to endorse the items and thus a less negative view of customers.

Traffic - This variable was defined as the probability that a given individual in the ghetto would use a particular store in a given period of time. Four raters independently rated each kind of store (zero through 10, with 10 being high) on this probability, and the interrater correlations were very high. The mean score based on these four ratings for each store was then calculated and was the score entered into the regression equation.

Price - This variable, like "traffic," was constructed after the data was collected. Four raters independently scored each store on the average price per item of merchandise. The scores ranged from zero to ten with ten being high. Once again the interrater correlations were very high. As with "traffic," means were calculated for each store based on the scores of the four raters.

Table 2. Predictors From Three City Longitudinal Data on Probability of Being Vandalized.

VARIABLE Rank by absolute size of Beta	BETA	TYPE OF VANDALISM
% customers White	-.341*	Familiarity
Price (same as "price" in 15 city data)	.222*	Attractiveness
Customer services	.190*	Retaliation
Commercial street location	.124*	Exposure
Negative to civil rights	.114*	Retaliation
Race of owner (White)	.114*	Symbolic
$R^2 = .21^*$ $N = 331$		

* $P < .05$, one-tailed test.

Chart 2. Operational Definitions in Three City Study.

Customers % White (1966) - After the interview was completed, interviewers were asked to complete the statement, "The customers observed at the time of the interview were. . ." with one of the following categories: (1) All Negro (coded 0% White), (2) Predominantly Negro, some White (25%), (3) About equal (50%), (4) Predominantly White (75%), (5) All White (100%). This was coded as an interval measure, from 000 to 100.

Customer Services (1968) - Respondents were asked if they offered a variety of services. Counted as services were as follows: (1) Cashing checks for people, (2) Lending or giving money to people from the neighborhoods, (3) Being friendly to customers (as a method of protecting the business from crime --- a dubious "service" but one that fits in with our rather cynical view of customer services in general). These three are given equal weight in the index, with a high score indicating the offering of more special services.

Type Street (1966): Dummy Variable -

"1" is a commercial-retail street

"0" is all other types, i.e., residential, mfg., wholesale, and other

Negative to Civil Rights (1966) - Respondents were asked: "What effect do you think the Civil Rights movement has had on police work?"

"1" indicates a negative response

"0" indicates positive or neutral

Price - Same as Fifteen City Price Variable

Race of Owner (1966) - Same as Fifteen City Race Variable

early April of 1968. In other words, in both the fifteen city data and the three city data, one can explain about a fifth of the variance in the selection of riot targets. Consequently, there is good evidence for patterns of attack; and the notion of random vandalism is severely undermined.¹⁰

If attacks are not random, what variables account for selection? Tables 1 and 2 present the relevant data with the number in each cell the standardized beta coefficient.¹¹

¹⁰ Note that the presence of independent variables that are being treated as interval data coupled with the use of a dichotomous dependent variable eliminates the possibility of obtaining a multiple correlation of 1.0 and additionally acts to decrease the amount of variance that can be "explained." Thus the multiple R²'s are probably conservative estimates (Neter and Maynes, 1970).

¹¹ Though Darlington (1968) and Pugh (1968) argue that more accurate estimates of the independent impact of variables can be obtained by partitioning variance, we are limited to the use of slope measures because of the dichotomous nature of our dependent variable. Neter and Maynes (1970) show how variance measures of effect can be a function of the marginal distributions involved; and consequently, partitioning variance may lead to erroneous conclusions when the dependent variable is dichotomous. For our data, the standardized beta coefficients can be interpreted as the amount of change in the probability of being vandalized that would result from changes

I. *Choice of Targets by Attractiveness of Merchandise*¹²

"Price" in Tables 1 and 2 is the most obvious indicator for the "attractiveness of

in the independent variable. The use of standardized as opposed to raw regression coefficients is justified because of the arbitrary nature of many of the measures used as independent variables. For a good discussion (debate) of these issues, see Cain and Watts (1970) and Coleman (1970).

¹² In the analyses to follow, findings from the three city and fifteen city data will be integrated. Wherever possible identical measures from both sets of material are employed. However, in many cases measures available in one set are not available in the other. Consequently, we often employ measures from the two surveys that are comparable to varying degrees. In addition, we sometimes use variables that are operationalized in only one of the two surveys.

This problem of finding satisfactory indicators to test our types of finding vandalism highlights an important vulnerability in what will follow. The fit between concepts and their indicators will sometimes be less than satisfactory. Though the types of vandalism are all plausible and each indicator included in the analysis is statistically significant, we have several indicators that could operate in more than one type of vandalism. Where this occurs we alert the reader. Note that this does not necessarily invalidate the type of vandalism or the relevant measure. Rather it may reflect a reality in which a particular store characteristic affects potential vandals in several ways.

merchandise" hypothesis. In the fifteen city data, its beta is .342, in the three city data, .222. Stores carrying more expensive merchandise appear to be more popular targets. If one assumes that the price of merchandise affects its desirability, the attractive merchandise motivation gains credibility. This strongly implies that vandals could be acting in part for personal gain, though the reader should not automatically assume it. Possibly more expensive items like television sets are perceived as important symbols of white society, or possibly an effective retaliation against merchants is theft of valuable merchandise.

The impact of traffic in the fifteen city data may also be evidence for the attractiveness of the merchandise as a motivation in vandalism. "Traffic" was defined as the probability that a ghetto resident would use a store in a given time period; thus it may be a measure of merchandise importance. Stores visited more often could contain goods that are more necessary and/or attractive. For example, supermarkets which sell perishables, score high on this scale. (No measure of traffic was used in the three city study.) In this context, the positive beta of .283 may be interpreted as support for the hypothesis that stores with more attractive merchandise are vandalized. (Another interpretation of the effect of traffic will be discussed later.)

In summary, for both sets of data, stores with more expensive merchandise are more likely to be vandalized during civil disorders. In the fifteen city data, stores used more regularly are more likely to be vandalized. Thus, the attractiveness of the merchandise as motivation for vandalism during civil disorders receives substantial though not iron-clad support.

II. *Choice of Targets by Retaliation*

Four approaches were employed to test for retaliatory vandalism, and for various reasons none is completely satisfying. The first approach began by assuming that merchants with hostile and derogatory attitudes about their customers would sometimes behave antagonistically, and consequently become targets for retaliation. To test this in the fifteen city material, an index of mer-

chants' attitudes towards their customers was developed. Interestingly, "non-negative attitudes towards black customers" does have a statistically significant beta; but interpretation is complicated by unclear causal direction.

Although we cannot make as strong a case as one would like, there is some support from the three city data for a retaliatory interpretation of causal direction, i.e., merchants with hostile attitudes behave in ways that provoke retaliation. Table 2 shows a beta of .114 for the variable "negative to civil rights," and the longitudinal data make this an independent variable. Thus, at least one complex of attitudes does seem to generate retaliation. Additionally, more extensive analysis of merchant attitudes in the fifteen cities shows that negative feelings towards customers is associated with hostility towards black grievances and black demands (Berk, 1970). This argues that Berk's "attitudes" and Aldrich's "negative to civil rights" are probably correlated and that "non-negative attitudes towards customers" may indeed be an independent variable.

A second approach assumed that offering credit to ghetto customers created considerable antagonism. Certainly there is evidence that some customers are overcharged for credit and face painful collection procedures (Caplovitz, 1967; Jacob, 1969). Also, the economically unpredictable lives of many ghetto residents probably make buying on credit an agonizing procedure even under the "fairest" of circumstances. In short, it was assumed that stores selling on credit would be riot targets. However, extension of credit used as a dummy variable in the fifteen city data was eliminated from the regression equation. It explained little variance and had a raw regression coefficient of virtually zero. Thus, somewhat surprisingly, our measure of credit practice made no independent difference in patterns of vandalism.

A third attempt to test the retaliation model was based on questionnaire items from the fifteen city material asking merchants to evaluate ("agree strongly," "agree slightly," "disagree strongly," "disagree slightly") three business practices that might produce a poorer dollar value for

customers. The questions concerned bargaining with customers and taking whatever "breaks" one could get, buying "seconds," and pricing to cover the "unusual" overhead in doing business in ghetto neighborhoods. Our goal was to gauge business practices, and in the absence of participant observation techniques, or comparative shopping, indirect questions about general policies of "merchants in neighborhoods like these" were thought the only approach to such sensitive topics. Though, as mentioned earlier, these items are good predictors of the salience of consumer complaints at the city level of analysis (Rossi and Berk, 1970), they were useless in predicting patterns of vandalism.

The fourth approach evolved initially as a *post hoc* explanation for the effect of "offer special services" (e.g., cashing payroll checks, giving personal advice, etc.). In the fifteen city data "offer special services" was originally conceived as a measure of good will; but the beta coefficient was positive, indicating that the more merchants offered special services to their customers, the *more* likely they were to be vandalized. We were inclined to dismiss this finding as provocative but probably due to chance, until the same finding appeared in the three city prediction equation. Stores offering more "customer services" were more likely to be vandalized.¹³

One way to approach these positive betas was to dismiss the notion that offering special services was viewed favorably by the local community. Closer scrutiny of the services suggests that a large component of business self-interest could be operating (cf., Reiss and Aldrich, 1971). The cashing of payroll checks is often made contingent on buying merchandise in the store; often there is a service charge. Helping customers fill out forms and applications is often part of the sales process (for certain businessmen such as car dealers and pawn brokers),

and is clearly required when credit is extended. "Extending credit to people others would not help" certainly may have an altruistic component, but the extension still facilitates sales, and interest is earned on the loan. Even under the most honorable circumstances, at least three of the services can be conceptualized in part not as favors, but as ways to increase sales and profits. Consequently, customers may see the offering of special services as a manipulative device.

The remaining two special services, contributing money to local churches and charities, and giving personal advice, seem clearly altruistic; but merchant altruism may be abrasive to inner city residents. Giving personal advice can be interpreted as a sales pitch or as profit-motivated public relations. Even when personal advice is not directly linked to dollars and cents, the interpersonal processes may not be especially flattering to the recipient. Advice can be patronizing even when well-intentioned. Indeed, given the racial, class, and life style differences between shopkeepers and many ghetto residents, it may take an unusually skilled diplomat to avoid "talking down" to customers.

Effects of giving to local churches and charities may be more difficult to evaluate. Many recipients of merchant charity have little to do with the black community directly (like the Red Cross); and even when causes are clearly tied to a neighborhood, identification of the donor remains problematic. Additionally, the assumption that donors are liked personally is subject to the same questions as the service of giving advice.

In summary, retaliatory interpretation receives modest support not so much because of relatively small betas, or their number, but because of vulnerable interpretations. There is evidence that merchants not favorably disposed to black customers and their aspirations provoke attack. There is also evidence that merchants providing customer services provoke attack. Finally, merchant behavior interpreted as patronizing by customers may also provoke attack. However, all these interpretations have potential problems for which we see no ready solution.

¹³ One reader has suggested that the tendency to offer special services is actually a measure of the size of the store and that size encourages vandalism because of a greater selection of loot. We had a measure of size in the three city data, the number of employees; but this indicator was eliminated from the regression equation.

III. *Targets as Symbols*

If a reason for attacking a business is its symbolic content, one would expect race to be an important predictor of vandalism. However, the zero order correlation between race and being vandalized during riots is only .017 in the fifteen city data and .068 in the three city data, indicating that unless the observers of civil disorders can partial a number of variables in their heads, the relationship between race and vandalism is far from obvious.

Black merchants are often located in high traffic stores in areas of high exposure. They tend to offer special services, and hold attitudes positively correlated with being vandalized. Only when these effects are controlled does race appear of some importance. In the fifteen city material the beta is .157, in the three city data, .114. White merchants do have a higher probability of attack. However, the beta in the fifteen city data is relatively small, ranking fifth in magnitude among the six coefficients. In the three city data, race ranks sixth of six coefficients. Thus, the independent impact of race provides limited support for "targets as symbols."

Given the slight effect of "race" measured through the owner or manager of the store, we approached symbolic targets through another measure of a store's racial identity. One could argue that neighborhoods label a store by the racial mixture of its employees; and in the three city data we used the measure, "percent white employees." However, when this was added to the regression equation for the three city data, the increment to variance explained and the raw regression coefficient failed to reach the .10 level of significance.

Another variable tested for symbolic impact was the religious identity of the store manager. Since the statement "I am sure glad that damn Jew got burned out" (Gilbert, 1968:189) may have been a common sentiment, one could speculate that Jewish merchants would be more likely targets than non-Jews. However, the data from the fifteen cities do not support this view. Though the zero order correlation with "vandalized" is .12, the correlation results from lower vandalism rates for black stores

and a single black Jew in the sample. Holding race constant, the relationship disappears; and indeed, religion adds nothing to the six variable regression. This null finding can be taken at face value or "explained" through poor labeling by ghetto residents. Many merchants may be called "Jew" for reasons other than ethnic background; consequently, there may be a poor fit between a label of "Jewish" and a self-defined Jew.

Absentee ownership may be another symbolic factor to many inner city residents. If symbolism is important, and blacks are able to distinguish absentee from locally owned businesses, one might expect absentee owned businesses to be attacked. However, for many reasons, not the least inaccurate labeling, absentee ownership does not significantly increase the variance explained or have a significant raw regression coefficient in the three city prediction equation.

In summary, the symbolic type of motivation has some support, but only for race, not religion or absentee ownership. Its power to influence selection of targets independently is rather modest, especially when compared to other variables. Thus, the symbolic model has statistically significant but substantively small effects.

IV. *Choice of Targets by Familiarity*

One variable from the three city data relates rather directly to the amount of contact between merchants and potential rioters. The estimated percentage of white customers using a store has a beta of $-.341$: stores more likely to be frequented by blacks are more likely to be attacked. Though the percent of black customers is not a direct measure of contact between inner city residents and ghetto merchants, it is a reasonable inference that stores more heavily patronized by blacks (as compared to whites) will have relatively more black contact and be more familiar to blacks. Stores with which potential rioters are more familiar will be selected as riot targets because: (1) familiar stores will seem safer in times of danger and stress, (2) rioters seeking specific kinds of merchandise will choose stores they know well enough to systematically loot, and (3) more impulsive rioters might

vandalize the first store that comes to mind.

The fifteen city measure "traffic" may be similarly interpreted. Stores with a higher probability of being used by customers in a given time period have a higher probability of being vandalized. Although we discussed "traffic" earlier in the context of attractiveness of merchandise, we noted that an additional and/or alternative interpretation for "traffic" was plausible. High traffic stores may be stores with which customers have more contact and familiarity.

Additional evidence for choice of target through familiarity comes from the fifteen city variable "awareness of problems that blacks have."¹⁴ The beta in riot cities is a relatively high .342, indicating more aware merchants are more likely targets. If increased contact between merchants and customers leads to increased shared knowledge, then high awareness could be interpreted as a measure of contact. In turn, high contact stores are stores with which the black community is most familiar. However, there are at least two weaknesses with this interpretation. First, the correlation between "traffic" and "awareness," although positive, is only .10. An optimistic evaluation of the correlation has both variables measuring the concept of familiarity but picking up different components. Other possibilities are that one is measuring familiarity and the other is not, or that neither is. Second, one could easily postulate a causal direction from being vandalized to gaining a greater awareness of black problems.

Even if we acknowledge "percent white," "traffic," and "awareness" as indicators of contact between merchants and ghetto residents, there are still problems with our interpretations. Familiarity may be an indirect measure of accumulated hostility towards local business. If fundamentally the relations between inner city residents and ghetto merchants are abrasive, then increased contact will foster greater numbers of unacceptable interactions. In other words, if typical customer-merchant interactions are abrasive, then merchants having the most customer contact will be those produc-

ing the greatest amount of bad will. Then during civil disorders, these merchants' stores will be selected. In short, contact measures may reflect retaliatory motivation.

A fairly strong case can be made that stores with greater customer contact are likely targets. However, both the familiarity and retaliation interpretations of high contact rest on assumptions about processes that we have been unable to measure effectively. Thus, we have no data to choose between the two, and both interpretations seem equally plausible. Fortunately, the two approaches are not mutually exclusive and indeed both mechanisms may be operating.

V. Choice of Targets by Exposure

Clearly, the stores closer to intensive rioting are more likely targets. The three city measure of location indicates that businesses located on commercial-retail streets are more often attacked than those located in residential or mixed areas (the beta is .124). Civil disorders, for obvious reasons, were centered on commercial streets in the ghetto.

Though the interpretation of location appears clear initially, we must raise the possibility that location is actually another measure of contact. Stores located on commercial streets may be patronized more heavily. However, the correlation between the percentage of white customers (the only contact measure in the three city material) and location is -.06 indicating that if the former is a measure of contact, the latter probably is not.

SUMMARY AND CONCLUSIONS

The two data sets for this paper were drawn independently, and the plausibility of the findings is strengthened by complementarity revealed in the two statistical analyses. There were no contradictions in findings, and the two data sources supported one another consistently. Consequently, many interpretations which were *post hoc* in Berk's (1970) original analysis take on added credibility.

All five hypotheses about types of vandalism receive some confirmation, but to varying degrees (see Chart 3). The attractiveness of merchandise type is probably the most convincingly demonstrated. The

¹⁴ Note that "awareness" is a measure of knowledge and not necessarily a measure of sympathy or antipathy toward blacks.

Chart 3. Summary of Predictors by Kind of Interpretation.

15 City Data	3 City Data	Type of Vandalism
(5) Race - White +	(6) Race - White +	Symbolic
(1) Higher price + (3) Higher traffic +	(2) Higher price -	Attractiveness
(4) Offer special services + (6) Non-negative attitudes -	(3) Offer customer services + (5) Negative to Civil Rights +	Retaliation
(2) Awareness of Black problems + (3) Traffic +	(1) % White customers-	Familiarity/ Retaliation
(no indicator)	(4) Commercial location +	Exposure

(Number in parentheses is rank in size of Beta -- 1 = High. Sign is direction of effect in probability of being vandalized.)

symbolic type and exposure type, though more easily inferred from the data, have relatively small beta coefficients. The retaliation type and the familiarity type may be vulnerable to alternative explanations.

Several generalizations are suggested by these findings. First, patterns of attack during civil disorders strongly imply choices by some rioters. Civil disorders cannot accurately be described as "irrational" or "mindless" destruction. We are not arguing that the overall events were planned, but rather that individual participants appear to have been selecting many of their targets. One wonders whether detailed data on other forms of mob violence would similarly undercut contagion theories of collective behavior or an interpretation based on "animal spirits."

Second, participants in civil disorders appear motivated by varied concerns, from personal gain to collective protest and retaliation. To characterize the disorders by recourse to one or two monolithic interpretations is fruitless and misrepresents the facts.

As Rudé concluded in his historical analysis of violent crowds, "The crowd may riot because it is hungry or fears to be so, because it has some deep social grievance, because it seeks an immediate reform of the millenium, or because it wants to destroy an enemy or acclaim a 'hero'; but seldom for any single one of these reasons alone." (Rudé, 1964:217)

We do not deny that in some situations a subset of motivations may predominate, nor do we deny a potential role for ideology or "generalized belief." However, the kind and mixture of motivations in violent crowds are empirical questions; and mapping these is necessary before crowds can be understood. In this paper we have tried to map some of the motivations for one kind of crowd behavior, the civil disorder in America's inner cities.

Third, evidence supporting retaliatory vandalism suggests a "pre-political" (not apolitical) perspective for some rioters (Hobsbawm, 1959). Apparently some believe consumer goods are distributed unfairly, label the villains and take collective action against them. Evidence for symbolic motivation implies that some rioters had arrived at a common definition of the "enemy" abstracted beyond the actions of individual merchants. It may be a short step from such interpretations of reality to an ideology and long range goals. To the extent that these two criteria are met, one normally speaks of "political actions." However, even if the amount of conscious ideology and long range planning is limited, selection of merchants by race and abrasive action is evidence of motivations that cannot be dismissed as "apolitical." As Hobsbawm says, "The fact that the mob is a pre-political phenomenon does not mean that it had no implicit or explicit ideas about politics"

(Hobsbawm, 1959:110).¹⁵ It means in essence that its politics lack ideology and a long range program.

In conclusion, it must be emphasized that we have only begun to understand motivations behind the selection of riot targets. There are at least two other kinds of variables effecting patterns of vandalism for which we have no data. First, we have been unable to investigate the group processes operating during civil disorders. Rioting clearly is an interpersonal event and just as one cannot understand change in public opinion without knowing the interpersonal and group context of individuals, one cannot understand riot behavior without knowing about mob process. Second, riots do not occur in a vacuum. However one might choose to draw boundaries around groups of rioters, a number of external forces affect the crowd: for example, the presence of police, width of streets, weather, and time of day. It should come as no surprise then, that we have been unable to account for 75% of the variability in choice of targets.

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A THEORY OF ETHNIC ANTAGONISM: THE SPLIT LABOR MARKET

EDNA BONACICH
University of California, Riverside

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An important source of antagonism between ethnic groups is hypothesized to be a split labor market, i.e. one in which there is a large differential in price of labor for the same occupation. The price of labor is not a response to the race or ethnicity of those entering the labor market. A price differential results from differences in resources and motives which are often correlates of ethnicity. A split labor market produces a three-way conflict between business and the two labor groups, with business seeking to displace higher paid by cheaper labor. Ethnic antagonism can take two forms: exclusion movements and "caste" systems. Both are seen as victories for higher paid labor since they prevent undercutting.

SOCIETIES vary considerably in their degree of ethnic and racial antagonism. Such territories as Brazil, Mexico, and Hawaii are generally acknowledged to be relatively low on this dimension; while South Africa, Australia, and the United

States are considered especially high. Literally hundreds of variables have been adduced to account for these differences, ranging from religions of dominant groups, to whether the groups who migrate are dominant or subordinate, to degrees of dif-

ference in skin color, to an irreducible "tradition" of ethnocentrism. While some writers have attempted to synthesize or systematize some subset of these (e.g. Lieberman, 1961; Mason, 1970; Noel, 1968; Schermerhorn, 1970; van den Berghe, 1966), one is generally struck by the absence of a developed theory accounting for variations in ethnic antagonism.

One approach to this problem is to consider an apparent anomaly, namely that ethnic antagonism has taken two major, seemingly antithetical forms: exclusion movements, and so-called caste systems.¹ An example of the former is the "white Australia" policy; while South Africa's color bar illustrates the latter. The United States has shown both forms, with a racial caste system in the South and exclusion of Asian and "new" immigrants² from the Pacific and eastern seaboard respectively. Apart from manifesting antagonism between ethnic elements, exclusion and caste seem to have little in common. In the one, an effort is made to prevent an ethnically different group from being part of the society. In the other, an ethnically different group is essential to the society: it is an explicit class supporting the entire edifice. The deep south felt it could not survive without its black people; the Pacific coast could not survive with its Japanese. This puzzle may be used as a touchstone for solving the general problem of ethnic antagonism, for to be adequate a theory must be able to explain it.

The theory presented here is, in part, a synthesis of some of the ideas used by Oliver Cox to explain the Japanese-white conflict on the U.S. Pacific coast (Cox, 1948:408-22), and by Marvin Harris to analyze the difference between Brazil and the deep south in rigidity of the "color line" (Harris, 1964:79-94). It stresses the role of a certain kind of economic competition

in the development of ethnic antagonism. Economic factors have, of course, not gone unnoticed, though until recently sociological literature has tended to point them out briefly, then move on to more "irrational" factors (even such works as *The Economics of Discrimination*, Becker, 1957). A resurgence of Marxian analysis (e.g. Blauner, 1969; Reich, 1971) has thrust economic considerations to the fore, but I shall argue that even this approach cannot adequately deal with the problem posed by exclusion movements and caste systems. In addition, both Marxist and non-Marxist writers assume that racial and cultural differences in themselves prompt the development of ethnic antagonism. This theory challenges that assumption, suggesting that economic processes are more fundamental.

No effort is made to prove the accuracy of the following model. Such proof depends on a lengthier exposition. Historical illustrations are presented to support it.

Ethnic Antagonism

"Ethnic" rather than "racial" antagonism was selected as the dependent variable because the former is seen to subsume the latter. Both terms refer to groups defined socially as sharing a common ancestry in which membership is therefore inherited or ascribed, whether or not members are currently physically or culturally distinctive.³ The difference between race and ethnicity lies in the size of the locale from which a group stems, races generally coming from continents, and ethnicities from national sub-sections of continents. In the past the term "race" has been used to refer to both levels, but general usage today has reversed this practice (e.g. Schermerhorn, 1970; Shibutani and Kwan, 1965). Ethnicity has become the generic term.

Another reason for choosing this term is that exclusion attempts and caste-like ar-

¹ I do not wish to enter the debate over the applicability of the term "caste" to race relations (cf. Cox, 1948; Davis, *et al.*, 1941). It is used here only for convenience and implies no particular theoretical bent.

² The term "exclusion" has not usually been applied to immigrant quotas imposed on eastern and southern European immigrants; but such restrictions were, in effect, indistinguishable from the restrictions placed on Japanese immigration.

³ This usage contrasts with that of van den Berghe (1967a:9-10) who reserves the term "ethnic" for groups socially defined by cultural differences. In his definition, ethnicity is not necessarily inherited. I would contend that, while persons of mixed ancestry may be problematic and are often assigned arbitrarily by the societies in which they reside, inheritance is implied in the common application of the word.

rangements are found among national groupings within a racial category. For example, in 1924 whites (Europeans) attempted to exclude whites of different national backgrounds from the United States by setting up stringent immigration quotas.

The term "antagonism" is intended to encompass all levels of intergroup conflict, including ideologies and beliefs (such as racism and prejudice), behaviors (such as discrimination, lynchings, riots), and institutions (such as laws perpetuating segregation). Exclusion movements and caste systems may be seen as the culmination of many pronouncements, actions, and enactments, and are continuously supported by more of the same. "Antagonism" was chosen over terms like prejudice and discrimination because it carries fewer moralistic and theoretical assumptions (see Schermerhorn, 1970:6-9). For example, both of these terms see conflict as emanating primarily from one side: the dominant group. Antagonism allows for the possibility that conflict is mutual; i.e. a product of interaction.

The Split Labor Market

The central hypothesis is that ethnic antagonism first germinates in a labor market split along ethnic lines. To be split, a labor market must contain at least two groups of workers whose price of labor differs for the same work, or would differ if they did the same work. The concept "price of labor" refers to labor's total cost to the employer, including not only wages, but the cost of recruitment, transportation, room and board, education, health care (if the employer must bear these), and the costs of labor unrest. The degree of worker "freedom" does not interfere with this calculus; the cost of a slave can be estimated in the same monetary units as that of a wage earner, from his purchase price, living expenses, policing requirements, and so on.

The price of a group of workers can be roughly calculated in advance and comparisons made even though two groups are not engaged in the same activity at the same time. Thus in 1841 in the colony of New South Wales, the Legislative Council's Committee on Immigration estimated the relative costs of recruiting three groups of

Table 1. Estimated Cost of Three Types of Labor to Be Shepherds in New South Wales, 1841*

	Free Man (White)	Prisoner (White)	Coolie (Indian)
	£ s. d.	£ s. d.	£ s. d.
Rations	16 18 0	13 14 4	9 6 4
Clothing	- - -	3 3 0	1 1 8
Wages	25 0 0	- - -	6 0 0
Passage from India	- - -	- - -	2 0 0
Total per Annum	41 18 0	16 17 4	18 8 0

*From Yarwood (1968:13).

laborers to become shepherds. Table 1 shows their findings. The estimate of free white labor, for example, was based on what it would take to attract these men from competing activities.

Factors Affecting the Initial Price of Labor

Labor markets that are split by the entrance of a new group develop a dynamic which may in turn affect the price of labor. One must therefore distinguish initial from later price determinants. The initial factors can be divided into two broad categories: resources and motives.

1. Resources

Three types of resources are important price determinants. These are:

a. Level of Living, or Economic Resources—The ethnic groups forming the labor market in a contact situation derive from different economic systems, either abroad or within a conquered territory. For members of an ethnic group to be drawn into moving, they must at least raise their wage level. In general, the poorer the economy of the recruits, the less the inducement needed for them to enter the new labor market. Crushing poverty may drive them to sell their labor relatively cheaply. For example, Lind (1968:199) describes the effect of the living level on the wage scale received by immigrant workers to Hawaii:

In every case [of labor importations] the superior opportunities for gaining a liveli-

hood have been broadcast in regions of surplus manpower, transportation facilities have been provided, and finally a monetary return larger than that already received has been offered to the prospective laborer. The monetary inducement has varied considerably, chiefly according to the plane of living of the population being recruited, and the cheapest available labor markets have, of course, been most extensively drawn upon.

Workers need not accept the original wage agreement for long after they have immigrated, since other opportunities may exist; for instance, there may be ample, cheap land available for individual farming. One capitalist device for keeping wages low at least for a time is to bind immigrants to contracts before they leave the old economy. The Indian indenture system, for example, rested on such an arrangement (Gillion, 1962:19-38).

b. Information—Immigrants may be pushed into signing contracts out of ignorance. They may agree to a specific wage in their homeland not knowing the prevailing wage in the new country, or having been beguiled by a false account of life and opportunity there. Williams (1944:11), for example, describes some of the false promises made to draw British and Germans as workers to West Indian sugar plantations before the advent of African slavery. Chinese labor to Australia was similarly "obtained under 'false and specious pretences'" (Willard, 1967:9).

The possibilities for defrauding a population lacking access to the truth are obvious. In general, the more people know about conditions obtaining in the labor market to which they are moving, the better can they protect themselves against disadvantageous wage agreements.

c. Political Resources—By political resources I mean the benefits to a group of organizing. Organization can exist at the level of labor, or it can occur at higher levels, for example, in a government that protects them. These levels are generally related in that a strong government can help organize its emigrants. There are exceptions, however: strong emigrant governments tend not to extend protection to their deported convicts or political exiles; and some highly organized groups, like the Jews

in the United States, have not received protection from the old country.

Governments vary in the degree to which they protect their emigrants. Japan kept close watch over the fate of her nationals who migrated to Hawaii and the Pacific coast; and the British colonial government in India tried to guard against abuses of the indenture system (for example, by refusing to permit Natal to import Indian workers for their sugar plantations until satisfactory terms had been agreed to; cf. Ferguson-Davie, 1952:4-10). In contrast Mexican migrant workers to the United States have received little protection from their government, and African states were unable to intervene on behalf of slaves brought to America. Often the indigenous populations of colonized territories have been politically weak following conquest. Thus African nations in South Africa have been unable to protect their migrant workers in the cities.

In general, the weaker a group politically, the more vulnerable it is to the use of force, hence to an unfavorable wage bargain (or to no wage bargain at all, as with slavery). The price of a labor group varies inversely with the amount of force that can be used against it, which in turn depends on its political resources.

2. Motives

Two motives affect the price of labor, both related to the worker's intention of not remaining permanently in the labor force. Temporary workers tend to cost less than permanent workers for two reasons. First, they are more willing to put up with undesirable work conditions since these need not be endured forever. If they are migrants, this tolerance may extend to the general standard of living. Often migrant temporary workers are males who have left the comforts of home behind and whose employers need not bear the cost of housing and educating their families. Even when families accompany them, such workers tend to be willing to accept a lower standard of living since it is only short term.

Second, temporary workers avoid involvement in lengthy labor disputes. Since they will be in the labor market a short

while, their main concern is immediate employment. They may be willing to undercut wage standards if need be to get a job, and are therefore ripe candidates for strike-breaking. Permanent workers also stand to lose from lengthy conflict, but they hope for benefits to their progeny. If temporary workers are from elsewhere, they have no such interest in future business-labor relations. Altogether, temporary workers have little reason to join the organizations and unions of a permanent work force, and tend not to do so.

a. Fixed or Supplementary Income Goal—Some temporary workers enter the market either to supplement family income, or to work toward a specific purchase. The worker's standard of living does not, therefore, depend on his earnings on the job in question, since his central source of employment or income lies elsewhere. Examples of this phenomenon are to be found throughout Africa:

... the characteristic feature of the labor market in most of Africa has always been the massive circulation of Africans between their villages and paid employment outside. In some places villagers engage in wage-earning seasonally. More commonly today they work for continuous though short-term periods of roughly one to three years, after which they return to the villages. . . . the African villager, the potential migrant into paid employment, has a relatively low, clearly-defined and rigid income goal; he wants money to pay head and hut taxes, to make marriage payments required of prospective bridegrooms, or to purchase some specific consumer durable (a bicycle, a rifle, a sewing machine, a given quantity of clothing or textiles, etc.) (Berg, 1966:116-8).

Such a motive produces the "backward-sloping labor supply function" characteristic of many native peoples in colonized territories. In addition to the general depressing effects on wages of being temporary, this motive leads to a fairly rapid turnover in personnel, making organization more difficult and hindering the development of valuable skills which could be used for bargaining. If wages were to rise, workers would reach their desired income and withdraw more quickly from the market, thereby lessening their chances of developing the political resources necessary to raise their wages further.

b. Fortune Seeking—Many groups, commonly called sojourners (see Siu, 1952), migrate long distances to seek their fortune, with the ultimate intention of improving their position in their homeland. Such was the case with Japanese immigrants on the west coast and Italian immigrants in the east. Such workers stay longer in the labor market, and can develop political resources. However, since they are temporary they have little incentive to join the organizations of the settled population. Instead they tend to create competing organizations composed of people who will play a part in their future in the homeland, i.e. members of the same ethnic group.

Sojourner laborers have at least three features which affect the price of labor: lower wages, longer hours, and convenience to the employer. The Japanese show all three. Millis (1915:45) cites the U.S. Immigration Commission on the question of relative wages:

The Japanese have usually worked for a lower wage than the members of any other race save the Chinese and the Mexican. In the salmon canneries the Chinese have been paid higher wages than the Japanese engaged in the same occupations. In the lumber industry, all races, including the East Indian, have been paid higher wages than the Japanese doing the same kind of work. As section hands and laborers in railway shops they have been paid as much or more than the Mexicans, but as a rule less than the white men of many races.

And so on. The lower wage level of Japanese workers reflects both a lower standard of living, and a desire to get a foothold in the labor market. As Iwata (1962:27) puts it: "Their willingness to accept even lower wages than laborers of other races enabled the Japanese to secure employment readily."

Millis (1915:155) describes a basket factory in Florin, California, where Japanese workers had displaced white female workers because the latter were unwilling to work more than ten hours a day or on weekends. The Japanese, anxious to return to Japan as quickly as possible, were willing to work twelve to fourteen hours per day and on weekends, thereby saving their employers the costs of a special overtime work force.

The Japanese immigrants developed po-

litical resources through a high degree of community organization. This could be used for the convenience of the employer, by solving his recruitment problems, seeing that work got done, and providing workers with board and lodging. In the case of seasonal labor, the Japanese community could provide for members during the off-season by various boarding arrangements and clubs, and by transporting labor to areas of demand (Ichihashi, 1932:172-6; Millis, 1915:44-5). These conveniences saved the employer money.⁴

As the reader may have noted, I have omitted a factor usually considered vital in determining the price of labor, i.e. differences in skills. I would contend, however, that this does not in itself lead to that difference in price for the same work which distinguishes a split labor market. While a skilled worker may be able to get a higher paying job, an unskilled laborer of another ethnicity may be trained to fill that job for the same wage. Skills are only indirectly important in that they can be used to develop political resources, which in turn may lead to a difference in wage level for the same work.

Price of Labor and Ethnicity

Ethnic differences need not always produce a price differential. Thus, if several ethnic groups who are approximately equal in resources and/or goals enter the same economic system, a split labor market will not develop. Alternatively, in a two-group contact situation, if one ethnic group occupies the position of a business elite and has no members in the labor force (or in a class that could easily be pushed into the labor force, e.g. low-capital farmers) then regardless of the other group's price, the labor market will not be split. This statement is a generalization of the point made by Harris (1964) that the critical difference in race relations between the deep south and Brazil was that the former had

a white yeomanry in direct competition with ex-slaves, while the Portuguese only occupied the role of a business elite (plantation owners).

Conversely, a split labor force does not only stem from ethnic differences. For example, prison and female labor have often been cheaper than free male labor in western societies. Prison labor has been cheap because prisoners lack political resources, while women often labor for supplementary incomes cf. Hutchinson, 1968: 59-61; Heneman and Yoder, 1965:543-4).

That initial price discrepancies in labor should ever fall along ethnic lines is a function of two forces. First, the original wage agreement arrived at between business and new labor often takes place in the labor group's point of origin. This is more obviously a feature of immigrant labor, but also occurs within a territory when conquered peoples enter their conquerors' economy. In other words, the wage agreement is often concluded within a national context, these nationalities coming to comprise the ethnic elements of the new labor market. One would thus expect the initial wages of co-nationals to be similar.

Second, nations or peoples that have lived relatively separately from one another are likely to have developed different employment motives and levels of resources (wealth, organization, communication channels.) In other words, the factors that affect the price of labor are likely to differ grossly between nations, even though there may be considerable variation within each nation, and overlap between nations. Color differences in the initial price of labor only seem to be a factor because resources have historically been roughly correlated with color around the world.⁵ When color and resources are not correlated in the "ex-

⁴ Sojourners often use their political resources and low price of labor to enter business for themselves (a process which will be fully analyzed in another paper). This does not remove the split in the labor market, though it makes the conflict more complex.

⁵ It is, of course, no accident that color and resources have been historically related. Poverty among non-white nations has in part resulted from European imperialism. Nevertheless, I would argue that the critical factor in the development of ethnic segmentation in a country is the meeting that occurs in the labor market of that country. The larger economic forces help determine the resources of entering parties, but it is not such forces to which workers respond. Rather they react to the immediate conflicts and threats in their daily lives.

pected" way, then I would predict that price follows resources and motives rather than color.

In sum, the prejudices of business do not determine the price of labor, darker skinned or culturally different persons being paid less because of them. Rather, business tries to pay as little as possible for labor, regardless of ethnicity, and is held in check by the resources and motives of labor groups. Since these often vary by ethnicity, it is common to find ethnically split labor markets.

The Dynamics of Split Labor Markets

In split labor markets, conflict develops between three key classes: business, higher paid labor, and cheaper labor. The chief interests of these classes are as follows:

1. Business or Employers—This class aims at having as cheap and docile a labor force as possible to compete effectively with other businesses. If labor costs are too high (owing to such price determinants as unions), employers may turn to cheaper sources, importing overseas groups or using indigenous conquered populations. In the colony of Queensland in Australia, for example, it was believed that cotton farming would be the most suitable economic enterprise:

However, such plantations (being too large) could not be worked, much less cleared, by their owners; neither could the work be done by European laborers because sufficient numbers of these were not available—while even had there been an adequate supply, the high rates of wages would have been prohibitive. This was a consideration which assumed vast importance when it was realized that cotton would have to be cultivated in Queensland at a considerably lower cost than in the United States in order to compensate for the heavier freights from Queensland—the more distant country from England. It seemed then that there was no possibility of successful competition with America unless the importation of some form of cheap labor was permitted (Moles, 1968:41).

Cheaper labor may be used to create a new industry having substantially lower labor costs than the rest of the labor market, as in Queensland. Or they may be used as strikebreakers or replacements to undercut a labor force trying to improve its bargaining position with business. If cheap

labor is unavailable, business may turn to mechanization, or try to relocate firms in areas of the world where the price of labor is lower.

2. Higher Paid Labor—This class is very threatened by the introduction of cheaper labor into the market, fearing that it will either force them to leave the territory or reduce them to its level. If the labor market is split ethnically, the class antagonism takes the form of ethnic antagonism. It is my contention (following Cox, 1948:411n) that, while much rhetoric of ethnic antagonism concentrates on ethnicity and race, it really in large measure (though probably not entirely) expresses this class conflict.

The group comprising higher paid labor may have two components. First, it may include current employees demanding a greater share of the profits or trying to maintain their position in the face of possible cuts. A second element is the small, independent, entrepreneur, like the subsistence farmer or individual miner. The introduction of cheaper labor into these peoples' line can undermine their position, since the employer of cheaper labor can produce at lower cost. The independent operator is then driven into the labor market. The following sequence occurs in many colonies: settlement by farmers who work their own land, the introduction of intensive farming using cheaper labor, a rise in land value and a consequent displacement of independent farmers. The displaced class may move on (as occurred in many of the West Indies when African slave labor was introduced to raise sugar), but if it remains, it comes to play the role of higher paid labor.

The presence of cheaper labor in areas of the economy where higher paid labor is not currently employed is also threatening to the latter, since the former attract older industries. The importance of potential competition cannot be overstressed. Oftentimes writers assert the irrationality of ethnic antagonism when direct economic competition is not yet in evidence owing to few competitors having entered the labor market, or to competitors having concentrated in a few industries. Thus Daniels (1966:29) belittles the role of trade unions in the Asiatic Exclusion League by describ-

ing one of the major contributors as "an organization whose members, like most trade unionists in California, were never faced with job competition from Japanese." It does not take direct competition for members of a higher priced labor group to see the possible threat to their well-being, and to try to prevent its materializing. If they have reason to believe many more low-priced workers are likely to follow an initial "insignificant trickle" (as Daniels, 1966: , describes the Japanese immigration, failing to mention that it was insignificant precisely because a larger anticipated flow had been thwarted, and diverted to Brazil), or if they see a large concentration of cheaper labor in a few industries which could easily be used to undercut them in their own, they will attempt to forestall undercutting.

Lest you think this fear misguided, take note that, when business could override the interests of more expensive labor, the latter have indeed been displaced or undercut. In British Guiana the local labor force, composed mainly of African ex-slaves, called a series of strikes in 1842 and 1847 against planters' attempts to reduce their wages. Plantation owners responded by using public funds to import over 50,000 cheaper East Indian indentured workers (Despres, 1969). A similar situation obtained in Mississippi, where Chinese were brought in to undercut freed blacks. Loewen (1971: 23) describes the thinking of white landowners: "the 'Chinaman' would not only himself supply a cheaper and less troublesome work force but in addition his presence as a threatening alternative would intimidate the Negro into resuming his former docile behavior." Such displacement has occurred not only to non-white more expensive labor, but, as the effects of slavery in the West Indies show, to whites by white capitalists.

3. Cheaper Labor—The employer uses this class partly to undermine the position of more expensive labor, through strikebreaking and undercutting. The forces that make the cheaper group cost less permit this to occur. In other words, either they lack the resources to resist an offer or use of force by business, or they seek a quick return to another economic and social base.

With the possible exception of sojourners, cheaper labor does not intentionally undermine more expensive labor; it is paradoxically its weakness that makes it so threatening, for business can more thoroughly control it. Cox makes this point (1948: 417-8) in analyzing why Pacific coast white and Asian workers could not unite in a coalition against business:

... the first generation of Asiatic workers is ordinarily very much under the control of labor contractors and employers, hence it is easier for the employer to frustrate any plans for their organization. Clearly this cultural bar helped antagonize white workers against the Asiatics. The latter were conceived of as being in alliance with the employer. It would probably have taken two or three generations before, say, the East Indian low-caste worker on the Coast became sufficiently Americanized to adjust easily to the policies and aims of organized labor.

Ethnic antagonism is specifically produced by the competition that arises from a price differential. An oversupply of equal-priced labor does not produce such antagonism, though it too threatens people with the loss of their job. However, hiring practices will not necessarily fall along ethnic lines, there being no advantage to the employer in hiring workers of one or another ethnicity. All workingmen are on the same footing, competing for scarce jobs (cf. Blalock, 1967:84-92, who uses this model of labor competition). When one ethnic group is decidedly cheaper than another (i.e. when the labor market is split) the higher paid worker faces more than the loss of his job; he faces the possibility that the wage standard in all jobs will be undermined by cheaper labor.

Victory for More Expensive Labor

If an expensive labor group is strong enough (strength generally depending on the same factors that influence price), they may be able to resist being displaced. Both exclusion and caste systems represent such victories for higher paid labor.

1. Exclusion—Exclusion movements generally occur when the majority of a cheaper labor group resides outside a given territory but desires to enter it (often at the request of business groups). The exclusion move-

ment tries to prevent the physical presence of cheaper labor in the employment area, thereby preserving a non-split, higher priced labor market.

There are many examples of exclusion attempts around the world. In Australia, for instance, a group of white workers was able to prevent capitalists from importing cheaper labor from India, China, Japan and the Pacific Islands. Attempts at importation were met with strikes, boycotts, petitions and deputations (Willard, 1967:51-7). Ultimately, organized white labor pressed for strong exclusion measures, and vigilantly ensured their enforcement. As Yarwood (1964:151-2) puts it: "A comparison of the records of various governments during our period [1896-1923] leaves no doubt as to the special role of the Labour Party as the guardian of the ports." In other words, a white Australia policy (i.e. the exclusion of Asian and Polynesian immigrants) appears to have sprung from a conflict of interests between employers who wanted to import cheap labor, and a labor force sufficiently organized to ward off such a move.

California's treatment of Chinese and Japanese labor is another example of exclusion. A socialist, Cameron H. King, Jr., articulates the threatened labor group's position:

Unskilled labor has felt this competition [from the Japanese] for some time being compelled to relinquish job after job to the low standard of living it could not endure. The unskilled laborers are largely unorganized and voiceless. But as the tide rises it is reaching the skilled laborers and the small merchants. These are neither unorganized nor voiceless, and viewing the menace to their livelihood they loudly demand protection of their material interests. We of the Pacific Coast certainly know that exclusion is an effective solution. In the seventh decade of the nineteenth century the problem arose of the immigration of Chinese laborers. The Republican and Democratic parties failed to give heed to the necessities of the situation and the Workingman's party arose and swept the state with the campaign cry of "The Chinese must go." Then the two old parties woke up and have since realized that to hold the labor vote they must stand for Asiatic exclusion (King 1908:665-6).

King wrote this around the time of the Gentlemen's Agreement, an arrangement of the U.S. and Japanese governments to pre-

vent further immigration of Japanese labor to the Pacific Coast (Bailey, 1934). The Agreement was aimed specifically at labor and not other Japanese immigrants, suggesting that economic and not racial factors were at issue.

Exclusion movements clearly serve the interests of higher paid labor. Its standards are protected, while the capitalist class is deprived of cheaper labor.

2. Caste—If cheaper labor is present in the market, and cannot be excluded, then higher paid labor will resort to a caste arrangement, which depends on exclusiveness rather than exclusion. Caste is essentially an aristocracy of labor (a term borrowed from Lenin, e.g. 1964), in which higher paid labor deals with the undercutting potential of cheaper labor by excluding them from certain types of work. The higher paid group controls certain jobs exclusively and gets paid at one scale of wages, while the cheaper group is restricted to another set of jobs and is paid at a lower scale. The labor market split is submerged because the differentially priced workers ideally never occupy the same position.

Ethnically distinct cheaper groups (as opposed to women, for example, who face a caste arrangement in many Western societies) may reside in a territory for two reasons: either they were indigenous or they were imported early in capitalist-labor relations, when the higher paid group could not prevent the move. Two outstanding examples of labor aristocracies based on ethnicity are South Africa, where cheaper labor was primarily indigenous, and the U.S. south, where they were imported as slaves.

Unlike exclusion movements, caste systems retain the underlying reality of a price differential, for if a member of the subordinate group were to occupy the same position as a member of the stronger labor group he would be paid less. Hence, caste systems tend to become rigid and vigilant, developing an elaborate battery of laws, customs and beliefs aimed to prevent undercutting. The victory has three facets. First, the higher paid group tries to ensure its power in relation to business by monopolizing the acquisition of certain essential skills,

thereby ensuring the effectiveness of strike action, or by controlling such important resources as purchasing power. Second, it tries to prevent the immediate use of cheaper labor as undercutters and strike-breakers by denying them access to general education thereby making their training as quick replacements more difficult, or by ensuring through such devices as "influx control" that the cheaper group will retain a base in their traditional economies. The latter move ensures a backward-sloping labor supply function (cf. Berg, 1966) undesirable to business. Third, it tries to weaken the cheaper group politically, to prevent their pushing for those resources that would make them useful as undercutters. In other words, the solution to the devastating potential of weak, cheap labor is, paradoxically, to weaken them further, until it is no longer in business' immediate interest to use them as replacements.

South Africa is perhaps the most extreme modern example of an ethnic caste system. A split labor market first appeared there in the mining industry. With the discovery of diamonds in 1869, a white working class emerged.⁶ At first individual whites did the searching, but, as with the displacement of small farms by plantations, they were displaced by consolidated, high-capital operations, and became employees of the latter (Doxey, 1961:18). It was this class together with imported skilled miners from Cornwall (lured to Africa by high wages) which fought the capitalists over the use of African labor. Africans were cheaper because they came to the mines with a fixed income goal (e.g. the price of a rifle) and did not view the mines as their main source of livelihood. By contrast, European workers remained in the mines and devel-

oped organizations to further their interests.

Clearly, it would have been to the advantage of businessmen, once they knew the skills involved, to train Africans to replace the white miners at a fraction of the cost; but this did not happen. The mining companies accepted a labor aristocracy, not out of ethnic solidarity with the white workers but:

(as was to be the case throughout the later history of mining) they had little or no choice because of the collective strength of the white miners. . . . The pattern which was to emerge was that of the Europeans showing every sign of preparedness to use their collective strength to ensure their exclusive supremacy in the labour market. Gradually the concept of trade unionism, and, for that matter, of socialism, became accepted in the minds of the European artisans as the means of maintaining their own position against non-white inroads (Doxey, 1961:23-4).

The final showdown between mine owners and white workers occurred in the 1920's when the owners tried to substitute cheaper non-white labor for white labor in certain semi-skilled occupations. This move precipitated the "Rand Revolt," a general strike of white workers on the Witwatersrand, countered by the calling in of troops and the declaration of martial law. The result was a coalition between Afrikaner nationalists (predominantly workers and small-scale farmers being pushed off the land by larger, British owned farms) and the English-speaking Labor Party (Van der Horst, 1965:117-8). The Revolt "showed the lengths to which white labour was prepared to go to defend its privileged position. From that time on, mine managements have never directly challenged the colour-bar in the mining industry" (Van der Horst, 1965:118).

The legislative history of much of South Africa (and of the post-bellum deep south) consists in attempts by higher priced white labor to ward off undercutting by cheaper groups, and to entrench its exclusive control of certain jobs.⁷

⁶ Such a split was not found in the early Cape Colony, where business was one ethnicity—white, and labor another—non-white. Actually in neither case was the ethnic composition simple or homogeneous; but the important fact is that, among the laborers, who included so-called Hottentots, and slaves from Madagascar, Moçambique and the East Indies (cf. van den Berghe, 1967b:14), no element was significantly more expensive. The early Cape is thus structurally similar, in terms of the variables I consider important, to countries like Brazil and Mexico. And it is also noted for its "softened" tone of race relations as reflected in such practices as intermarriage.

⁷ Ethnically based labor aristocracies are much less sensitive about cheap labor in any form than are systems that do not arrive at this resolution because they are protected from it. Thus, Sutherland and Cressey (1970:561-2) report that both the deep south and South Africa continue to use various forms of prison contract labor, in con-

This interpretation of caste contrasts with the Marxist argument that the capitalist class purposefully plays off one segment of the working class against the other (e.g. Reich, 1971). Business, I would contend, rather than desiring to protect a segment of the working class supports a liberal or laissez faire ideology that would permit all workers to compete freely in an open market. Such open competition would displace higher paid labor. Only under duress does business yield to labor aristocracy, a point made in *Deep South*, a book written when the depression had caused the displacement of white tenant farmers and industrial workers by blacks:

The economic interests of these groups [employers] would also demand that cheaper colored labor should be employed in the "white collar" jobs in business offices, governmental offices, stores, and banks. In this field, however, the interests of the employer group conflict not only with those of the lower economic group of whites but also with those of the more literate and aggressive middle group of whites. A white store which employed colored clerks, for example, would be boycotted by both these groups. The taboo upon the employment of colored workers in such fields is the result of the political and purchasing power of the white middle and lower groups (Davis, *et. al.*, 1941:480).

In sum, exclusion and caste are similar reactions to a split labor market. They represent victories for higher paid labor. The victory of exclusion is more complete in that cheaper labor is less available to business. For this reason I would hypothesize that a higher paid group prefers exclusion to caste, even though exclusion means they have to do the dirty work. Evidence for this comes from Australia where, in early attempts to import Asian labor, business tried to buy off white labor's opposition by offering to form them into a class of "mechanics" and foremen over the "coolies" (Yarwood, 1968:16, 42). The offer was heartily rejected in favor of exclusion. Apartheid in South Africa can be seen as an attempt to move from caste to the exclusion of the African work force.

Most of our examples have contained a white capitalist class, a higher paid white labor group, and a cheaper, non-white labor group. Conditions in Europe and around the world, and not skin color, yield such models. White capitalists would gladly displace with and undercut their white working-class brethren if they could, and have done so whenever they had the opportunity. In the words of one agitator for excluding Chinese from the U.S. Pacific coast: "I have seen men . . . American born, who certainly would, if I may use a strong expression, employ devils from Hell if the devils would work for 25 cents less than a white man" (cited in Daniels and Kitano, 1970:43).

In addition, cases have occurred of white workers playing the role of cheap labor, and facing the same kind of ethnic antagonism as non-white workers. Consider the riots against Italian strikebreakers in the coal fields of Pennsylvania in 1874. (Higham, 1965:47-8). In the words of one writer: "Unions resented the apparently inexhaustible cheap and relatively docile labor supply which was streaming from Europe obviously for the benefit of their employers" (Wittke, 1953:10).

Even when no ethnic differences exist, split labor markets may produce ethnic-like antagonism. Carey McWilliams (1945: 82-3) describes an instance:

During the depression years, "Old Stock"—that is, white, Protestant, anglo-Saxon Americans, from Oklahoma, Arkansas, and Texas—were roundly denounced in California as "interlopers." The same charges were made against them that were made against the Japanese: they were "dirty;" they had "enormous families"; they engaged in unfair competition; they threatened to "invade" the state and to "undermine" its institutions. During these turgid years (1930-1938) California attempted to *exclude*, by various extra-legal devices, those yeoman farmers just as it had excluded the Chinese and Japanese. "Okies" were "inferior" and "immoral." There was much family discord when Okie girl met California boy, and vice versa. . . . The prejudice against the Okies was obviously not "race" prejudice; yet it functioned in much the same manner.

Conclusion

Obviously, this type of three-way conflict is not the only important factor in

trast to the northern U.S. where the contract system was attacked by rising labor organizations as early as 1880.

ethnic relations. But it does help explain some puzzles, including, of course, the exclusion-caste anomaly. For example, Philip Mason (1970:64) develops a typology of race relations and finds that it relates to numerical proportions without being able to explain the dynamic behind this correlation. Table 2 presents a modified version of his chart. My theory can explain these relationships. Paternalism arises in situations where the cleavage between business and labor corresponds to an ethnic difference. A small business elite rules a large group of workers who entered the labor market at approximately the same price or strength. No split labor market existed, hence no ethnic caste system arises. The higher proportion of the dominant ethnicity under "Domination" means that part of the dominant group must be working class. A labor element that shares ethnicity with people who have sufficient resources to become the business elite is generally likely to come from a fairly wealthy country and have resources of its own. Such systems are likely to develop split labor markets. Finally, competition has under it societies whose cheaper labor groups have not been a major threat because the indigenous population available as cheap labor has been small and/or exclusion has effectively kept business groups from importing cheap labor in large numbers.

Table 2. Numerical Proportion of Dominant to Subordinate Ethnic Groups*

Category		
Domination	Paternalism	Competition
Situations		
South Africa (1960) 1-4	Nigeria (1952) 1-2000	Britain (1968) 50-1
U.S. South (1960) 4-1	Nyasaland (1966) 1-570	U.S. North (1960) 15-1
Rhodesia (1960) 1-16	Tanganyika 1-450	New Zealand 13-1
	Uganda 1-650	

*Adapted from Mason (1970:64).

This theory helps elucidate other observations. One is the underlying similarity in the situation of blacks and women. Another is the history of political sympathy between California and the South. And, a third is the conservatism of the American white working class, or what Daniels and Kitano (1970:45) consider to be an "essential paradox of American life: [that] movements for economic democracy have usually been violently opposed to a thorough-going ethnic democracy." Without having to resort to psychological constructs like "authoritarianism," this theory is able to explain the apparent paradox.

In sum, in comparing those countries with the most ethnic antagonism with those having the least, it is evident that the difference does not lie in the fact that the former are Protestant and the latter Catholic: Protestants are found in all three of Mason's types, and Hawaii is a Protestant dominated territory. It does not lie in whether the dominant or subordinate group moves: South Africa and the deep south show opposite patterns of movement. It is evident that some of the most antagonistic territories have been British colonies, but not all British colonies have had this attribute. The characteristic that those British colonies and other societies high on ethnic antagonism share is that they all have a powerful white, or more generally higher paid working class.

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SOCIOECONOMIC-ETHNIC STATUS INCONSISTENCY AND PREFERENCE FOR POLITICAL CHANGE

MARVIN E. OLSEN JUDY CORDER TULLY
Indiana University University of Texas

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Although there have been numerous empirical tests of Lenski's proposition that status-inconsistent persons tend to favor political change more than consistent persons, three critical issues remain unresolved: (1) Do all forms of status inconsistency have such consequences, or just the pattern of low ascribed but high achieved status? (2) Which kinds of political attitudes and behaviors, if any, are actually affected by status inconsistency? (3) Does status inconsistency explain enough variation in any political variables to be of theoretical value? Survey data from Indianapolis show no significant relationships between any combinations of occupation, education, and income with any of ten indicators of preference for political change. Inconsistency between one's overall socioeconomic status and an index of overall ethnic status is related to the two variables of liberal economic attitudes and Democratic voting, but it explains only about one percent of the variation in both cases. Hence we conclude that status inconsistency should be discarded as an explanation of preferences for political change, and further attention given to a variety of other variables.

THE central thrust of Gerhard Lenski's (1954) original study of "status crystallization" was that "the more frequently acute status inconsistencies occur within a population, the greater . . . the proportion of that population willing to support programs of social change," as indicated by a preference for the Democratic Party and by liberal attitudes on economic issues.¹ Degree of status consistency was seen as a "non-vertical dimension" of stratification whose effects on behavior were presumably independent of one's statuses on various vertical dimensions.

Since then, a number of researchers have attempted to test this proposition empirically, using a variety of conceptual approaches and analytic techniques. The results have been contradictory and confusing: Kenkel's (1956) early study found no relationship between status inconsistency

and desires for political change, while Goffman's (1957) research did. A few years later, two reanalyses of Lenski's data by Jackson (1962) and by Treiman (1966) revealed that Lenski's proposition should have been limited to a few special forms of status inconsistency. At about the same time, Brandmeyer (1965) and Kelly and Chambliss (1966) published new data that did not substantiate the proposition; while the results of Lenski's (1967) study of four Western nations did. Next came a series of studies at the University of Michigan—by Segal and Knoke (1968), Segal (1969), Segal and Knoke (1969) and Smith (1969)—that fairly consistently supported the proposition, but only for certain forms of status inconsistency and only for the dependent variable of preference for the Democratic Party.²

Most recently, Broom and Jones (1970) reported finding no significant relationships between occupational-educational-income inconsistencies and preferences for the Aus-

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¹ Several writers before Lenski—beginning with Sorokin (1927)—discussed the phenomenon of status inconsistency, but Lenski was the first to investigate empirically the relation between inconsistency and preference for political change.

² Even within individual studies, contradictions often abound. For example, Segal (1968) found that persons with inconsistencies between race and income and between religion and occupation did not tend to favor the Democratic Party; whereas all other combinations of ethnic-socioeconomic status inconsistency did. And in Smith's (1969) study, inconsistencies between religion and both occupation and education were significantly associated with preferences for the Republican Party.

tralian Labor Party, but a significant tendency for Catholics of high socioeconomic status to favor this party more often than expected. Laumann and Segal (1971), meanwhile, found that inconsistencies between ethnoreligious and educational statuses evidenced virtually no significant relationships with several measures of preference for political change.³

These findings are contradictory in part because the studies used several different methods of data analysis. Without going into detail, we can observe that such methods as simple frequency counts or the four-cell "fold-over" technique are now generally regarded as inadequate or unreliable for measuring status inconsistency effects. Consequently, many of the earlier findings should probably be discarded. More recently, however, two more sophisticated analytical procedures have been used: dummy variable regression analysis (by Jackson and Burke, 1965; Treiman, 1966; Broom and Jones, 1970), and a variant of the analysis of covariance (Laumann and Segal, 1971). Even these studies contain contradictory findings, however, so that a definite conclusion concerning the validity and usefulness of Lenski's proposition remains elusive.

In particular, three critical issues concerning the political consequences of status inconsistency remain unresolved:

First: Do all forms of status inconsistency affect preferences for political change, or (as most recent studies have assumed) are such consequences limited to the particular pattern of low ascribed but high achieved status? If the latter is true, which kinds of ascribed status—race, religion, or nativity—and which indicators of achieved status—education, occupation, or income—are most relevant?

³ The sole exception was the case of highly educated Jews, who were more liberal than expected, but this pattern is easily explained by Jewish cultural values. When the analysis was restricted to persons expressing relatively high levels of status concern, a few other categories of respondents with low socioreligious but high educational statuses also evidenced significant relationships with the various political measures; but Laumann and Segal fairly convincingly explain all these findings by subcultural values rather than status inconsistency.

Second: If any forms of status inconsistency do have political consequences, what kinds of attitudes and/or behaviors are affected by such inconsistency? Are these relations limited to liberal political party and voting choices and liberal economic attitudes—the only indicators of preference for political change used in most previous studies—or do they also extend to other political variables?

Third: If statistically significant relationships exist between any forms of status inconsistency and any political measures, are they strong enough to warrant serious consideration in sociological theory? That is, despite statistical significance, does the predictor variable of status inconsistency explain enough variation in dependent political variables to justify its future use?

The research in this paper was designed to confront all three issues, providing more precise grounds for evaluating Lenski's proposition than presently exist. In short, we tried to see if, with every analytical advantage, status inconsistency could be of any real help in explaining desires for political change. In the following three sections we discuss the above issues in detail, formulating them as a series of five research questions.

Forms of Status Inconsistency

Lenski originally argued that status inconsistency was related to political liberalism, without regard to the status dimensions or patterns of inconsistency involved.⁴ After both Jackson (1962) and Treiman (1966) called attention to the importance of ethnic status in Lenski's analysis, he acknowledged (1967) that the association existed primarily with sharp discrepancies between ethnic and socioeconomic statuses, and not with inconsistencies limited to such socioeconomic indicators as occupation, education, and income. More recent studies have supported the generalization that if status inconsistency has any noteworthy political consequences, they emerge only when one's socioeconomic statuses are dis-

⁴ Lenski originally pointed to the combination of low ethnic and high socioeconomic statuses as being more strongly related to political liberalism than some other patterns of inconsistency, but he did not emphasize this phenomenon theoretically.

parate from one's racial, religious, nativity, or other ethnic statuses.

To explain theoretically the significance of this pattern of socioeconomic-ethnic status inconsistency, most writers distinguish between achieved statuses (such as education, occupation, and income) and ascribed statuses (such as race, religion, or nationality). As first suggested by Goffman (1957), specified more precisely by Jackson (1962), adopted by Lenski (1964), elaborated by Segal and Knoke (1968), and used by all subsequent writers, this thesis argues that status inconsistency will have political consequences only when it involves sharply disparate achieved and ascribed statuses. Since presumably most people see achieved statuses as more or less changeable, inconsistencies involving only disparate achieved statuses should result in individual mobility rather than desires for socio political change. Only when one's status inconsistency involves an ascribed status that is essentially unchangeable through individual mobility should the individual seek to change the socio-political system.

Moreover, this thesis applies only to the specific combination of a low ascribed and high achieved status. Only then is a low unalterable status combined with a high status attained through personal effort. No matter how high one rises on the educational, occupational, or income ladders, in many realms a person of low ethnic status will still encounter social barriers that he cannot change. Hence his only recourse—should he wish to escape his status inconsistency—will be to seek broad-scale social change through political action. A similar argument cannot be applied to persons of high ascribed but low achieved status, since normally they should be able to attain relative status congruence through individual mobility, without socio political change.⁵

⁵ This entire argument rests, of course, on the assumption that the individual occupying perceived inconsistent statuses wants to change the situation to escape from this inconsistency. As Lenski (1954) noted, if the person is happy with his inconsistent statuses, or has accommodated to them, he is not likely to try to change the situation. Similarly, social cross-pressures (Segal, 1969) or strong political alienation (Lane, 1959:147-154) might cause him to withdraw completely from the political arena. Some other factor, such

The importance of this particular pattern of status inconsistency was emphasized by Treiman (1966), who pointed out that the three statistically significant relationships in Lenski's original data all combined high socioeconomic with low ethnic status.⁶ The only study that has explicitly tested this hypothesis, however, was Broom and Jones' (1970) recent research in Australia. They found that high-SES Catholics disproportionately favored the Australian Labor Party, while low-SES Protestants did not.

If the above thesis is valid, we face a further unresearched question: Is the process limited to some particular kinds of achieved and/or ascribed statuses, or does it occur whenever any form of low ascribed status is combined with any form of high achieved status? For instance, does it matter whether one's measure of achieved socioeconomic status is education, occupation, income, or a combined index of all three? Similarly—and perhaps more critical—does it matter whether one's measure of ascribed status is race or religion or nationality?⁷

as dissatisfaction with one's social relationships (Bauman, 1968), or public visibility and social pressures (Segal, 1969), must propel the individual to reduce his status inconsistency. The individual of low ethnic status who has been relatively successful in the socioeconomic sphere would be particularly likely both to experience internal tensions and to feel that his condition is publicly visible and socially undesirable.

⁶ Brandmeyer (1965) earlier examined the specific categories of low ethnic/high occupational and low ethnic/high educational statuses, but attached no special theoretical importance to them. In contrast, all the various Michigan studies mentioned previously stressed the theoretical significance of this particular pattern of status inconsistency; but none explicitly compared this pattern with its converse of high ascribed and low achieved statuses.

⁷ Laumann and Segal (1971) recently carried this line of investigation even further by classifying their respondents according to the specific country from which their ancestors had come and analyzing each nationality category separately. However, the use of these fine nationality distinctions ignores the common theoretical argument that status inconsistency produces political consequences only when one's ascribed status is clearly defined, salient, and relatively disadvantaged. We doubt whether some of Laumann and Segal's ethno-religious categories—such as German Methodists—meet these criteria. Moreover, their sample was limited to native-born whites. They could not examine race or foreign birth as alternative kinds of ascribed statuses.

The theoretical reasoning sketched in this section led us to examine three related questions in our research:

1. Will status inconsistency based only on disparate achieved statuses show any relationships with preference for political change, or must status inconsistency involve both an ascribed and an achieved status before it evidences political consequences?

2. Among persons with incongruent ascribed and achieved statuses, are some combinations of ascribed and achieved statuses more likely than others to be associated with desires for changes in the political system, or will such consequences occur with all combinations of ascribed and achieved statuses?

3. Will marked preferences for political change occur only among persons with the pattern of low ascribed and high achieved statuses, or will it also occur among those having high ascribed and low achieved statuses?

Relevant Political Variables

As we noted, most previous research has been limited to the variables of economic attitudes, party preference, and voting choices as indicators of preference for political change. To determine whether the political consequences of status inconsistency cover a wider array of political attitudes and behaviors, this study examines ten different political variables. These dependent variables—all of which are conceptualized as indicators of preference for political change—are as follows:

(1) Liberal Attitudes on Economic Issues—an index composed of responses to four opinion questions about governmental economic programs and policies.⁸

⁸ The questions used in this index and the index on racial affairs are quite similar to items devised by Kelly and Chambliss (1966) for their indexes of economic and racial attitudes. The statements comprising our economic index, to which respondents replied on five-point Likert scales, were (a) "The Federal income tax should be abolished in order to stimulate private business and keep our economy strong," (b) "When unemployment is high, the government should spend money in ways that will help to create more jobs," (c) "Government welfare programs like Social Security and Medicare do more harm than good, and should

(2) Liberal Attitudes on Racial Affairs—an index composed of responses to four opinion questions about current civil rights activities and goals.⁹

(3) Attitudes of Political Discontentment—an index based on four attitude items concerning lack of confidence or trust in politicians and the political system.¹⁰

(4) Legitimacy of Protest Actions—a scale which measures the respondent's willingness to grant legitimacy to several different kinds of social protest actions.¹¹

(5) Increase Power of the Federal Government—a measure of expressed desire for the federal government to exercise more power than at present.¹²

be stopped," and (d) "Economic security and well-being for everyone is not possible without extensive governmental participation in economic activities."

⁹ The component statements in this index, scored in the same way, were (a) "When schools are racially integrated, the quality of education almost always declines," (b) "If Negroes are not getting fair treatment in jobs and housing, the government should act to help them," (c) "If Negroes want to be accepted by whites, they must learn to work harder and show that they can be responsible citizens," and (d) "Civil rights demonstrations to block construction of housing projects and other facilities are often morally justified."

¹⁰ The component statements, to which respondents replied on three-point agree-disagree scales, were (a) "Most public officials are not qualified for their jobs, and many of them are only concerned about getting money or power," (b) "Money is the most important factor influencing public policies and decisions," (c) "Political leaders usually represent the special interests of a few powerful groups, and rarely serve the common needs and welfare of all citizens," and (d) "Politicians spend most of their time getting re-elected or re-appointed to office, and don't give enough attention to their public responsibilities." For further discussion of this concept of political discontentment, see Olsen (1969).

¹¹ The component agree-disagree statements, which formed a unidimensional scale for this sample, were given in reference to the general question of whether the respondent thought that dissatisfied groups in this country have the right to take the following actions: (a) "Hold public meetings and rallies," (b) "March quietly and peacefully through town," (c) "Take actions such as picketing or boycotting or petitioning," (d) "Take actions such as strikes or 'sit-ins' or 'walk-outs,'" (e) "Stage mass protest demonstrations with large crowds of people," and (f) "Engage in civil disobedience by purposefully breaking minor laws." For further analysis of this scale, see Olsen (1968, 1970).

¹² Goffman's (1957) early study used such a

(6) Democratic Party Preference—a measure of expressed preference for the Democratic Party in national elections.¹³

(7) Democratic Voting in the 1966 Congressional Election—reported voting choice in the 1966 election for the U. S. House of Representatives.¹⁴

(8) Democratic Voting in the 1964 Presidential Election.

(9) Active Partisan Political Participation—an index measuring frequency of participation in four different kinds of party activities.¹⁵

(10) Active Governmental Interaction—an index measuring frequency of three kinds of interaction with local and national government.¹⁶

Our fourth research question was to discover which, if any, of these political inci-

cators are correlated with any form of status inconsistency.

Explanatory Power of Status Inconsistency

Almost all previous research on the political consequences of status inconsistency focused on whether such a relationship did exist, and if so, whether it was statistically significant. Having discovered a statistically significant relationship, however, one should go on to examine the amount of variation in the dependent variables that can be explained by the independent variable of status inconsistency. Despite statistical significance, a correlation that explains only one or two percent of the total variation is essentially meaningless. Our fifth research objective was therefore to examine the amount of variation explained, as well as the statistical significance of all relationships. Unless status inconsistency can account for at least a moderate amount of variation in one or more political variables, it is of questionable theoretical use in explaining preferences for political change.

Following Lenski's (1964) suggestion, almost all recent studies of status inconsistency have treated this variable statistically as an interaction effect between two status dimensions. As Segal and Knoke (1969) noted, "the interaction term cannot be equated with inconsistency effects, since . . . interaction might be attributed to a wide range of phenomena, but if one assumes additive main effects, then one can use the interaction term to estimate the inconsistency effect." Based on this reasoning, and following the examples of Jackson and Burke (1965), Treiman (1966), and Broom and Jones (1970), we used dummy-variable regression analysis in this research. This technique differentiates between additive and interaction models, enabling us to separate (a) the effects of interaction between status variables (which we take as an indicator of status inconsistency effects) on the dependent political variables, from (b) the straightforward additive effects of these status variables taken separately. The technique also allows us to determine the proportion of total variation in each dependent variable explained by status inconsistency.¹⁷

measure, but no subsequent study of status inconsistency has included it. This item asked: "In your opinion, does the federal government exercise too much power in the United States today, about the right amount of power, or not enough power?" Data on desires for a weaker government are not reported in Table 1, since they are not relevant to the proposition being tested; but the ensuing discussion does refer to these findings.

¹³ This question was worded as follows: "In national elections, do you consider yourself a Republican, a Democrat, an Independent, or do you have some other political preference?"

¹⁴ This was the last national election held before collecting these data. With both voting measures, percentages of respondents voting Democratic were based only on the number of persons who actually voted in that election, and excluded those who did not vote by choice or ineligibility.

¹⁵ Vast research has documented the existence of consistent relationships among all forms of political participation and both socioeconomic and ethnic status (see Milbrath, 1965). Surprisingly, however, there has been almost no research on the effects of status inconsistency on political participation. Lane (1959:249) argued that persons of high socioeconomic but low ethnic status should be particularly active in partisan political activities, but he presented no data to support this contention. The activities included in our index of partisan participation were reading party literature, talking with a party worker, wearing a campaign button or displaying a political poster or bumper sticker, and attending a party meeting, rally, or dinner.

¹⁶ The component activities included writing a letter to a state or federal legislator, contacting a public official in some way, and attending a meeting of a public committee, board, commission, or council.

¹⁷ For extensive discussions of the distinction

Research Design

Data for this research were taken from the 1967-68 Indianapolis Area Project of the Institute of Social Research at Indiana University. The population for this study was the entire urbanized area (i.e., central city and surrounding suburbs) of Indianapolis, from which a sample was drawn using probability sampling with quotas.¹⁸ A total of 750 adults was interviewed (by graduate students and professional interviewers) during January-March 1968.

Three different indicators of achieved socioeconomic status were used (a) education—measured by total number of years of formal schooling completed, including vocational or technical training; (b) occupation—measured with the first digit of the Duncan Socioeconomic Index number for the occupation of the head of the household; and (c) income—measured by total family income in 1967 before taxes. In addition, a combined Socioeconomic Status Index was constructed from these separate measures.¹⁹

Four different kinds of ascribed status were ascertained, all of which can be included under the general heading of ethnicity. Each variable was measured as a dichotomy: (a) race—with all nonwhites categorized as low status; (b) religion—

with Catholics, Jews, and Orthodox Christians categorized as low status;²⁰ (c) nativity—with the low status category consisting of all persons born outside the United States or both of whose parents were foreign-born;²¹ and (d) ethnic identification—with low status indicated by a "yes" response to the question: "Do you consider yourself a member of a minority group, on the basis of your race or religion or national origin?"²² A combined Ethnicity Index was also constructed from all four measures.²³

Before using dummy-variable analysis, we reduced all the above indexes (except the dichotomized ethnicity indicators) into trichotomies, with categories as nearly equal as possible in number of cases.²⁴ Although all three categories of each index were used in the analyses, primary attention was given to the high and low cells in each case, to insure that status discrepancies would be relatively pronounced. We thus tried to create favorable conditions for discovering correlations between status inconsistency and the dependent political measures. Each cate-

²⁰ Catholics were classified as "disadvantaged" on the grounds that although this society does not formally discriminate against them, their average rankings on all three indicators of socioeconomic status are significantly below those for Protestants. There were only six Jews and one Orthodox Christian in the sample. Persons with no religious preference were classified with Protestants as "non-disadvantaged."

²¹ The population of Indianapolis does not contain any large nationality minorities (most of the people are at least second-generation Americans); thus, it was not possible to classify respondents by country of origin in any meaningful way. Hence we could not replicate Laumann and Segal's (1971) scheme of socioreligious groups based on nationality. Our dichotomy of high and low nativity was the only classification that was both theoretically meaningful and yielded enough cases in the low status category for analysis.

²² Although ethnic identification is a subjective phenomenon, we included it on the grounds that, in this area, how one identifies or classifies himself may be important in shaping attitudes and behavior.

²³ This Ethnicity Index consisted of three categories: low = two or more kinds of low or disadvantaged ethnic status; medium = one kind of low ethnic status; and high = no low ethnic statuses.

²⁴ The cut-off points used in making these categorizations are not given here for reasons of space, but this information is available from the first author on request.

between additive and interaction effects in relation to status inconsistency, see Blalock (1967a and b) and Jackson and Burke (1965).

¹⁸ See Sudman (1966). This procedure, developed by the National Opinion Research Center, uses probability sampling down through the selection of blocks (or clusters of blocks), but chooses individual respondents by quotas within five different categories defined by age for men and employment status for women (which are the factors that have been found to affect availability for interviewing most directly). Quotas for each category in each block are determined by the composition of the population living in that census tract. In addition, strict controls are imposed on the interviewers to insure that no dwelling units with potential respondents are skipped. Though this procedure produces a small amount of additional sampling error (less than 10 percent in most cases), it saves considerable time and money.

¹⁹ To construct this combined index, respondents' scores on the three separate measures (all of which were scored 0-8) were added together. The resulting array of summed scores (ranging from 0 to 24) was then grouped into eight categories as nearly equal in number of cases as possible.

gory of each status variable was then converted into a dummy variable by giving each respondent a score of "1" on that variable if he fell into that category, or a "0" if he did not. To avoid linear determinancy, the lowest category of each independent variable was omitted from the regression equation.

Two different kinds of analysis were carried out with each political measure. In the first, two independent variables (e.g., a socioeconomic and an ethnicity variable) were crossclassified to form a two-way analysis-of-variance table. Using the coefficients for each independent variable from the additive regression equation, we also calculated an expected score for each cell in the table. We then determined both (a) the total proportion of variation in the dependent variable explained with a strictly additive model, and (b) the amount of additional variation explained by interaction effects across all cells in the table. Finally, we obtained an F ratio to test the statistical significance of these overall interaction effects (Cohen, 1968).

The second procedure was used only on those dependent variables that either (a) displayed significant interaction effects in the first analysis, or (b) contained an inconsistent cell with a noticeably high or low score. Our focus in this stage was on the specific high-low and low-high inconsistent cells. Each of these cells was converted to a dummy variable which was included in the regression equation. We thus determined whether that one inconsistent cell explained any variation in the dependent variable beyond the amount explained with an additive model. Finally, we calculated an F ratio to test the significance of the additional variation explained by the inconsistent cell.

In the interest of examining any relationship between status inconsistency and any political variables that approached statistical significance, we looked at all correlations with $p < .10$.

Results and Analysis

Socioeconomic Inconsistency: The three socioeconomic status measures of occupation, education, and income were first com-

pared with one another in pairs to see if any of the resulting forms of status inconsistency produced interaction effects with any of the ten political variables. These data are not reported here, since none of the resulting thirty tables showed any statistically significant relationships, and most of the small interaction effects that did occur were in the opposite direction from that predicted by the theory. In addition, a number of checks also convinced us that no significant interaction effects were likely to emerge when any two-variable SES index (e.g., occupation and income) was cross-classified with the third status variable (e.g., education). On the basis of these findings, we can answer our first research question in the negative: Status inconsistency involving only achieved socioeconomic statuses does not appear to produce discernible preference for political change.

Socioeconomic-Ethnic Inconsistency: Next, we put together each of the three socioeconomic variables (occupation, education, and income) with each of the four ethnicity measures (race, religion, nativity, and identification) in successive pairs, to discover if any of these twelve forms of socioeconomic-ethnic inconsistency produced interaction effects with any of the ten dependent political variables. The resulting 120 tables cannot be given here, but the findings can be briefly summarized: small but statistically significant relationships were observed between all twelve forms of inconsistency and the two political variables of liberal economic attitudes and Democratic voting in the 1966 election, but not with any other indicators of preference for political change.

Two conclusions can be drawn from these findings: (1) In answer to our second research question, we note that the political effects of status inconsistency are not limited to any particular forms of ascribed or achieved status, but emerge whenever any ascribed status dimension is paired with any achieved status dimension. (2) With respect to our fourth research objective, we see, however, that these political consequences of status inconsistency may be very narrow in scope. We shall explore this latter

possibility in more detail, after first considering the implications of the former conclusion.

It is perhaps not surprising to find that the variables of education, occupation, and income are in effect interchangeable in our analyses, since they are empirically related and can be viewed theoretically as simply different aspects of one's overall socioeconomic status. But it is notable that this same situation occurs with all four measures of ascribed ethnic status. The political consequences of status inconsistency—however limited they may be—are not a function solely of race or religion or nativity or ethnic self-identification, but emerge when any of these forms of ethnicity is paired with any indicator of socioeconomic status. In short, the crucial factor is a combination of any socioeconomic measure with any ethnicity measure, so that we can speak of this process as a general interaction effect between ascribed and achieved statuses, regardless of the particular empirical measures used. For this reason, we shall hereafter conceptualize both socioeconomic and ethnic status as single, but separate, dimensions of social stratification, and report only data on inconsistency between the two indexes of socioeconomic and ethnicity status.

Table 1 shows the results of the analyses performed with these SES and ethnicity indexes, conceptualized as measures of achieved and ascribed statuses, respectively. For ease of presentation, only the four cells from each table involving high and low SES and ethnic statuses (i.e., the high-high, high-low, low-high, and low-low cells) are reproduced here, since these are the most theoretically meaningful status combinations. (In no table did cells involving medium status on either or both stratification dimensions contribute significant interaction effects.) For each political variable, the table gives (a) the observed score in each of these four cells (the higher the score, the more liberal), (b) the predicted score for each cell, based on an additive model, (c) the difference between the observed and predicted scores, (d) the percent of total variation in the dependent variable explained by a strictly additive model, (e) the amount of variation in the entire table explained by the inter-

action model, (f) the F ratio for this interaction,²⁵ (g) the level of significance of the F, and (h) the N's for the four cells reported and for the whole table.

In the first panel of the table, we see that people with inconsistent socioeconomic-ethnic statuses tend to hold more liberal attitudes toward governmental economic programs than predicted by an additive model; significant overall interaction effects occur here. Note also that status-consistent persons are less likely than expected to be liberal in this area. In the next four panels, however, it is evident that status inconsistency does not produce higher-than-predicted liberal scores in any of the other attitudinal areas of racial affairs,²⁶ political discontentment, protest actions legitimacy, or power of the federal government. In fact, in all but one of the inconsistent cells, the slight tendencies that do occur are in the opposite direction from that predicted by the theory. None of these minor interaction effects produces statistically significant results.

The sixth panel shows a slight tendency for inconsistent persons to favor the Democratic Party in greater numbers than predicted, but this relationship is not significant. Status-inconsistent people also show a clear preference for the Democratic Party in both the 1966 and 1964 elections, although only the relationship for the 1966 election is statistically significant. Two possible explanations for the weaker relationships with the 1964 data are that (a) at least some of the respondents' achieved statuses may have changed during the four-year interval between that election and the time of the survey, and (b) the high rate (over 90%) of Democratic voting among all persons of low ethnic status in that election left little margin for persons of low ethnic but high socioeconomic status to evidence a higher-than-expected rate of Democratic preference. The table does not show data for Republican voting in the two elec-

²⁵ The formula used to compute these F values is given in Cohen (1968:435).

²⁶ This finding of a nonsignificant relationship between status inconsistency and tolerance for racial integration accords with the results of an earlier study on this topic by Fauman (1968).

Table 1. Socioeconomic-Ethnic Status Inconsistency Related to Ten Indicators of Preference For Political Change

	High SES- High Ethnicity	High SES- Low Ethnicity	Low SES- High Ethnicity	Low SES- Low Ethnicity	Total Table
Liberal Attitudes on Economic Programs					
Observed score	2.57	3.74	3.35	3.27	
Predicted score	2.68	3.07	3.23	3.62	
Difference	-.11	.67	.12	-.35	
Explained by additive model					.045
Explained by all interaction					.017
F for all interaction					2.90
Significance (P)					<.05
N	128	23	85	45	656
Liberal Attitudes on Racial Affairs					
Observed score	2.30	3.30	2.20	3.63	
Predicted score	2.21	3.37	2.39	3.55	
Difference	.09	-.07	-.19	.08	
Explained by additive model					.098
Explained by all interaction					.006
F for all interaction					1.28
Significance (P)					>.10
N	133	23	93	54	697
Attitudes of Political Discontentment					
Observed score	1.40	1.57	2.02	2.37	
Predicted score	1.38	1.61	2.06	2.29	
Difference	.02	-.04	-.04	.08	
Explained by additive model					.040
Explained by all interaction					.001
F for all interaction					.11
Significance (P)					>.10
N	140	23	105	57	745
Legitimacy of Protest Actions					
Observed score	3.11	3.30	2.28	2.58	
Predicted score	3.03	3.35	2.21	2.53	
Difference	.08	-.05	.07	.05	
Explained by additive model					.041
Explained by all interaction					.006
F for all interaction					1.06
Significance (P)					>.10
N	140	23	105	57	745
Increase Power of Federal Government					
Observed score	10%	14%	31%	49%	
Predicted score	07	19	34	45	
Difference	03	-04	-04	04	
Explained by additive model					.077
Explained by all interaction					.004
F for all interaction					.61
Significance (P)					>.10
N	131	21	65	47	643
Democratic Party Preference					
Observed score	20%	59%	51%	72%	
Predicted score	20	51	48	80	
Difference	0	08	03	-08	
Explained by additive model					.122
Explained by all interaction					.004
F for all interaction					.84
Significance (P)					>.10
N	135	22	88	50	687

Table 1—Continued

Democratic Voting in 1966					
Election					
Observed score	27%	93%	60%	83%	
Predicted score	28	70	54	95	
Difference	-01	23	06	-12	
Explained by additive model					.178
Explained by all interaction					.016
F for all interaction					2.06
Significance (P)					<.10
N	101	14	45	30	421
Democratic Voting in 1964					
Election					
Observed score	33%	90%	61%	98%	
Predicted score	34	79	59	100	
Difference	-01	11	02	-02	
Explained by additive model					.212
Explained by all interaction					.006
F for all interaction					1.02
Significance (P)					>.10
N	111	21	52	43	506
Active Partisan Participation					
Observed score	1.84	1.26	0.90	1.56	
Predicted score	1.73	1.96	1.07	1.29	
Difference	.11	-.70	-.17	.27	
Explained by additive model					.025
Explained by all interaction					.009
F for all interaction					1.69
Significance (P)					>.10
N	140	23	105	57	745
Active Governmental Interaction					
Observed score	1.71	1.48	0.29	0.67	
Predicted score	1.59	1.75	0.38	0.54	
Difference	.12	-.27	-.09	.13	
Explained by additive model					.101
Explained by all interaction					.005
F for all interaction					.96
Significance (P)					>.10
N	140	23	105	57	745

tions, but as an aside we might also note that in both cases status-inconsistent persons decisively rejected the Republican Party: in 1966, only 20% of all inconsistencies, but 49% of all consistent voters Republican; while in 1964 the corresponding rates were 23% and 45%.²⁷

²⁷ An added factor in this situation is the relatively high rate of nonvoting among persons with inconsistent statuses, especially those with low SES but high ethnic statuses. The average rate of voting in both elections was 88% among consistent but only 65% among inconsistent. When they vote, status-inconsistent people clearly favor the Democratic over the Republican Party; but a third of them stay home on election day. Theoretically this is not at all surprising, since political apathy is often mentioned as an alternative response to status inconsistency (Segal, 1969).

Finally, in the last two panels of the table, interesting but completely unexpected findings emerge. In the areas of both partisan political participation and government interaction, status-inconsistent people tend to be less active than predicted by an additive model; while those with consistent statuses are more active than expected. These trends are in the opposite direction from that predicted by the theory, but neither is statistically significant.

We can now answer our fourth research question: No general relationship exists between status inconsistency and preference for political change. As first observed in the previous analyses of the various separate status measures, and as documented in Table 1, such relationships occur only with the

dependent variables of liberal economic attitudes and Democratic voting, and not with any other forms of political attitude or behavior. Rather interestingly, these same two indicators of political liberalism were the only ones used by Lenski (1954) in his original study of status inconsistency.

Single-Cell Analyses: Table 1 tells us that socioeconomic-ethnic status inconsistency, measured as interaction between two major dimensions of social stratification, is at least minimally related to two indicators of preference for political change—liberal economic attitudes and Democratic voting in the 1966 election. In neither case, however, do these analyses indicate which inconsistent cells have significant interaction effects. We therefore analyzed the inconsistent cells individually, to determine the strength of their interaction effects. In addition to the two sharply inconsistent cells from each of the above dependent variables, we similarly examined the inconsistent cells for the 1964 election (since the two elections had similar trends) and for partisan participation and

governmental interaction (since their negative trends were interesting, though non-significant). The remaining five dependent variables were discarded on the grounds that they lacked significant overall interaction effects or sufficiently interesting potential tendencies. Table 2 reports the results of these single-cell analyses. For each of the five dependent variables, the table gives (a) the proportion of total variation in that variable explained by the additive model (these figures are repeated from Table 1, and are included here for comparative purposes); (b) the proportion of variation explained by the interaction in each of the two sharply inconsistent cells in that table; (c) the F ratio for each interaction term; and (d) the level of significance of the F.

As expected, the high SES/low ethnicity cells display significant interaction effects on the variables of liberal economic attitudes and democratic voting in 1966. For the 1964 election, the F ratio for this single cell is higher than was the F for the overall interaction effects (Table 1); but it still is

Table 2. Socioeconomic-Ethnic Status Inconsistent Cells Related to Five Indicators of Preference for Political Change

	Total Table Additive Model	High SES- Low Ethnicity Cell Interaction	Low SES- High Ethnicity Cell Interaction
Liberal Attitudes on Economic Programs			
Variation explained	.045	.013	.001
F for single cell interaction		8.35	0.68
Significance (P)		<.01	>.10
Democratic Voting in 1966 Election			
Variation explained	.178	.010	.002
F for single cell interaction		5.21	0.96
Significance (P)		<.05	>.10
Democratic Voting in 1964 Election			
Variation explained	.212	.003	.001
F for single cell interaction		1.91	0.25
Significance (P)		>.10	>.10
Active Partisan Participation			
Variation explained	.025	.007	.002
F for single cell interaction		5.18	1.52
Significance (P)		<.05	>.10
Active Governmental Interaction			
Variation explained	.101	.002	.001
F for single cell interaction		1.65	0.74
Significance (P)		>.10	>.10

not statistically significant. The relationship for the partisan participation cell is statistically significant, though in the opposite direction from that predicted by the theory. The corresponding figure for governmental interaction is not significant, however. In contrast to these three significant relationships for the high SES/low ethnicity cells, no result for the low SES/high ethnicity cells approaches statistical significance.²⁸

These findings imply that the pattern of low achieved status coupled with high ascribed status produces no observable tendency to favor political change; whereas the opposite pattern of high achieved and low ascribed status does somewhat affect some political attitudes and behavior. Our third research question is thus answered.²⁹

Finally, one last but vital observation remains to be made of the data in Table 2, to satisfy our fifth research objective. The fact that socioeconomic and ethnic statuses have statistically significant interaction effects with at least three indicators of preference for political change does not itself show how important these interaction effects are in predicting variation in those political variables. We argued above that statistical significance is meaningless unless the predictor variable explains at least a moderate amount of variation in the dependent variables. Thus we must examine Table 2's figures for the proportion of variation explained in the three significant cells. We find that all are extremely small: 1.3% for economic attitudes, 1.0% for 1966 voting, and 0.7% for partisan participation. Accounting for inter-

action effects adds practically no predictive power beyond that provided by a simple additive model.

Discussion and Conclusions

Our five research questions have been answered in the following ways:

1. Status inconsistency based only on disparate achieved statuses does not discernibly effect preferences for political change. Only when the inconsistency involves an achieved status (such as some form of socioeconomic status) that differs sharply from an ascribed status (such as some form of ethnicity) does this condition have political consequences for the individual.

2. Within the limits of our data, however, it does not seem to matter which particular achieved or ascribed statuses are involved in this process. All combinations of our three socioeconomic indicators—education, occupation, and income—when paired with any of our four kinds of ethnicity—race, religion, nativity, or subjective identification—discernibly effect some political attitudes and behavior. The most pronounced effects appear when a person's status on the combined Socioeconomic Index is sharply inconsistent with his status on the combined Ethnicity Index.

3. The pattern of one's status inconsistency is a crucial factor, however. Greater-than-expected preferences for political change occur only among individuals with the specific pattern of a low ascribed but high achieved status; no such tendency occurs among persons with other inconsistency patterns.

4. Of our ten indicators of desire for political change, only two—liberal attitudes toward economic issues and Democratic voting—display relationships with low-ascribed/high-achieved status inconsistency that are statistically significant in the predicted direction. A significant relationship between this kind of status inconsistency and political participation was discovered, but it is in the opposite direction from that predicted—status consistent people were more active than inconsistent.

5. None of these statistically significant relationships explains more than 1½% of

²⁸ Though the data are not given in the paper, there were also no significant results in any of the mildly inconsistent cells (such as high-medium or medium-low).

²⁹ Readers of an earlier version of this paper asked whether our finding that persons in the high SES/low ethnicity cell tend to be politically liberal might not be due to the presence of a high proportion of well-educated Jews in this cell. In other words, they suggested that these findings might better be explained by a socioreligious subculture theory instead of a status inconsistency theory. For the six Jews among the twenty-three people in this cell, such an explanation might well be relevant; but this argument (a) ignores the fact that everyone in this cell has two forms of low ascribed status, not just one, and (b) would not apply to the other seventeen people in the cell.

the total variation in the dependent variables.

We therefore conclude that, *in a strictly technical sense*, our research substantiates, with severe qualifications, both Lenski's original generalization that status inconsistency leads to a preference for political change, and the underlying theoretical argument that the crucial factor in this situation is the existence of a sharp and undesirable contrast between ascribed and achieved statuses that cannot be alleviated through individual mobility. But Lenski's proposition is valid only in the case of low-ascribed/high-achieved status inconsistency, and only with the two dependent political variables of liberal economic attitudes and Democratic voting.

The above generalization is true, however, only in the technical sense that two relevant statistically significant relationships do exist in our data. If one is concerned instead with the explanatory power of concepts such as status inconsistency, then Lenski's proposition appears relatively worthless theoretically. It does not substantially increase our ability to predict preferences for political change beyond what we can predict from the separate effects of socioeconomic status and ethnicity.

Moreover, we seriously question the theoretical reasoning commonly given to explain the observed relationship between status inconsistency and liberal economic attitudes. People with high achieved but low ascribed statuses are already relatively successful in socioeconomic terms, and probably stand to gain little personal benefit from public programs for the socioeconomically disadvantaged. It would be quite understandable if they strongly favored eliminating ethnic discrimination through political action—which in fact they do not—but their status inconsistency does not by itself explain why they tend to express liberal attitudes on economic issues.

Perhaps a more fruitful approach to this question of preferences for political change would be to study further the separate effects of a variety of independent variables, such as one's social power, privileges, and prestige. For instance, much research has demonstrated that both socioeconomic status

and ethnicity have marked additive effects on voting choices and other political variables.⁸⁰ In our data, both of these explanatory variables have significant effects—either direct or inverse—on all the political measures except the two indicators of participation in partisan and governmental activities (these variables are strongly related to SES, but not to ethnicity). Many relevant empirical questions remain unanswered, however—such as the relative importance of various SES and ethnicity factors in influencing desires for sociopolitical change, or the degree to which the effects of these factors are mediated by one's perceptions of opportunities for individual mobility. Nor do we presently have adequate theoretical explanations for many of these empirical relationships.

Moving beyond these variables of socioeconomic status and ethnicity, we might also explore such basic sociodemographic variables as age, sex, and marital status. For instance, to what extent are we now witnessing a process of social change through cohort socialization as the new generation of post-World War II children come to adulthood? Why have women recently become militant, protesting against conditions that have existed for generations? And does the assumption of marital and/or parental responsibilities dampen one's desires to change society?

Alternatively, we might follow up some of Lenski's (1966) more recent propositions concerning the importance of social power in social change. Is it inevitable that the more powerful one becomes, the more likely he is to defend the status quo, since it is the source of his power? Under what conditions will relatively powerless people cease to accept the legitimacy of the established social order and seek to change or destroy it? If power could be effectively decentralized in a modern society, what consequences would this have for sociopolitical change?

Closely related to power is the phenomenon of economic security. Regardless of a person's objective economic conditions, does the degree to which he feels himself to be economically secure influence his desires for

⁸⁰ See Lane (1959) and Lipset (1960) for summaries of much of this literature.

change? We might hypothesize, for instance, that one must attain some adequate level of economic security before he will welcome experimentation and change in the economic and political spheres. From this perspective, it is not the most deprived persons who press for social change, but those who combine economic (and perhaps social) security with political concern and sophistication.

Finally—though this topic falls more within the domain of political science—the structure and processes of the political system clearly affect people's perceptions of possibilities for change and desires to attempt change.

In short, we have many possible explanations for why people do or don't seek sociopolitical change, without bringing in the phenomenon of status inconsistency. It is true that the pattern of a low ascribed status coupled with a high achieved status is significantly related to liberal economic attitudes and Democratic voting, as Lenski proposed. But more importantly, we conclude that Lenski's proposition is not particularly useful for explaining preferences for change since status inconsistency appears to explain only about one percent of the variation in these phenomena beyond that given by a straightforward additive approach using socioeconomic status and ethnicity. Let us therefore file this proposition under the heading of "valid but trivial information," and get on with the vital task of identifying, measuring, and explaining all the factors that contribute to desires for sociopolitical change in contemporary society.

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THE ALIEN AS A SERVANT OF POWER: COURT JEWS AND CHRISTIAN RENEGADES

LEWIS A. COSER

State University of New York at Stony Brook

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When political rulers are greedy for power, when they wish to maximize their autonomy in the face of feudal, bureaucratic and other impediments, they tend to avail themselves of the services of alien groups of men who have no roots in the country they rule. The rootless alien is an ideal servant of power who can easily be bent to the ruler's purposes because he is totally dependent and cannot accumulate autonomous power. Which particular alien groups can be used for these purposes depends on availability and historical circumstances. This paper deals with two historical cases: The Court Jews of Baroque Germany and the Christian renegades who served the Ottoman Empire at its height.

WHEN political rulers in absolutist states or bureaucratic empires wish to shore up their autonomous powers so as not to be dependent on feudal retainers, bureaucratic officials, gentry families, estates, or guilds of commoners, they are likely to attract to their court men who have no roots in the society over which these rulers exercise dominion. Men distant from the underlying population by

virtue of their alien birth are ideal servants of power. The ruler can afford to be close to them because they are so far removed from him in the status order they can never threaten his rule.

To illustrate the notion of social distance: Robert Park once wrote "The lady of the house may be on the most intimate personal relations with her cook, but these intimate relations will be maintained only as long

as the cook retains her 'proper distance' " (1964:257, cf. also Mason, 1962: chapters 1 and 2). What is true for domestic servants is often true for servants of power. Rulers can deal with them intimately when they are socially distant, better still when they are alien to everyone else in the society.

Reliable servants of power become especially useful in periods of social transformation when rulers want to wrest economic and political resources from dispersed power centers not under their control. At such historical junctures rulers become especially greedy. As I have argued elsewhere (1967), they then attempt to recruit to their staff men who will serve them totally.

Such allegiance is hard to gain from the ordinary run of men whose roles are varied because they are embedded in the texture of social life through ties with peers and kin, or involvement in occupational or residential groups. I showed in an earlier paper (1964, cf. also Hopkins, 1963) that political eunuchs can be seen as prototypical servants of power because they lacked roots in the social structure and hence depended on the oriental monarchs who used them. Being typically recruited from young children taken in war raids on the periphery of the Empire and then castrated, they had neither effective families of orientation nor of procreation. They could establish no ties in the community, and their loyalty was available to the monarch. There are, however, many cases where uncastrated members of alien communities served absolutist power. Such aliens, although sexually potent, were as politically impotent as the eunuchs.

A full historical survey would show that rulers used a great variety of alien elements to maximize their power and free them from indigenous social forces. The Islamic rulers of Egypt relied on slave soldiers, the Mamelukes, to buttress their regime. Jews were major advisors in the Muslim courts of medieval Spain, supporting the regime against the partly Christian population. Several African rulers relied on alien slaves. French kings used Swiss mercenaries, and Roman emperors used Praetorian Guards. Only two such cases are analyzed here. This is not an idiographic description but is meant to pinpoint a soci-

ological mechanism that operates in a variety of structurally similar historical situations. This is not to say that centralizing regimes cannot operate without such mechanisms nor that such mechanisms alone assure success. Rather, the point is that where such mechanisms have been available they have substantially contributed to the viability of centralizing regimes.

Which groups of men rulers will choose for their privileged servants will depend on availability. In Eastern civilizations, where harems were major institutions of the court, eunuchs that had originally served as harem guards were readily available for political tasks when the need for dependable instrumentalities arose. German absolutist rulers of the Baroque chose Ghetto Jews having pariah status in the country at large and cosmopolitan ties. In the Ottoman Empire, renegade Christians from the conquered areas of the sultan's territory were readily available.

COURT JEWS IN SEVENTEENTH-AND EIGHTEENTH-CENTURY GERMANY

The German princes and Hapsburg Emperors, wishing to rebuild and modernize their states and armies after the bloodletting of the Thirty-Years War and to emulate the splendor and power of the Sun King in Versailles, searched for suitable instruments. Their sixteenth-century predecessors had used the financial power of great South-German bankers and merchants such as the Fuggers for borrowing large sums of money and streamlining their financial administration. But such patricians no longer existed. In France and England a system of taxation had already developed. A native bureaucracy worked hand in hand with native bankers and merchants to keep the government solvent and powerful. In Germany, by contrast, the once proud middle class had declined into congeries of petty and spiritless small-town bourgeois; and the apparatus of state was weak (Stern, 1950:8 ff). Moreover, the decline of urban culture had revived the feudal impediments to centralized government, whether guild or estates, to which the insecure population now clung with reinforced vigor.

Under these circumstances, the German absolutist ruler, or would-be absolutist ruler must often have been near despair as he surveyed his exhausted, dispirited, tradition-ridden population. Few sectors or strata of that population gave him a lever for lifting the country into the modern age of absolutist and centralized rule.

Under the pressure of such structural limits all German rulers, great or small, turned to the Jews.

Jewish communities had existed in Germany since the early Middle Ages. Living in ghettos, the Jews at times enjoyed the special protection of the Emperor or other potentates against payment of high taxes. Persecuted and despised by the population in general and by guilds in particular, they were often driven from certain states then readmitted. Their position was highly insecure and marginal. At the mercy of the ruler, they had no citizen rights and were wholly unprotected, lacking those feudal safeguards and privileges that guilds or estates provided to Christians to shield them from the powers to be.

The very rootlessness and powerlessness of the Jews made it possible for their seventeenth and eighteenth century rulers to make a select few among them into tools for increasing power. The Jews attracted to court lived in twice compounded conditions of alienation. Their ghetto homes were communities of pariahs living in partly enforced and partly self-imposed separation from the surrounding Christian world. Ghetto Jews were prototypical strangers (Simmel, 1950:402 ff). But the Jews who became devoted servants of the court had to leave the Ghetto and abandon the rituals, and traditions, though not the religion, of other Ghetto dwellers, thus exposing themselves to the latter's contempt and rejection.

At court Jews lived in a social no-man's land. There they became the ruler's instrument for destroying feudal forces, estates, and guilds restricting his power. They were his financiers and bankers, advisors and collaborators, in his drive to establish a modern mercantilist economy and a unified, bureaucratized government. Only men free from corporative and traditional restraints, having international ties and no connections with feudal dignitaries or the urban institu-

tions of commoners were able to play such auxiliary roles.

A variety of gifted Jews superbly enacted the script written for them by the socioeconomic conditions of Baroque Germany.

Their effectiveness rested on mutual confidence and mutual support. They and the ruler were partners who, for reasons of their own, stood opposed to the traditional powers and the adversaries of modernity. Though the Jews were by far the weakest in the triad of forces contending for power and influence, their support was nevertheless central to the victory of absolutist rulers. (Cf. Caplow, 1968.)

Jewish financiers and entrepreneurs supplied the armies of their prince, financed his wars, arranged new loans and settled old debts. They supplied the jewels for the prince's wife and his mistresses, but they also were innovators in building up trade and industry in defiance of guild restrictions. At times they monopolized the trade in silver, salt, or tobacco. They built silk, ribbon, cloth, and velvet factories in Prussia; they were chief tax collectors and diplomatic representatives, financial administrators and bankers, but above all confidants of the prince.

The closeness that often developed between the ruler and the Court Jew was based on their common distance from the population at large. As Selma Stern (1950:12) has written:

Despite the great differences in social position, culture, racial background and tradition which separated them, they both lived in the same isolation: the prince because of his omnipotence and inaccessability, and the Court Jew because of his birth and religion, which caused him to be isolated from all classes of society and to be avoided by them all.

Simmel once remarked (1950:404) that the stranger "often receives the most surprising openness-confidences which sometime have the character of a confessional and which would be carefully withheld from more closely related persons." Since the stranger is estranged from the rest of the community, one can confide in him, knowing he will not be able to use the "guilty knowledge." (Cf. Hughes 1971:277 ff.)

While the prince's relations with his subjects were impersonal and mediated through

a variety of institutionalized arrangements, his relation to the Court Jew was direct, personal, and unmediated. The Court Jew typically enjoyed the right of immediate and direct approach and communication with the prince (Schnee, 1952-55, vol III: 203 and 210).

Prince and Court Jew were one in wishing to break down those intermediary powers of estates and guilds, which prevented the direct subordination of the prince's subjects to his personal rule. In a sense, they desired that the direct personal relations which prevailed between them should become the prototype for all subjects of the prince as yet shielded from his will.

The dues and taxes paid by the Jews went directly to the prince and were not subject to the financial controls ordinarily exercised by estates and towns (Carsten, 1958:149). It stands to reason that this was perceived by the estates as a dangerous precedent and by the prince as a harbinger of things to come.

What Selma Stern (1962:75) says about the policies of Frederick William, the Great Elector of Brandenburg (1640-1688), applies more or less to all German princes of the seventeenth and eighteenth century:

He used the Jews in his struggle against the estates, just as he used them in his struggle against the guilds; he relied on them, just as he relied on all the non-privileged classes of his country, officials and officers, pastors and industrialists. They became one of the engines of war with which he destroyed the world of the estates and they were at the same time a part of the scaffolding with the help of which he erected the modern state.

Though the Court Jew performed highly important services for the prince, and though they were often intimate, their relations remained asymmetrical. The social gulf that separated them was so vast that symmetrical reciprocity could never develop between them. The Court Jews were rewarded with titles, money, specialized immunities and privileges. Despite these exchanges of goods and services the Prince expected particular loyalty and gratitude from the Jew. In Simmel's view (1950: 395), the Jew was considered as having a "generalized obligation . . . which [could] be redeemed by no accomplishment

whatever." He was permanently in debt to the ruler who had raised him from the depths.

Though the position of Court Jew became institutionalized in time, each incumbent to that position depended exclusively on the personal support of his prince. There was no security of tenure, as in bureaucratic organizations, nor could the Court Jew transmit the position to his descendants, as in hereditary service aristocracies. Unsupported by the population, unprotected by bureaucratic safeguards, unable to transmit their privileges to their descendants, the Court Jews could accumulate no power and hence were ideal instruments for men of power.

Specific Court Jews served specific princes on a particularistic basis. Hence the death of a ruler and the accession of a successor often caused the fall of the favorite of the preceding reign. The boon of particularism then turned to disaster. Often the Jew was thrown to the wolves, hung with the misdeeds of the previous regime. Even in his fall, he served his rulers. The guilds and estates who had bitterly opposed him not only as a competitor but as representative of the hated state, were now free to vent their resentment.

Their hatred was directed not against the prince, who was surrounded by the trappings of divine right and so high that he could not be reached by their criticisms, but against his 'evil councillors' who levied taxes in his name and carried out his policies. Among these councillors the Jews were particularly vulnerable because they had no recognized status, because they were strong only if protected by the prince. (Carsten, 1958:150.)

The growth of the modern state in Germany rested to a high degree on the services Court Jews provided for their princes. Their role came to an end in the nineteenth century when the growth of modern banking and modern administration made them obsolete. As the economic historian David S. Landes (1960:205) has written, "Personal considerations retained their importance [only] so long as the credit of the state was weaker than that of the banker."

Court Jews, as has been shown, were lifted by their princes from their status as pariahs into the heady world of the court to serve their masters as totally beholden instruments of rule. They were the func-

tional equivalents of political eunuchs of the Eastern world and celibate priests of the Catholic Church. Though they had families of their own, these families were as effectively cut off from the surrounding world of the gentiles as individual eunuchs or celibate priests were from the community and kinship that grow from sexual union.

Their descendants frequently moved into upper class positions, most often after becoming Christians and in this sense were in the vanguard of Jewish upward mobility in the next period. But in so doing they became structurally unfitted for the services their ancestors had provided.

Similar dynamics were at work in the use of renegade Christians by the Ottoman Empire. Here also rulers made these men devoted servants because they were aliens in the land. Their descendants too could carve themselves a niche in the upper class; but they themselves, while often reaching heights of power and privilege depended on the ruler who had raised them.

RENEGADE CHRISTIANS AS MAINSTAYS OF OTTOMAN RULE

In its early stages, the Ottoman Empire was based on a relatively unstable balance between the Sultan's forces and his civilian and military bureaucracy, and "feudal" and aristocratic land owning strata. (Eisenstadt, 1960:288.) But the great sultans of the fourteenth and fifteenth century not only gradually extended the Turkish domain in the Balkans and eastern Mediterranean but reduced previously powerful feudal forces, establishing the sultan's administration as the undisputed power in the land. They succeeded mainly because they fashioned a most peculiar civilian and military administration consisting almost entirely of non-native recruits unattached to the native Muslim population. At the summit of its power and splendor under Suleiman the Magnificent (1520-1566), the Ottoman state rested largely on a military and civilian staff of renegade Christians. The standing army of foot soldiers, the Janissaries; the regular mounted regiments of the household, the Sipahis of the Porte; as well as the personnel of the sultan's court, his key advisors and administrative aides, had all the status

of *Kul*, slaves of the sultan. None were native-born Muslims.

Slaves were partly acquired in the usual way, through capture or purchase. But since the late fourteenth century the Ottoman Empire resorted to a social invention not to be found in other Bureaucratic Empires: periodical levies (*devsirme*) of young male children of the sultan's Christian subjects. These tribute boys were carefully trained, converted to the Muslim faith, and used as mainstays of the standing army and administrative system. Around three thousand tribute boys were annually recruited, and the whole number at any one time is estimated to have been around eighty thousand (Lyber, 1966:49).

The children so enrolled were completely dependent on the sultan. They were cut off from their parent and kin, and had almost no contact with the native Muslim population. However, though technically slaves, they were not socially inferior. They were aware that they constituted the main support of the sultan's regime and that they monopolized state positions from which free-born Muslims were excluded. Like European aristocrats, these "slaves" were immune from taxation and responsible only to their own courts and the sultan himself.

Upon joining the sultan's great slave household the young Christian recruits were examined and classified by highly trained officials. The great majority, about ninety percent, were sent to Asia Minor to serve with feudal landowners, to learn the language, to master the arts of war and to be converted to Islam. On their return to the capital they entered one of the great military corps. Meanwhile some ten percent of the total, selected for appearance and ability, were separated from the rest and trained as pages in the Palace Schools to become administrative leaders of the Empire and courtiers of the sultan. After graduation they first assumed minor posts in the provinces and then climbed the ladder of hierarchy. As one historian puts it (L. S. Stavrianos, 1958:85) "The net result of this remarkable system was that a great Muslim empire was based upon Christian brawn and Christian brain." Between 1453 and 1623 only five of the forty-seven grand viziers were of Turkish origin. The Ottoman

system took boys from the cattle-run and the plow and made them courtiers, administrators and army officers (Lybyer, 1966: 45 ff). For Christian renegades, though not for native Muslims, the career was open to all talent. As an overawed Hapsburg ambassador observed at the time of Suleiman (Stavrianos, 1958:85):

In Turkey every man [read every slave, L.A.C.] has it in his power to make what he will of the position into which he is born and of his fortune in life. Those who hold the highest posts under the sultan are very often the sons of shepherds and herdsmen. . . . Thus among the Turks, dignities, offices, administrative posts are the rewards of ability and merit. . . . This is why the Turks succeed in all they attempt . . . Our method is very different: there is no room for merit, but everything depends on birth. . . .

Christian renegades in every branch of service were paid directly and in cash by the sultan and were never provided with income-producing fiefs, as was the case with officers in the feudal army, the provincial Sipahis. Hence, they could not build an independent basis of support. They remained as the seventeenth-century observer Paul Ricaut remarked, "strangers and foreigners in the land they ruled" (Lybyer, 1966:119). Their tenure was like that of officials in contemporary Russia, always uncertain. Moreover, no office was hereditary. In fact, the reverse of the hereditary system prevailed; the sons of ex-Christian slaves were normally excluded from employment in the imperial system which rested almost exclusively on the first generation of renegade Christians. Every man, then, was "his own ancestry"; and honor and prestige rested exclusively on achievement (Lybyer, 1966:119).

The members of the standing army, almost all Christian renegades, were effectively insulated from the rest of the population. They were paid in cash, as has been mentioned; but in addition they were kept ignorant of money-making crafts during their training, and forbidden to engage in such crafts at later stages in their career. Hence, they could not accumulate property (Gibb and Bowen, 1950:63). The government, moreover, acquired army supplies directly from producers, without resort to civilian middlemen, further shielding the army from contamination by civilian society

(Gibb and Brown, 1950:63). The slave army was a state within the state.

Janissaries were forbidden to marry before their retirement, though this prohibition was not always enforced. Sipahis of the Porte married late, if they married at all. This of course further insulated them from the native population. A number of contemporary observers, viewing the warriors' celibacy, were moved to note the monastic aspects of the system (Lybyer, 1966:70). In any case, the standing army was exclusively based on first-generation Christian renegades; and whatever children they may have sired were absorbed by the native Muslim population.

The Ottoman system at its height combined the advantages of ascription and achievement in filling positions. The native population remained tied to the soil, domesticated by traditional Muslim religious and legal institutions, and largely ruled by a native fief-holding service nobility that kept them in submission. The mainstays of the regime, in contrast, were rootless aliens entirely dependent on the sultan. The slave administration, the slave army and the slave court guarded the sultan from overthrow by the subject population and freed him from reliance on popular support while assuring wide open careers for the talented among his servants.

The slave administration freed the sultan from the drawbacks to which ascriptive and hereditary office holders are invariably prone: there could be no deadwood among the eager and hungry young recruits who must all have felt that they carried a marshal's baton in their knapsack, though the sword was never far from their neck. As a contemporary observer put it:

Being all slaves by condition, and slaves of a single lord, from whom alone they hope for greatness, honor, and riches, and from whom, on the other hand, they fear punishment, chastisement, and death, what wonder that in his presence and in rivalry with each other they will do stupendous things. (Quoted in Lybyer, 1966:65.)

The lowest Janissary had a chance to rise either through promotion in his corps, or by being transferred into the cavalry or administrative office. Expansion of the Empire through conquest ever opened room at the

top; and the losses from continuous warfare as well as frequent dismissals from office and executions, enlarged the chances of upward mobility (Lybyer, 1966:83).

Christian renegades were loyal to the sultan since he was their only support and since they were effectively barred from contact and commerce with the native population. With the advancements offered, they could well aspire to the highest military and civilian offices. Career aspirations were hence effectively harnessed to the functional requirements of the regime.

To this may be added an aspect of the psychological make-up of the renegade to which Simmel (1950:383) draws attention.

He exhibits a characteristic loyalty to his new political, religious, or other party. The awareness and firmness of this loyalty (other things being equal) surpasses those of persons who have belonged to the party all along. . . . [Janissaries] were the most loyal and energetic subjects. The special loyalty of the renegade seems to me to rest on the fact that the circumstances under which he enters the new relationship have a longer and more enduring effect than if he had naively grown into it, so to speak, without breaking with a previous one.

The renegade is, as it were, forever on trial. He must continually prove himself worthy of his new status and standing. Just as Weber's Calvinist must forever attempt to place himself among the elect, so the renegade must justify himself in his new master's eyes by devotedly and energetically executing entrusted tasks.

Hence not only self-interest made the Christian renegade strive to advance but the awareness of his status as outsider. With one exception, administrative positions were staffed by Christian renegades. And this exception significantly strengthens the sociological rule. The inner household of the sultan as well as the harem were staffed not by ordinary slaves, but eunuchs, some African, some Christian in origin (Lybyer, 1966:56 ff and 123 ff). For the most intimate functions performed for the sultan, to be a Christian renegade was not enough. These positions were reserved for men undeflected by sexual ties from devotion to the sultan. When it came to tending the holiest of holy, his inner sanctuary, the sultan relied on men whose alienation was twice com-

pounded; they were not only strangers in the land, but powerless to bond sexually with its inhabitants (Coser, 1964).¹

SUMMARY AND CONCLUSION

I have attempted with two historical examples, the Court Jews of Baroque Germany and the Christian renegades of the Ottoman Empire at its height, to illustrate the sociological point that whenever rulers are greedy for power, whenever they wish to maximize their autonomy in the face of feudal, bureaucratic, or other impediments, they tend to avail themselves of the services of alien groups rootless in the country they rule. The alien, I have argued, is easily bent to the ruler's purposes and an ideal servant of power.

I leave it to the reader's sociological imagination to conjure up other cases past or present where this approach might bear sociological fruit.

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¹ The Ottoman Empire began to decline when after the reign of Suleiman feudal principles again asserted themselves, and the Janissaries and other army units began to be staffed by native Muslims rather than renegade Christians. Ascription, and corruption, replaced the system based on advancement by merit; and the Empire gradually became the "Sick Man of Europe."

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REWARD DISTRIBUTION AND WORK-ROLE ATTRACTIVENESS IN THE KIBBUTZ—REFLECTIONS ON EQUITY THEORY¹

EPHRAIM YUCHTMAN

Tel-Aviv University

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The problematic nature of role attractiveness among office-holders in Israeli Kibbutzim is examined by comparing a representative sample of managers and workers of a typical production unit in the Kibbutz. The results indicate that, despite enjoying greater intrinsic and extrinsic rewards, managers tend to be less attracted than the rank-and-file to their jobs. This finding is explained by equity theory, i.e., the managers do not get out of the job what they should on the basis of what they have put into it. The apparent penetration of equity to the egalitarian-oriented Kibbutz is explained in light of its continuous processes of role differentiation and professionalization. The relevance of such processes for the equity argument is further indicated by the analysis of related data. The article concludes with some suggestions concerning possible extensions of the equity principle and the social conditions of its applicability.

THE differential allocation of rewards has been a major factor accounting for variations in the attractiveness of different roles for individuals. To be sure, the social psychological processes underlying the operation of rewards are not yet fully understood, and different parameters

have been offered as mediating the effects of rewards on attitudes and behaviors. The concepts of value importance (Vroom, 1964; Evans, 1969; Mobley and Lock, 1970), status consistency (Homans, 1961; Brandon, 1965; Sampson, 1968), expectancy (Vroom, 1964; Lawler, 1971) and equity (Homans, 1961; Jacques, 1961; Blau, 1964; Adams, 1965) illustrate the importance of such variables, particularly as they apply to the work situation. Yet, the general positive influence exerted by rewards on attitudes toward roles with which they are associated is commonly accepted

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on both theoretical and empirical grounds. Thus, the finding of a positive relationship between attraction to work roles and their occupational or organizational levels is largely explained by the greater rewarding potential of roles of higher positions (Herzberg, 1957, 1958; Vroom, 1964; Robinson *et al.*, 1969; Quinn *et al.*, 1971).

In view of these general trends it is interesting to note that members of Israeli Kibbutzim are not particularly attracted to higher positions in the community (Garber-Talmon, 1956; Spiro, 1956; Darin-Drabkin, 1962; Ben-Yosef, 1963; Leon, 1969). Furthermore, it has been claimed that candidates for central roles (usually managerial) often attempt to avoid their nomination, and public pressure is sometimes required for the candidate to take the job (Garber-Talmon, 1956). Indeed, expressed dissatisfaction among candidates to and occupants of higher positions may contain elements of ritualistic behavior. But students of the Kibbutz generally agree that, while instances of ceremonial disinterestness do occur, their overall importance should not be exaggerated. Thus, it remains to be explained why the expected positive relationship between the hierarchical position of the work role and its attractiveness is not obtained in the Kibbutz.

On first examination this peculiar phenomenon seems to be simply explained by the egalitarian nature of the Kibbutz. The principle of economic and social equality has been a vital ingredient of the Kibbutz' value-system since its beginning. At the societal level this principle is expressed in the collective structure of the community in general and in its virtually complete abolition of the institution of private property in particular.² A crucial mechanism for maintaining this egalitarian structure has been the denial of a different allocation of material and economic rewards for occupational and organizational positions (Rosen-

feld, 1951; Aurbach, 1953; Spiro, 1956; Talmon-Garber, 1956; Darin-Drabkin, 1962; Vallier, 1962; Ben-David, 1964; Weintraub *et al.*, 1969). Thus, one of the most important sets of incentives of higher level jobs is absent in the Kibbutz. Furthermore, as several researchers note (Spiro, 1956; Talmon-Garber, 1956; Rosner, 1964), office holders in the community tend to experience unpleasant encounters and difficulties causing them high levels of strain. Typical examples are the frequent exposure to criticism from fellow workers, including subordinates; the need to depend on committee consensus for important decisions; the need to rely almost exclusively on skills of personal persuasion and informal leadership, since formal sanctions are scarce; and labor days which spread into the night. These instances of negative outcomes built into higher positions, can be viewed as costs reducing the attractiveness of such jobs (Rosner and Abend, 1968).

This argument is much weakened, however, by evidence which indicates that office holding in the Kibbutz carries significant privileges and rewards. Several studies show that leaders usually enjoy great influence, authority, and esteem (Rosenfeld, 1951; Diamond, 1957; Schwartz, 1957; Darin-Drabkin, 1962). Some investigators suggest, in fact, that this hierarchical reward differentiation has formed an elite stratum in the Kibbutz (Revusky, 1945; Rosenfeld, 1951; Aurbach, 1953; Etzioni, 1959; Vallier, 1962). Other researchers, while rejecting this argument (Talmon-Garber, 1956; Darin-Drabkin, 1962), still acknowledge the significant rewards of office holding in the Kibbutz.

In addition to the social rewards, attention has been given to the intrinsic sources of gratification in higher positions. The latter tend to be more interesting and challenging, to provide better opportunities for using and developing skills and knowhow, and to allow for greater autonomy and responsibility on the job. Increased complexity in the division of labor in the Kibbutz, and the concomitant processes of specialization and professionalization have increased differentiation in the work role hierarchy for both social rewards and their task incentives.

² During its earlier period the Kibbutz implemented the idea of collective sharing vigorously in both the production and consumption spheres (Diamond, 1957; Rosenfeld, 1957; Talmon-Garber and Stupp, 1970). The emphasis on total communal sharing of consumption goods has gradually been weakened (Rosenfeld, 1957; Rabkin and Spiro, 1970), though the collective ownership of all means of production has been fully maintained.

In view of this evidence one cannot argue simply that higher positions in the Kibbutz are relatively unattractive because they lack meaningful rewards and incentives. Previous research is far from conclusive on whether office holders are remunerated more or less than the rank-and-file workers. The ambivalence toward this issue is illustrated by the following two statements of Spiro (1956):

There is no class structure in Kiryat Yedidim, and there is no differential reward system for different kinds of labor based on some ranking technique. Some kinds of work, as has already been observed, are valued more highly than others; but those who occupy the more highly valued jobs receive no greater reward than the others. The important psychological fact about the Kibbutz culture is that everyone, regardless of his work, is viewed as a worker, with the same privileges and responsibilities as anyone else (pp. 23-4).

Although the various Kibbutz offices are held on a temporary and a rotation basis, those who happen to hold these offices do enjoy considerable power. Moreover, as is noted below, though the tenure of office is limited to two or three years, only a small number of chaverim possess the necessary skills required to cope with the complexities of such offices . . . so that in effect these offices rotate among a small core of twelve to fifteen persons (p. 25).

The relation of role attractiveness to reward structure is difficult to understand for two related reasons. First, the relationship between reward distribution and job satisfaction has been examined only indirectly, with the relevant variables as yet empirically unassessed. Inadequate or unreported sampling procedures weaken the significance of many of these studies. In one exception (Rosner, 1964), a sample of managers and rank-and-file workers were carefully compared; but the degree of role attractiveness among the two groups was not similarly measured. Second, the tendency has been to treat managerial and other higher positions homogeneously. However, considerable vertical and horizontal differences exist among these roles (Talmon-Garber, 1956; Cohen, 1963; Rosner and Abend, 1968). Thus, an examination of each type of office is required for meaningful generalizations to be made about both problems of role attrac-

tiveness and reward distributions of higher positions in the Kibbutz.

The present study attempts to overcome these difficulties and to understand the relation of reward distribution to role attractiveness in the Kibbutz. At the empirical level, the investigation compares a representative sample of managers and rank-and-file workers of a widely-spread production unit—the poultry branch³—with respect to the amount and types of rewards enjoyed as well as the degree of role attractiveness reported. From a theoretical view, the study examines the socio-psychological principles which may underlie the effect of the work role's rewards on its attractiveness for incumbents. The hope was that the empirical data would shed light on whether the egalitarian principle, successfully institutionalized in the economic sphere, is similarly accepted as the rule of reward distribution for non-material rewards. The importance of this question goes beyond its relevance for the study of the Kibbutz. Recent advancements in exchange theories (Thibaut and Kelley, 1959; Gouldner, 1960; Homans, 1961; Blau, 1964; Adams, 1965; Meeker, 1971) have renewed the interest in justice as a crucial dimension of the reward distribution process. Most empirical research in this area, however, has concentrated on Adams' concept of equity as the predominant principle in modern societies, though we do not know as yet how universal it is. Some researchers point to limiting conditions of its applicability (Gamson, 1964; Weik, 1966, 1967; Sampson, 1968; Goodman and Friedman, 1971). Both Gamson and Sampson propose that equality rather than equity may, under certain conditions, be the prevailing rule. In light of this debate the present investigation, and the

³ The economic structure of the Kibbutz consists of various organizational units—known as "branches"—each specializing in the production of certain goods or services. A typical Kibbutz includes several agricultural branches, such as field crops or plantations, and service units, like the kitchen or the clothing. In addition, many Kibbutzim have one or more factories, as well as a few more unique branches. Each unit is headed by a manager nominated by the appropriate community organs. While the nomination process may vary from one Kibbutz to another, it almost always involves the implicit consensus of workers in that branch.

study of the Kibbutz in general may help develop general theories of social exchange.

Method

Population. The data for the present investigation are based on a probability sample representing the entire population of the poultry branch in the Kibbutz movement, with the exception of branches comprising single full-time workers. The sample consists of twenty-six Kibbutzim (one poultry branch per Kibbutz) and comprises 130 respondents, divided into twenty-six managers and 104 workers, all full timers and members of the Kibbutz.⁴ Data were collected via self-administered questionnaires delivered personally to all the workers who could be located and who agreed to cooperate. A little over 80% of the original sample returned complete, or nearly complete, questionnaires.

The choice of this particular population was not determined by the subject matter of the present paper. It should be noted, however, that the poultry is one of the most widely-spread branches in the Kibbutz movement, existing in over 75% of the total of some 230 Kibbutzim. In addition, there is little variation in the technology of the branch across Kibbutzim, a fact which contributes to this branch's similarity of tasks and organizational structure in different Kibbutzim.⁵ This organization was found to be rather simple, consisting usually of a branch manager and rank-and-file workers.

⁴ In most Kibbutzim there exist, in addition to regular workers, several other categories of employees. For example, children of the Kibbutz spend several hours a day at work in various branches. Similarly, in many Kibbutzim one finds non-members, such as volunteers from abroad. Hired labor, used by many Kibbutzim, is quite rare in the poultry branch. In any case, all these categories of workers, who comprise a small fraction of the labor force of this branch, were excluded from the investigation.

⁵ The poultry branch is a market-oriented organization, based on the application of scientific know-how and modern technology, including various types of machinery, semi-automated equipment, quality control procedures and an elaborated system of medication—all of which require relatively sophisticated technical, administrative and marketing skills. Hence, in the medium and large branches these functions are fulfilled by full-time managers.

The size of the poultry branch is also typical, averaging about five full-time workers. In the few groups where the number of workers exceeds ten, the organizational structure is slightly more complex, including an intermediate level of supervisory roles. Since few such cases exist and both managers and rank-and-file workers judge them as unimportant, we decided to use the dual distinction between managers and workers.

Variables

A. Reward Distribution. On the basis of previous research on the Kibbutz, including a pilot study, three clusters of rewards, each comprising three specific outcomes, were selected for comparing managers and workers:

1. *Intrinsic job satisfaction;*
 - 1a. the opportunities to learn new things
 - 1b. the opportunities to actualize personal ideas
 - 1c. the opportunities to make use of skills and knowhow
2. *Power-related rewards;*
 - 2a. degree of influence over what is going on in the branch
 - 2b. degree of authority over other persons
 - 2c. degree of independence in the work process
3. *Socio-emotional rewards;*
 - 3a. amount of esteem received by fellow workers
 - 3b. amount of consideration shown by fellow workers
 - 3c. the quality of social relations in the branch⁶

The above nine outcomes were measured by close-ended questions, having five alternatives ranging from "very high" to "very low." These questions were intended to measure the amount of rewards obtained by each individual in his view.

In addition, another form of measurement was used to examine the validity of the findings. All the respondents were presented with a list of various potential outcomes and asked to rank-order the three most often associated with the role of the branch manager. An alternative indicating that the managers enjoy no special advan-

⁶ This is an indirect question, based on the assumption that the perception of the quality of social relations in the group reflects, to a large degree, the social position of the person in it.

tages was also provided. The list of outcomes is given below:

1. higher esteem
2. greater job variety
3. more independence
4. better opportunities for social contacts
5. better opportunities to make use of and develop skills
6. better opportunities to make use of and develop knowhow
7. more influence in the branch

Note that unlike the first, the second form of measurement explicitly focuses the respondents' attention on the issue of reward distribution in general, and on the manager's relative advantage with regard to this distribution in particular.

B. *Work-Role Attractiveness*. Following Thibaut and Kelley (1959), role attractiveness was conceived in terms of two related but distinct aspects. The first is implied by their concept of "comparison level." Accordingly, the amount of satisfaction derived from a given interaction reflects the gap between the actual outcomes and those expected on the basis of a subjective yardstick developed by the person for that interaction. The second concept—"comparison level for alternatives"—implies another subjective yardstick used by the individual to evaluate his outcomes. This yardstick determines the gap between the actual outcomes and those expected from

the best available alternative. Thus, a person may be dissatisfied with his role in terms of the comparison level but still prefer to stay in it since his outcomes are positive in terms of the comparison level for alternatives.

The questions used to measure these two aspects of the role attractiveness were formulated as follows:

1. "All and all, how satisfied are you with your job?" The five alternatives of response range from "very much satisfied" to "very little satisfied."

2. "To what extent do you prefer to stay or leave this branch?" The alternatives to this question ranged from "definitely prefer to stay" to "definitely prefer to leave."

A product-moment correlation of 0.56 was obtained between the two measures, indicating the common factor they share as well as their distinct aspects. In view of this result and the conceptual distinction it reflects, we decided to keep the two measures separate rather than combine them in a single index.

Results

The distribution of rewards. Table 1 compares the managers and workers with respect to the distribution of intrinsic rewards. The findings indicate a clear advantage of managers over workers. Thus,

Table 1. Distribution of Intrinsic Rewards by Workers and Managers (%)

Rewards Degree	Opportunities to Learn New Things ¹		Opportunities to Use Personal Ideas ²		Opportunities to Apply Skills and Knowhow ³	
	Workers (N=97)	Managers (N=24)	Workers (N=99)	Managers (N=24)	Workers (N=100)	Managers (N=24)
Very high	10.3	29.2	11.2	25.0	12.0	29.2
High	29.9	37.5	35.3	58.3	37.0	20.8
Medium	38.1	25.0	35.3	12.5	39.0	45.8
Low	16.5	8.3	15.2	4.2	8.0	4.2
Very low	5.2	0.0	3.0	0.0	4.0	0.0
Total	100.0	100.0	100.0	100.0	100.0	100.0

¹Gamma = 0.45

²Gamma = 0.56

³Gamma = 0.19

Table 2. Distribution of Power-Related Rewards by Workers and Managers (%)

Rewards Degree	Authority over Other Persons ¹		Independence in the Work Process ²		Influence in the Branch ³	
	Workers (N=100)	Managers (N=24)	Workers (N=102)	Managers (N=25)	Workers (N=97)	Managers (N=24)
Very high	13.0	20.8	23.5	16.0	13.3	37.5
High	25.0	45.8	33.3	60.0	33.0	50.0
Medium	26.0	25.0	31.4	24.0	31.0	12.5
Low	18.0	8.4	5.9	0.0	17.5	0.0
Very low	18.0	0.0	5.9	0.0	5.2	0.0
Total	100.0	100.0	100.0	100.0	100.0	100.0

¹Gamma = 0.46²Gamma = 0.22³Gamma = 0.66

about 67% of the first group perceive large, or very large, opportunities in their jobs to learn new things; while only 40% of the workers perceive their jobs in the same way. Similarly, about 83% of the managers, compared to 47% of the workers, indicate that their jobs enable them to actualize personal ideas. The differences with respect to skills and knowhow are much smaller but in the same direction as those obtained for the first two outcomes.

Table 2 presents the findings for the power-related rewards. These data reveal a pattern very similar to the previous set: the

distribution of outcomes clearly favors the managers on the three measures, particularly those of authority and influence. Thus, the large majority of this group (two-thirds and over) feel that they have high degrees of authority over other persons, independence in the work-process and overall influence in the branch. The corresponding figures for the workers are much smaller. These results should be evaluated in light of the argument that insufficient authority serves as a source of role strain among office holders in the Kibbutz. At least for the managers of this branch, the present data

Table 3. Distribution of Socio-Emotional Rewards by Workers and Managers (%)

Rewards Degree	Esteem by Co-workers ¹		Consideration and Understanding by Co-workers ²		Quality of Social Relations in the Branch ³	
	Workers (N=101)	Managers (N=22)	Workers (N=101)	Managers (N=25)	Workers (N=103)	Managers (N=23)
Very high	34.7	27.3	16.8	16.0	4.8	4.3
High	29.7	40.9	31.7	28.0	34.9	30.4
Medium	26.7	27.3	41.6	40.0	47.6	56.5
Low	8.9	4.5	6.9	16.0	11.7	4.3
Very low	0.0	0.0	3.0	0.0	1.0	4.3
Total	100.0	100.0	100.0	100.0	100.0	100.0

¹Gamma = -0.01²Gamma = -0.08³Gamma = -0.04

Table 4. Overall Ranking of the Advantages of Branch Managers, by Workers and Managers (%)^{*}

Advantages--Workers (N=98)		Advantages--Managers (N=24)	
1. Influence	52.0	1. Influence	54.2
2. Knowhow	41.8	2. Variety	45.8
3. Variety	36.7	3. Knowhow	41.7
4. Independence	35.7	4. Independence	37.5
5. Social Contacts	27.5	5. Social Contacts	20.8
6. Esteem	22.4	6. Esteem	16.7
7. Skill Development	14.3	7. Skill Development	4.2
No advantages	12.2	No advantages	16.7

^{*}Since respondents could mention up to three advantages, the total percentage exceeds 100.

neither support nor disprove the argument.⁷

Table 3 presents the allocation of the socio-emotional rewards. The findings here differ considerably from the earlier results in that the distribution among managers and workers is nearly identical on all the three measures. Managers and workers are alike in their perceptions of the esteem and the understanding given them by fellow workers, as well as in their evaluation of the quality of social relations in the branch. Thus, the position of the managers on this dimension is lower than their standing on the other two. At the same time, these data do not show that the managers are particularly prone to experience negative interpersonal relations, or to be disliked by their fellow workers, as has been claimed. Altogether, the findings thus far indicate that the position of a manager in the poultry branch is associated with several important positive outcomes not shared to the same degree by rank-and-file workers.

Table 4 shows the results of the additional examination of the rewards of the managerial office. This table reveals three important findings:

(1) Only a small portion of respondents indicate that the managerial position is de-

void of any advantage (12% and 17% of the workers and managers, respectively). (2) A high degree of agreement exists between the perceptions of workers and managers. With the exception of the alternation between knowhow and job variety, the two rankings completely overlap. In fact, the absolute figures are also very similar. (3) the findings seem to be consistent with the previous results. Power-related and intrinsic rewards appear in the upper portion—the socio-emotional in the lower portion, except for skills development.

In general, both sets of findings seem to indicate that the managers of the poultry branch, as compared with the rank-and-file, enjoy a significant increment of rewards of various kinds, particularly those related to self actualization and the exercise of influence and authority.

Work-role attractiveness. Tables 5 and 6 give the distributions of responses to the two questions concerned with role attractiveness. Both tables show that the managers are *less* attached to their work role than the workers. About 66% of the latter report high degree of satisfaction with their job, compared with 42% among the managers; and while only 17% of the workers prefer to leave the branch, 42% of the managers prefer to leave. It is interesting to note also the difference of scatter between the two tables. The responses to the measure of job satisfaction tend to concentrate around

⁷ Thus, only 21% of the managers define their degree of authority as "very high." It is possible, therefore, that lower degrees of authority, which characterize the majority of this group, are a source of tension in it.

Table 5. Degree of Job Satisfaction by Workers and Managers (%)^a

Degree	Workers (N=101)	Managers (N=24)
Very high	21.8	8.3
High	44.6	33.3
Medium	23.7	50.0
Low	4.0	4.2
Very low	5.9	4.2
Total	100.0	100.0

*Gamma = -0.35

the middle and the adjacent upper category, while the distribution of the second variable tends to polarize among the managers. This finding can be explained by assuming that managers have better job alternatives than workers; thus, dissatisfied managers are more inclined to relinquish their jobs than dissatisfied workers. In any case, these results indicate the problematic nature of role attractiveness for leaders in the Kibbutz. To assess the scope of generality of this phenomenon we need to examine other higher positions. For the poultry branch, it appears that the managers, while enjoying more rewards than the rank-and-file, are less attracted to their work role.⁸

Discussion

The relative dissatisfaction of managers, despite their obvious advantage in rewarding outcomes vis-a-vis the workers, can be explained by the equity concept. As formulated by Adams (1965), this concept draws on earlier works, particularly those of Jacques (1956), Sayles (1958), Patchen (1959), and Homans (1961), in which the notion of justice is introduced as a critical dimension of exchange processes and their consequences for participants' attitudes and behaviors. Inequity, according to Adams,

⁸ The significance of this apparently peculiar phenomenon may be put in question by the suggestion that the managers do not attach much importance to the outcomes they enjoy. However, additional data which will be presented later rule out this possibility.

Table 6. Inclination to Stay in the Branch or Leave It by Workers and Managers (%)^a

Inclination	Workers (N=104)	Managers (N=26)
Definitely to stay	44.2	41.7
Incline to stay	31.7	12.5
Indifferent	7.7	4.2
Incline to leave	9.6	29.2
Definitely to leave	6.7	12.5
Total	99.9	100.1

*Gamma = -0.22

"exists for Person whenever he perceives that the ratio of his outcomes to inputs and the ratio of Other's outcomes to Other's inputs are unequal" (1965:280). Inequity results usually in a feeling of dissatisfaction, although the theory provides for predicting additional consequences, such as the altering or cognitive distortion of outcomes and/or inputs. Note that the bases of the comparison processes referred to in the theory are rather limited and, as Weik showed (1966, 1967), there exist several potential alternative modes of comparison, including an individual's use of his own internal standards. For the present discussion, however, the relevant theme of equity is that "people try to get out of an exchange what they think they deserve on the basis of what they have put into it" (Meeker, 1971:487). Accordingly, the managerial role has a lower degree of attractiveness for its incumbents because they put more into it than they get out. The fact that managers are rewarded better than the rank-and-file is irrelevant because the managers, whether comparing their own ratio with the workers' or using an alternative yardstick, presumably evaluate their outcomes with comparison levels of higher order. This interpretation becomes more meaningful in light of added evidence on general trends in the Kibbutz movement as well as other findings in this study.

In the last two decades the Kibbutz has undergone rapid technological and economic changes, followed by greater role differentia-

tion in the economic and organizational spheres. These processes which were systematically described and analyzed, especially by Cohen (1963, 1966, 1972), stemmed from the Kibbutz' own goals of economic and material progress as well as from the pressing problems of economic and technological development in Israeli society. As a result, "... the Kibbutz was pressed to persist in the constant effort to expand, improve and modernize its technology and economy, its educational facilities and its organizational structure" (Cohen, 1966:184). An integral aspect of these processes has been the specializing and professionalizing of occupational and administrative roles. But the concern with progress and its dynamics of change have inevitably weakened the Kibbutz communal spirit. As Cohen indicated, an inherent conflict exists between the idea of progress, with its emphasis on performance, rationality and objectivity and the spirit of communality, which rests on highly personal, intimate and fraternal modes of social relations. Thus, in its preoccupation with adapting and developing, the Kibbutz could not escape the inevitable consequence of a decline in its collective spirit and a strengthening of individualism. Cohen's analysis of the dynamics of change in the Kibbutz is highly consistent with the two-factor model of social structure and behavior developed recently by Sampson (1968). One proposition derived from this model concerns the compatibility between equity and equality as alternative modes of distributive justice, on the one hand, and the group's relative emphasis on problems of adaptation and goal attainment ("mastery" in Sampson's terms) on the other.

One immediate and rather interesting implication of this formulation suggests that to the extent that task performance is directly affected by the problems of mastery, task performance will suffer *more* (at least for a time) where justice is to be accomplished through equality than where justice can be achieved through equity (Sampson, 1968:266).

Cohen's specific analysis and Sampson's conceptual framework thus indicate the social changes conducive to the emergence of equity as an alternative to equality in certain areas of Kibbutz life. The validity of

this argument is revealed by examining the extent to which the Kibbutz has successfully implemented the rule of rotation. This rule has been a major mechanism in the community for maintaining the egalitarian principle. Rotation deemphasizes potential input differences by role incumbents, to allow for recruiting office holders from a large pool of candidates. With increased role differentiation, however, this rule has become harder to adhere to, particularly for technicians and managers.⁹ The limited application of rotation can be viewed in two complementary ways: on the one hand, it is an outcome of the system's need to progress economically through an efficient division of labor; on the other hand, it further differentiates higher and lower positions and their occupants. Furthermore, it is reasonable to assume that as the same persons tend to hold the higher offices, they are more likely to be aware of the distinctiveness of their positions, including the requisite qualifications. In other words, they recognize that professionals are distinguished by such features as intellectual ability and technical skill. The salience of these differences enhances the likelihood of equity, rather than equality, to prevail as the mode of reward distribution.

The discussion so far suggests that the Kibbutz structure is inclined to incorporate the equity principle in its reward structure. It remains to be seen if this general analysis applies to the poultry branch. To explore this question, we will present findings on differences in input levels and manager and worker concern about outcomes.

The first two findings, shown in Tables 7 and 8, can be viewed as unobtrusive measures of both the professional level and the managers' selectivity. Table 7 compares the amount of formal technical training received by managers and rank-and-file. As this table shows, a significantly higher proportion of branch managers participated in each type of professional training available for this branch. The largest difference is obtained for the course on "branch management" which prepares technical and administrative

⁹ Spiro, for example, reports that only twelve to fifteen members rotated among themselves the highest positions in the Kibbutz he studied (see quote on page 65).

Table 7. Professional Training of Workers and Managers (%)^{*}

Type of Training	Workers (N=104)	Managers (N=25)
Seminars	52.9	72.0
Elementary course on poultry	40.4	48.0
Advanced course in branch management	9.6	52.0
Other	20.1	28.0

^{*}Since some respondents participated in more than one type of training, the percentages in the two columns do not add up to 100.

experts for branches in the Kibbutz¹⁰ and is the most advanced course for this management level. Note also that managers express a stronger desire than workers for education and technical training. About 38% of the managers indicate a strong desire for this opportunity compared to 15% of the workers. It appears, then, that managers are characterized by their objective professional knowhow as well as by a desire to improve that knowhow.

The second relevant measure pertains to the respondents' amount of participation in several central committees and offices typical to the Kibbutz. Nomination to these positions is based mostly on ability and performance, thus membership in them indicates the members' high standing in terms of these criteria. The five comparisons given in Table 8 reveal a consistent pattern of a higher proportion of managers who are appointed to the committees and offices.¹¹ Note especially the economic committee which demonstrates the largest gap between the two groups. This organ is responsible for the major economic decisions in the Kibbutz, and its members are mostly technical and administrative experts.

The findings reported in Tables 7 and 8 indicate that the general processes in the

Table 8. Participation in Offices and Committees (%)^{*}

	Workers (N=104)	Managers (N=25)
All types of offices	63.4	92.0
Economic committee	13.5	56.0
Secretariat	15.4	32.0
Cultural and educational committee	17.3	32.0
Chairmanship of committees	8.6	24.0

^{*}Since some respondents participated in more than one type of office, the percentages in the two columns do not add up to 100.

Kibbutz of differentiation and professionalization apply, to some degree, to the poultry branch. We argued, however, that these processes enhance awareness among office holders of the distinctiveness of their role qualifications.

To determine if managers have such an awareness, we asked them along with workers, to assess the scarcity of manpower skills required for performing their respective jobs.¹²

The distribution of responses, given in Table 9, clearly shows that managers tend to regard the skill requirements of their work role more highly. Only 8% of this group, compared to 44% of the workers, think that most people could learn their jobs. This finding is consistent with responses to the question which required sub-

¹²Specifically, they were asked to estimate the proportion of members who could perform the job, had they time enough to learn it.

Table 9. Estimation of How Many Members Can Learn Own Job (%)

	Workers (N=100)	Managers (N=24)
Almost everyone	44.0	8.3
Many	43.0	58.4
Few	13.0	33.3
Total	100.0	100.0

¹⁰It should be noted that the participants in this course, which takes place in a special educational institution established by the Kibbutz movement, are sent by their Kibbutz on the basis of their potential or actual merits as branch managers.

¹¹Note that the nominations of branch managers are not ex officio.

Table 10. Desirability of Intrinsic Rewards by Workers and Managers (%)

Rewards Desirability	Learning New Things ¹		Use of Personal Ideas ²		Actualization of Skills and Knowhow ³	
	Workers (N=98)	Managers (N=24)	Workers (N=94)	Managers (N=24)	Workers (N=91)	Managers (N=24)
Very high	32.6	50.0	21.3	33.3	24.1	50.0
High	43.9	45.8	38.3	62.5	40.7	41.7
Medium	15.3	4.2	31.9	4.2	29.7	8.3
Low	4.1	0.0	5.3	0.0	5.5	0.0
Very low	4.1	0.0	3.2	0.0	0.0	0.0
Total	100.0	100.0	100.0	100.0	100.0	100.0

¹Gamma = 0.42²Gamma = 0.53³Gamma = 0.55

jects to assess the length of time needed for a person to learn his job.¹⁸ The average length was 30.1 and 11.8 weeks, as estimated by managers and by workers, respectively. Taken together, these results tend to support the contention that managers are more professional and selective and are conscious of the higher input levels associated with their roles.

The second aspect of the equity argument treats the concern about the work role outcomes. We expected that in view of the input differences between workers and mana-

gers, the latter would attach greater importance to the rewarding outcomes of their role. Accordingly, the two groups were compared with regard to the two sets of intrinsic and power-related rewards. You will recall that the managers' advantage was demonstrated for these types of outcomes, so that to see the outcomes' significance for them is particularly germane. The results of this comparison, in Tables 10 and 11, show consistently that managers regard both sets of rewards more highly. Thus, while majorities in the two groups attach more importance to the intrinsic job rewards, this tendency is clearly stronger among the managers. A similar pattern ob-

¹⁸ The question was formulated as follows: "How many weeks are required for an average member to learn to perform your job?"

Table 11. Desirability of Power-Related Rewards by Workers and Managers (%)

Rewards Desirability	Authority over Other Persons ¹		Independence in the Work Process ²		Influence in the Branch ³	
	Workers (N=94)	Managers (N=24)	Workers (N=95)	Managers (N=24)	Workers (N=100)	Managers (N=24)
Very high	4.2	8.3	30.6	37.5	22.0	50.0
High	14.9	41.7	44.2	41.7	46.0	45.8
Medium	33.0	37.5	18.9	16.7	30.0	4.2
Low	24.5	8.3	4.2	4.2	1.0	0.0
Very low	23.4	4.2	2.1	0.0	1.0	0.0
Total	100.0	100.0	100.0	100.0	100.0	100.0

¹Gamma = 0.58²Gamma = 0.13³Gamma = 0.61

tains for the desirability of power related outcomes. Note the results which treat the importance of having authority over others. This is the least preferred outcome among managers and workers, probably because of its negative ideological implications. Yet, almost three times as many managers as workers (50% vs. 19%) express strong desire for it.

Taken together, the different sets of findings strongly indicate an equity effect among managers. This group consists mainly of professionals who are aware of their higher input levels and expect, accordingly, adequate outcomes from their office. The lower degree of role attractiveness for them may be attributed, therefore, to the apparent disproportion of outcomes versus inputs. Yet, one may argue that these findings support the equity thesis only indirectly, since they pertain to the aggregate level of analysis.

To demonstrate the presence of equity more directly, one must show that, *within* the management group, variations in role attractiveness are related to differences in input and outcomes. A direct derivation from equity theory implies that the correlation between input level and role attractiveness would be negative with outcomes controlled: managers with similar outcomes would be less attracted to their office the greater their inputs. The test results for this hypothesis are presented in Table 12 which contains partial correlations between input level (indicated by skills requirements) and role attractiveness (indicated by the inclination to stay or leave the office), with the six types of outcomes separately controlled for.

As this tables shows, all six partial correlations are negative, as predicted. This latter result further demonstrates that equity considerations pervade work-role attractiveness among office holders in the present study.¹⁴ It remains to be seen how far this

Table 12. Partial Correlations between Input Level and Role-Attractiveness, Controlling for Outcomes (Managers only, N=22)

Type of Outcome	Partial Correlation
Opportunities to learn	-0.41
Opportunities to use personal ideas	-0.30
Opportunities to apply skills and knowhow	-0.27
Amount of authority	-0.30
Independence in the work process	-0.29
Amount of influence	-0.34

principle of reward distribution applies to other situations in the Kibbutz.

Conclusions and Some Implications

Any result reported here—the distribution of rewards and their importance, the attractiveness of the work role and its different inputs—can probably be accounted for by alternative theoretical principles. On the whole, however, these data appear mutually consistent and most meaningful within the framework of equity theory. At the same time, this study raises several questions unresolved by the current concept of equity. Two of these and suggestions for their clarification will be briefly discussed below.

Conditions of equity and equality. As the present discussion implies, equity and equality norms co-exist in the Kibbutz. Specifically, the Kibbutz maintains the egalitarian principle in the material sphere, and the equity principle in the non-material sphere. To understand this phenomenon, a more basic question must first be answered: Under what conditions are these two principles more likely to occur? Sampson's (1968) model and the present analysis suggest one proposition; that is, the likelihood of equity increases and that of equality decreases as the group becomes preoccupied with "external" problems of adaptation and goal-attainment, and as considerations of rationality, objectivity and efficiency dominate the

¹⁴ An alternative method of testing the equity hypothesis is to compare the correlations between outcomes and role attractiveness with the correlations between the ratio of outcomes-to-input and role attractiveness. The correlations in the second case should be, according to equity, higher than those in the first. Such an analysis was performed and the results were highly consistent with this prediction.

group's organization and use of its human and material resources.

Gamson's (1964) analysis of coalition formations contains a complementary proposition, pointing to circumstances favorable for equality and unfavorable for equity. Among alternative models of such formations, Gamson identifies one which is characterized by its "anti-competitive theory"; namely, outcomes are allocated equally regardless of individual input differences. This process is more likely to occur when problems of group solidarity are prominent so that equal sharing is accepted to maintain group integration.

The two propositions together argue that the stronger the emphasis on external rather than internal group problems—the more likely will equity prevail. On the other hand, as the group becomes more concerned with internal maintenance problems—equality will prevail over equity. These complementary propositions can account for much research concerned with the applicability of the equity principle. Most experimental work demonstrating the presence of equity involved ad-hoc groups whose members probably cared little about the problem of solidarity. On the other hand, when the groups studied were "real," as were the classmates in Morgan and Sawyer's investigation (1967), equality rather than equity was established as the dominant rule.

The two propositions can be elaborated if one recognizes that the relative dominance of equity or equality in a given situation need not apply to all types of outcomes available in the group. Equal sharing of material rewards, for example, may be vital for integrating some social groups; whereas large variations in allocating these outcomes can be tolerated in others. The family in Western culture represents a case where the egalitarian principle regulates the allocation of material rewards, while equity applies in the spheres of authority and influence. The co-existence in the Kibbutz of equity and equality can be similarly explained. Egalitarianism is maintained in the economic sphere since members agree that it is at the core of the Kibbutz' existence.¹⁵ At the

same time, the pressures of economic development and their implications for a rational division of labor, led to greater role differentiation and the acceptance of equity in distributing intrinsic and power-related rewards. Note also that the collective control of consumption makes it technically easier for the Kibbutz to regulate the flow of material rewards. The less tangible stuff, like intrinsic rewards or influence, is more closely attached to specific roles and less prone to central regulation.

Inconsistency among outcomes. Suppose we try to predict how the managers of the poultry branch can reduce their sense of inequity. Since inequity is created from a disproportional distribution between outcomes and input it follows, as Adams suggested (1965), that inequity can be reduced through increasing outcomes. In other words, so long as the ratio of outcomes to inputs is improved, inequity will decrease, regardless of the specific outcomes which are received. However, a strain toward consistency among outcomes may exist, similar to the strain toward consistency between outcomes and inputs. Under such an assumption, the pattern of available outcomes becomes highly relevant for determining how inequity can be reduced. Specifically, improving the outcomes-to-inputs ratio by increasing already large outcomes may increase rather than decrease inequity. The latter is expected to decrease only through an improvement in those outcomes toward which a person has a relatively low standing.

As for the managers in this study, two different predictions can be made for reducing inequity among them from this discussion. According to the present formulation of equity, the amount of inequity among managers will be reduced if they get more of any of the three sets of outcomes examined here. On the other hand, if the assumption about the strain toward consistency is valid, only an improvement in the socio-emotional outcomes will decrease inequity.

The theoretical relevance to each other

¹⁵ In a recent convention of one of the three major sectors of the Kibbutz movement, one of the several hundred delegates proposed to intro-

duce some form of a material incentive system for enhanced productivity. The proposal was overwhelmingly rejected.

of the concepts of consistency and equity has emerged from the studies of Branden (1965) and Kardush (1968), and been joined in the same conceptual framework by Sampson (1968). Such an approach is useful in integrating phenomena which may otherwise appear mutually independent. The extension of the equity concept, implied by the proposition advanced here, is consistent with this approach and deserves to be tested empirically.

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A SPECIFICATION OF RELATIONSHIPS BETWEEN JOB COMPLEXITY AND POWERLESSNESS *

BILL TUDOR

Central Michigan University

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Relationships between feelings of powerlessness and three types of job complexity are found to be statistically significant but weak in a representative sample of American men. Meaningfully strong relationships in a proportional reduction in error (PRE) sense are shown to exist only for a minority of men, which is specified by occupation, intergenerational mobility experience, and income. Tests for spuriousness indicate that, within this minority, powerlessness is associated with job complexity independent of education and income.

THIS paper examines the proposition that industrial man's definition of self as "powerful" or "powerless" depends to some extent on the complexity or simplicity of his work. The roots of this proposition reach back to the beginnings of sociology. Since Adam Smith's time, students of industrial society have been trying to assess the human consequences of the acute specialization and routinization of work roles found in industrial societies. And at least since Marx's time, sociologists have sought to link industrial labor with alienation experienced in the form of powerlessness. But empirical tests of such a link have not been convincing, and the frequently assumed relationship between work roles and powerlessness remains meagerly supported if not unconfirmed.

The imputed powerlessness of industrial man has been attributed to many sources, not all of which are rooted in the world of work. Yet because of the importance of work roles to individuals, and the dominance of economic institutions and ideologies in western industrial societies, the specification of occupational sources of powerlessness seems crucial to the understanding of powerlessness as a more general phenomenon.

Several observers have suggested that powerlessness results from what Wilensky and Lebeaux (1965:59-61) describe as the "dilution of skills" due to work simplification in industrial societies. Blauner (1964), for example, suggests that objective condi-

tions of powerlessness are more prevalent in textile and auto manufacturing than in printing and chemical industries, and that this difference is due in part to the fact that higher levels of task complexity in the latter industries produce work environments conducive to greater freedom and self-control. Kohn and Schooler (1969) define "occupational self-direction" largely in terms of task complexity. And they report that occupational self-direction is significantly related to a variety of attitudes, including self-confidence and self-respect (Kohn and Schooler, 1969:672). Other studies have reported significant relationships between workers' skill levels and generalized feelings of powerlessness (Neal, 1969; Simpson, 1970), between the North-Hatt Occupational Prestige Scale and feelings of powerlessness (Dean, 1961), and between skill levels of industrial workers and an index of mental health which includes items measuring self-esteem (Kornhauser, 1965).

Despite the general support these studies lend to the hypothesis that simple-minded jobs produce feelings of powerlessness, such support must be regarded as feeble in several respects. Most of these studies fail to distinguish between the relative effects of occupational complexity and other highly correlated variables such as education and income. This deficiency is aggravated by the general use of rather crude measures of "occupation," such as the U. S. Census ranking of broad occupational categories. In the two studies which employ fairly precise measurements of job complexity while controlling education and income, dependent variables incorporate indexes which are

*I would like to thank Richard A. Peterson and John Petras for their comments on earlier versions of this paper.

closely related to powerlessness; but they do not themselves measure powerlessness separately (Kohn and Schooler, 1969; Kornhauser, 1965).¹

To the extent that these studies demonstrate a relationship between job complexity and subjective powerlessness, the most consistent and noteworthy finding is that statistically significant relationships between these variables are remarkably weak. At best, they explain less than five percent of the variance in powerlessness. As Dean (1961) notes of his reported correlations between powerlessness and occupational prestige, these relationships are generally so weak that predictions based on them would be impractical. An even more negative statement is Seeman's (1967) report that no significant relationship exists between subjective powerlessness and the degree of "interest" or "challenge" perceived by workers in their jobs, and that work alienation is for practical purposes unrelated to feelings of general powerlessness.

The validity of the proposition that subjective feelings of powerlessness are related to the complexity of occupational tasks thus remains questionable. If the statistical significance of reported relationships supports such a proposition, the weakness of these relationships and the lack of controls for education and income suggest that the proposition has slight practical utility.

The aim of the following analysis will be to test the hypothesis that powerlessness varies inversely with job complexity, and to clarify the meaning of anticipated relationships. The reasoning behind this initial hypothesis does not differ from that of previous studies. I assume that occupation defines a "master status" and a central life interest for men in industrial societies, and that self-definitions are based in salient social roles such as those defined by occupation.² I assume that the extent of one's

mastery over his occupational environment varies directly with the complexity of his job. And I assume that industrial man's definition of self as powerful or powerless, stemming from the experience of mastery or powerlessness in the performance of work roles, is thus determined in part by the complexity of occupational tasks.

In finding significant but weak relationships, the following analysis replicates previous research in this area. I attempt to clarify the meaning of previous research in several ways. First, this study uses measures of occupational complexity which are more precise than the generally used Edwards socio-economic groupings; and these measures distinguish between several dimensions of complexity. Second, the study examines relationships between powerlessness and these more precise measures of complexity *within* conventional occupational categories, in which the salience of different dimensions of complexity might be expected to vary. Third, by controlling extraneous variables such as social mobility, education, and income, an attempt is made to determine whether reported relationships with job complexity are spurious. Finally, subpopulations are specified in which powerlessness is related to occupational complexity independent of the effects of education, income, and intergenerational occupational mobility.

Research Methodology

The following analysis is based on a nationally representative sample of native-born, non-agriculturally employed white males, interviewed by the Survey Research Center in its Election Studies of 1960 and 1964.³ My dependent variable is the SRC

however, that the evidence on this point is equivocal, particularly for the upper and lower extremes of the occupational hierarchy (see, e.g., Hollingshead and Redlich, 1958:77; Kahl, 1966:184-217; Wilensky, 1960:559).

³ Females were excluded from my sample because of the lesser salience of occupational roles to many women. Members of other exploited minority groups (Blacks and foreign-born) were excluded to control some confounding effects of minority group membership on powerlessness. Agricultural workers were excluded because I conceive this to be a study of industrial man. Slightly less than a fourth of the men meeting my sampling criteria

¹ Melvin Kohn reports by letter that the index of substantive job complexity used by Kohn and Schooler (1969) is significantly but weakly related to a Guttman Scale of powerlessness in their data ($r=.05$; $p<.011$).

² The assumption of the centrality of work roles to male self-concepts in industrial society is a common one, supported empirically by the findings of Kornhauser (1965:8), Morse and Weiss (1955), and Tausky (1969) among others. Note,

index of personal effectiveness, conceptualized as a measure tapping Seeman's (1959) concept of powerlessness (Campbell, 1962).⁴ My independent variable, occupational complexity, is defined by classifying SRC respondents' jobs according to descriptions provided by the *Dictionary of Occupational Titles* (1965).⁵

Occupational complexity is measured along three dimensions. One dimension reflects the variety of occupational tasks. Another reflects the complexity of interpersonal relationships, and a third reflects the complexity of data-manipulations required by job definitions. Variety has been described as a condition critical to the exercise of occupational self-direction, and a crucial factor determining the degree to which work roles encourage self-esteem through providing challenges (Kohn and Schooler, 1969; Kornhauser, 1965:129). The indexes measuring the other two dimensions have been described as hierarchies of prescribed versus discretionary duties involving *people* and *data* on the job, such that the greater the complexity of a job on

either of these dimensions, the greater is the discretion allowed the worker in performing his work (Fine, 1968:7-8). An index reflecting the additive effects of data-complexity, people-complexity, and variety was constructed from the sum of standardized scores on each of these dimensions.

Findings

Table 1 presents the distribution of subjective powerlessness across conventionally ranked occupational categories. The highest levels of powerlessness in this table appear among service and unskilled laborers. The lowest levels are found among professionals. As throughout this paper, Table 1 is arranged so that findings consistent with previous theory and research are indicated by an inverse (negative) relationship between occupation and powerlessness. Although the degree of association is small as measured by gamma, the relationship is statistically significant.⁶ This replicates previous research, both in the statistical significance of findings and in the uncertainty of their practical significance.

One way to explain the relationship in Table 1 would be to suggest that feelings of powerlessness are encouraged by overly simple jobs. But such an interpretation forces a fairly precise conceptualization of "job complexity" onto data which provide only a rough rank-ordering of occupational groups. Among "professionals" we include occasional wrestlers and strippers as well as physicists, for example; and among "service workers" we include police detectives along with bootblacks. Before placing confidence in interpretations phrased in terms of job complexity, we need a measure of complexity more precise than the occupational categories defined by the Census Bureau.

Table 2 presents relationships between powerlessness and various dimensions of job complexity as defined by the *Dictionary of Occupational Titles* (DOT). All three dimensions of complexity are significantly related to feelings of powerlessness within

in the SRC samples of 1960 and 1964 were lost from the sample through missing responses to SRC questions, or through job descriptions too ambiguous to classify according to the *Dictionary of Occupational Titles*. Comparisons with U.S. Census statistics indicate that distributions of several demographic characteristics (age, occupation, residence) in my sample are nevertheless comparable with distributions of these characteristics in the total U. S. population of nonagriculturally employed white males.

⁴ Inter-item correlations for this index range between .25 and .40 in the SRC samples from which my data are taken, averaging about .30 (Robinson *et al.*, 1968:652). Operational definitions for the index as it is used in this paper may be found in the Appendix.

⁵ Detailed job descriptions for respondents were obtained through examining SRC microfilms of original questionnaires. DOT ratings for these jobs are based on observations by trained occupational analysts. As descriptions of "typical" occupational requirements rather than requirements of particular jobs, the DOT ratings represent what are presumed to be objective and accurate appraisals of an "ecological variable." Their validity is suggested by Kohn and Schooler's (1969:672, n. 13) report that measurements of the complexity of *particular jobs* held by respondents to a nationally representative survey were "highly consistent with the *Dictionary's* ratings for entire occupations." Operational definitions of job complexity may be found in the Appendix to this paper.

⁶ Except where noted otherwise, significance tests here and below make use of the normal approximation to the sampling distribution of gamma in large samples, and test the one-tailed null hypothesis that $\gamma \geq 0$ in the population represented by my sample.

Table 1. Relationship between Respondents' Occupations and Feelings of Powerlessness

Powerless- ness	Occupation						N
	Prof.	Mang.	Sales/ Clerical	Skilled	Semi- Skilled	Service/ Unskilled	
High	18% (18)	27% (34)	33% (31)	25% (54)	31% (33)	46% (26)	(196)
Medium	32% (30)	34% (44)	34% (32)	29% (44)	40% (42)	28% (16)	(208)
Low	50% (47)	39% (50)	33% (31)	36% (54)	29% (30)	26% (15)	(227)
Total	100% (95)	100% (128)	100% (94)	100% (152)	100% (105)	100% (57)	(631)

$$\gamma = -.17; p < .0001$$

the sample as a whole, but the proportional reduction in predictive error made possible by a knowledge of any of these dimensions is slight.⁷ Again the data provide statistical evidence for the existence of relationships which we have good theoretical reason to expect in the population, but the weakness of these relationships leads to the conclusion that they may be unimportant in a practical sense. And a comparison of Table 2 with Table 1 suggests that, within the population as a whole, the precision of measurement and the specification of different dimensions of complexity provided by the *DOT* gain us little if anything over the crude categorizing of occupational skill levels. This is true whether we consider the effects of each index separately, or jointly.

Several alternate reasons for the small size of relationships described in Tables 1 and 2 may be considered. Powerlessness might be weakly related to complexity throughout the entire population, in which case the relationships in these tables would accurately represent relationships in various subpopulations. Or the relationships

might be spurious, imperfectly reflecting more meaningful relationships between powerlessness and education or income (both of which are known to be related to our independent and dependent variables). In either case, these findings would support Seeman's (1967) suggestion that the relationship between powerlessness and work is, for practical purposes, insignificant.

Alternately, non-spurious relationships with job complexity could exist in some categories of the population, but not in others. In this case, weak relationships in the overall sample would obscure the importance of relationships in some subpopulations while falsely suggesting their presence in others. The problem of analysis here would be to identify variables by which these different subpopulations might be distinguished from one another.

Table 2. Relationships between Powerlessness and Complexity*

Type of Complexity	Strength (γ)	Significance
Data	-.16	.0001
People	-.12	.004
Variety	-.11	.02
Additive Index	-.14	.0005

⁷ Costner (1965) interprets gamma as a "proportional reduction in error" (PRE) measure, since the value of gamma indicates the proportion by which error in estimating the relative ranking of any two men on the dependent variable ("powerlessness" in this case) can be reduced by a knowledge of their rank ordering on the independent variable (job complexity).

*N = 631

The crude occupational categories of Table 1 suggest one such variable. Since the salience of different types of complexity is likely to vary among different occupations, we might be able to identify subpopulations in which a given type of complexity is salient, and thus related to powerlessness, by examining relationships between powerlessness and each type of complexity *within* crude occupational categories. "People-complexity," for example, might be salient to managers and sales personnel, but not to unskilled laborers, for whom "variety" might be more salient. Both variables might then appear weakly related to powerlessness in the overall sample, but an accurate description of the source of these overall relationships would be possible only after controlling occupational categories.

Neal and Seeman (1964:217) suggest one other "structural complication" to "any work-based test of alienation," and this is intergenerational social mobility. In discussing hypothesized relationships between powerlessness and organizational membership, they identify upward mobility as an experience which may weaken relationships between work-related variables and powerlessness, due to the increased "sense of the manageability of events" which upwardly mobile men may enjoy. In recognition of the possibly complicating effects of occupationally defined mobility, therefore, the following analysis specifies relationships within categories defined by intergenerational mobility as well as by occupational category.⁸

Table 3 displays relationships between powerlessness and each separate dimension of complexity as well as the index measuring their combined effect. Moderately strong inverse relationships are clustered in the stable category of this table, and at lower levels of the downwardly mobile group. Relationships are consistently weak in the upwardly mobile category, and strongly positive among downwardly mobile white-collar workers.

Occupationally defined mobility obviously confounds the expected relationship

between powerlessness and occupational complexity. The general weakness of relationships in the upwardly mobile category of Table 3 is consistent with Neal and Seeman's (1964) speculations, but the reversal in the direction of expected relationships found at upper levels of the downwardly mobile group is more difficult to explain. This reversal exists among managerial as well as clerical and sales workers, and is not likely to be the result of random sampling error. When the two downwardly mobile white-collar categories of Table 3 are combined, the relationship between powerlessness and the index of job complexity is $\gamma = +.63$ ($\chi^2_{(1)} = 4.208$; $p < .05$). While no convincing interpretation of this finding suggests itself, it is obvious that the hypothesized negative relationship between powerlessness and complexity does not exist in this group.⁹

Specifying relationships within different mobility categories helps us considerably to understand why so many previously reported relationships between occupation and powerlessness have been so weak. Almost one-half of American workingmen are upwardly mobile, as indicated both by this sample and by Blau and Duncan (1967: 154); and we find no important relationships in this group. When combined with the reversal of expected relationships among downwardly mobile white-collar workers, these data identify a rather large source of variance in subjective powerlessness which remains unexplained by job complexity.

Turning to those categories of Table 3 in which reasonably strong relationships exist as hypothesized, we still face the possibility that education or income rather than occupation may best explain these relationships. In testing this possibility I have focused on the strongest relationships

⁸ Intergenerational mobility is defined in this study by comparing father's and son's occupational statuses as reflected in broad occupational categories.

⁹ Whatever the explanation for this reversal of expected relationships, it is worth noting that the one exception to Neal and Seeman's (1964:221) hypothesized relationship between powerlessness and organizational membership occurs among non-striving white-collar workers, where powerlessness was found to be significantly greater among *organized* than *unorganized* workers. The greatest reversal in the direction of relationships in their data occurs among the downwardly mobile workers of this group.

Table 3. Relationships between Powerlessness and Complexity with Occupation and Mobility Controlled*

Occupation	Type of Complexity	Mobility		
		Upward	Stable	Downward
Prof.	Data	-.05	-.11	
	People	+.04	+.21	
	Variety	(+)	(+)	Empty Cell
	Index (N)	+.14 (52)	+.30 (37)	
Managerial	Data	(-)	(-)	
	People	-.09	-.35	
	Variety	(-)	(-)	
	Index (N)	-.09 (78)	-.38 (32)	(12)
Sales and Clerical	Data	-.05		+.47
	People	-.19		(+)
	Variety	+.08		(+)
	Index (N)	-.01 (54)	(10)	+.49 (24)
Skilled	Data	-.01	-.06	
	People	+.07	-.34	
	Variety	+.16	-.39	
	Index (N)	+.05 (77)	-.17 (51)	(17)
Semi-Skilled	Data		-.24	-.25
	People		(-)	(-)
	Variety		-.29	-.33
	Index (N)	(12)	-.25 (56)	-.29 (36)
Service and Unskilled	Data			-.24
	People			-.21
	Variety	Empty Cell		-.32
	Index (N)		(13)	-.24 (41)

* Parentheses indicate that gamma is based on a frequency distribution with unbalanced marginals (75% or more cases filling one category). No relationships are indicated for cells containing fewer than 20 cases.

evident in various categories of Table 3, attempting to reduce these to a minimum by controlling education and income. Since all dimensions of complexity have been shown to be significantly related to powerlessness in Table 2 above, and since the significance appears to be based in the groups under consideration here, the purpose of *post hoc* analysis here is not to test statistical significance but to determine the strength of relationships independent of education and income.

The best predictor of powerlessness within upper occupational categories of Table 3 is people-complexity. The best predictor at lower levels is variety. Income differences affect these relationships, but

do not appear to explain them. At all occupational levels, relationships are strong within higher income groups and weaker in lower income groups. Table 4 shows values of gamma at different income levels for different groups defined according to occupation and mobility.

The strong relationships between powerlessness and job complexity in the upper income levels of Table 4 stand independent of the effects of education and further variation in income. Among stable managerial, clerical, sales, and skilled workers, for example, education is only weakly related to powerlessness in the upper income group ($\gamma = -.18$); and further variations in income within this group are positively related

Table 4. Relationships between Powerlessness and Complexity at Different Income Levels

Mobility	Occupation	Type of Complexity	Income Level	
			\$0-\$5999	\$6000+
Stable	Managerial Clerical Sales Skilled	People	-.22 (N=28)	-.47 (N=63)
Stable	Skilled Semi-skilled	Variety	+.10 (N=49)	-.34 (N=56)
Downward	Semi-skilled Service Unskilled	Variety	-.29 (N=40)	-.52 (N=36)

to powerlessness ($\gamma = +.36$). We thus cannot explain the strong inverse relationship between powerlessness and people-complexity ($\gamma = -.47$) as the spurious reflection of educational or income differences. Among stable skilled and semi-skilled workers, education is unrelated to powerlessness in the upper income group ($\gamma = -.02$), and further variations in income are also unrelated to powerlessness ($\gamma = -.04$). Among downwardly mobile semi-skilled, service, and unskilled workers, education is slightly related, and further variations in income positively related, to powerlessness in the upper income group ($\gamma = -.24$ and $\gamma = +.11$ respectively). As before, we must conclude that the relationship between powerlessness and variety ($\gamma = -.52$) is not explained by educational or income differences in this group.

Relationships between job complexity and powerlessness in lower income categories of Table 4 are not only weaker than relationships in upper income categories, they tend not to be independent of education and income. While this might reflect multicollinearity of independent variables rather than spuriousness, we cannot reject the possibility that education or income rather than job complexity may "really" explain the extent to which lower income men feel powerless. Such an explanation would be compatible with Wilensky's suggestion that "a large slice of the labor force behaves the way it does mainly because of low and unstable income, low status, and

their psychological correlates" (1960:551-552).

Discussion

This analysis indicates that occupational complexity in one form or another is meaningfully related to feelings of powerlessness among significant numbers of non-mobile and slightly mobile men with moderate to high incomes. These men comprise a definite minority of the labor force, and it is reasonable to question the practical significance of job complexity when sources of powerlessness are examined in the population as a whole. But within the minority of men specified by this analysis, complexity of work roles is associated with generalized feelings of personal effectiveness, its absence is associated with feelings of powerlessness, and the association remains strong independent of the effects of education and income.

In specifying subpopulations in which relationships between powerlessness and job complexity attain practical significance in a proportional reduction in error sense, this study clarifies the source of the significant but weak overall relationships which have been persistently reported by previous researchers. This clarification is important first and most simply because it describes social categories in which individual attitudes of powerlessness or personal effectiveness may be traced back to the occupational structure shaped by industrial bureaucracy and technology. Within specified

categories, task complexity or simplicity does indeed "explain" a good deal of the powerlessness experienced by men occupying different positions in the occupational structure, as indicated by a proportional reduction in predictive error averaging about 45%. Thus this study may be interpreted as encouraging the general search for structural explanations of at least some psychological phenomena.

In specifying other social categories in which the effect of job complexity on subjective powerlessness is nonexistent or reversed from expectations, and in finding a majority of the work force to be in such categories, this study also questions some of the simpler explanations and speculations concerning the human impact of job "trivialization" in industrial society. Job complexity as I have measured it is unrelated to whatever general feelings of powerlessness the majority of American men may experience.¹⁰ This finding cannot be explained as a product of the measuring instruments used here, since the same instruments reflect strong relationships among a minority subpopulation of men. Thus, while the strength of relationships in some categories must encourage the search for structural explanations of powerlessness, the absence or reversal of relationships in other categories suggests that this search must be for more complicated explanations than those typically provided by theories of alienation.

These findings are important beyond simply establishing the fact that work roles and self-concepts are meaningfully related within some categories of men, and not within others. For example, if one were attempting to predict the consequences of future changes in national income and occupational structures, conceptualizing today's working population as heterogeneous in its response to job complexity would lead

to different predictions than would the assumption that it is homogeneous in this respect. The latter assumption leads to expectations that higher levels of per capita income, or more equitable distributions of national wealth, would not change the strength of overall relationships between powerlessness and job complexity. But this analysis suggests that if increasing proportions of the labor force entered higher income categories, the proportion of men whose powerlessness is based in their work might also increase. We would expect this to be accompanied by an increase in the strength of statistical relationships in the population overall.

If Blauner's (1964) description of broad evolutionary trends is valid we can expect to see some increase in the complexity of jobs in the future, along with a projected rise in per capita income. Whether general levels of powerlessness in the population will decline with these developments is difficult to predict, given other bases of powerlessness than job complexity, and given the existence of opposing trends such as the increasing importance of leisure and the possibility of a complementary decline in the general salience of work. In any case, the result of such changes is likely to differ from what would be expected if the weakness of overall relationships were uniformly distributed throughout the labor force.

APPENDIX

OPERATIONALIZATION OF VARIABLES

- I. "Powerlessness" is measured by an index consisting of the sum of scores in response to the following three items. Negative responses receive a score of "0," neutral responses a score of "1," and positive responses a score of "2," so that high scores indicate low feelings of powerlessness.

- A. "Have you usually felt pretty sure your life would work out the way you want it to, or have there been times when you haven't been very sure about it"?
- B. "Do you feel that you are the kind of person who gets his share of bad luck, or do you feel that you have mostly good luck"?

¹⁰ This is not to say that complexity is unrelated to objective conditions of powerlessness, or that these objective conditions do not affect men's psychological states or the social structures in which they participate. It is simply to say that, whatever the effects of trivial tasks, most men do not translate these effects into subjective feelings of powerlessness such as those tapped by the questionnaires used here.

- C. "When you make plans ahead do you usually get to carry out things the way you expected, or do things usually come up to make you change your plans"?

II. Complexity of occupational tasks along three separate dimensions is indicated by *lower* scores on the following indexes.

A. Variety of occupational tasks

<i>Index Score</i>	<i>Type of Activity</i>
1	Situations involving a variety of duties often characterized by frequent change.
2	Situations involving neither variety as defined above nor repetition as defined below.
3	Situations involving repetitive or short cycle operations carried out according to set procedures or sequences.

B. Complexity of Data manipulation

<i>Index Score</i>	<i>Type of Activity</i>
0	synthesizing
1	coordinating
2	analyzing
3	compiling
4	computing
5	copying
6	comparing
7 & 8	no significant relationship with data

C. Complexity of interaction with People

<i>Index Score</i>	<i>Type of Activity</i>
0	mentoring
1	negotiating
2	instructing
3	supervising
4	diverting
5	persuading
6	speaking-signalling
7	serving
8	no significant relationship with people

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ROLE-TAKING AND POWER IN SOCIAL PSYCHOLOGY*

DARWIN L. THOMAS

Family Research Center, Brigham Young University

DAVID D. FRANKS

University of Denver

JAMES M. CALONICO

Louisiana State University, New Orleans

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It is theorized that when structural control resources are not available, persons will tend to use role-taking as a "management" strategy. Thus role-taking ability is at a premium when the subordinate attempts to balance the structurally based power of a superordinate by increasing the latter's ego gratification in the relationship. Role-taking and power are seen as distinct strategies by which persons achieve control of others. Drawing from a sample of 888 subjects from 222 family units, questionnaire data are analyzed to test the hypothesis that role-taking accuracy will be inversely related to power inside the family. Using position in the nuclear family as an index of power, it was found that fathers were less accurate role-takers than mothers and mothers less accurate than children. Data were also analyzed using children's perceptions of their parents' power and the wife's perceptions of conjugal decision-making as measures of power. Partial support for the hypothesis was found in that dominant wives were poorer role-takers than wives with less decision-making power (autonomic). Research is suggested to test the proposition that role-taking ability may be a function of the person's position on a situationally given role-set rather than a static personality trait.

THEORETICAL RATIONALE

Role-Taking. From a sociological orientation in social psychology, few concepts rival role-taking in theoretical importance. It is a truly interactive concept (Turner, 1962), and at the same time it plays a critical role in a sociological view of the development of self as well as in a theory of social control (Shibutani, 1961: 194-201). The "biological" individual becomes a member of a social community when he becomes self-conscious about his own actions from society's standpoint. This makes him distinctively human and roots his social nature in the interactive process. "The biologic individual must be able to call out in himself the response his gesture calls out in the other, and then utilize this imagined response of the other for the

control of his own further conduct. The ability to call out the same response in both self and other gives the content necessary for community of meaning." (Mead, 1934, p. xxi)

Self is not a biological given but emerges in social interaction. The child develops a self (becomes self-conscious) only to the extent that he can see himself as others do (role-taking) and use this to monitor his behavior with others. One becomes an object to himself through being able to take others' perspectives. This depends on significant symbols or shared meanings (language) which actors use in a given reference group. Paradoxically, only through an inherently social process of role-taking can one develop a viable sense of individuality.

In view of the central nature of the theoretical construct of role-taking, (see Cottrell, 1950; Stryker, 1971) one would expect sociologists interested in individual conduct to continue to contribute at both the theoretical and hypothesis testing levels. Such is not the case. The theoretical potential of the concept has not been matched by attention at the operational and research level (Stone and Farberman, 1970; Kuhn,

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1964). Stryker (1971) laments the absence of sociologically oriented theory and research in his recent review of the latest *Handbook of Social Psychology*. Both theoretically and empirically, the relationship between role-taking and power is a potentially fruitful area of investigation in social psychology.

Role-Taking and Power. Often in theoretical discussions of interpersonal behavior along the dimension of power, an important distinction in levels of analysis is overlooked. Sociologists are appropriately interested in clarifying the interactional nature of power relations (see Wrong, 1968:673; Emerson, 1962:31-32). They emphasize that subordinates can have "power" since they can and do constrain superiors. Thus Emerson describes one of four types of "balancing operations"¹ which:

... increases the weaker member's power to control the formerly more powerful member through increasing the latter's motivational investment in the relation. This is normally accomplished through giving him status recognition in one . . . of its many forms, from ego-gratifications to monetary differential. The ego rewards . . . are highly valued . . . while given at a low cost to the giver. (p. 39)

This theoretical formulation is important, but the broad use of "power" tends to obscure the distinction between resources stemming from one's social *position* and those stemming from an individual's self-system. Resources accruing from the self-system are potentially available to all persons depending on their individual competencies in interpersonal relations.²

Granted that both subordinates and superordinates have at their disposal potentials for controlling each other, *power* will

refer to potentials given by one's structural position vis à vis the other; and the term *influence* will be used to refer to potential for control based on one's interpersonal skill, independent of his social position. Power and influence are fundamentally distinct modes of gaining control, the former utilizing a relatively enduring³ structure and the latter the reservoir of interpersonal abilities.

For the social psychologist how are power and role-taking related? Following Goffman (1959), role-taking not only socializes the individual, but it is used to control others' responses by pleasing them on their own terms. A premium is placed on *accuracy* in role-taking when the subordinate utilizes controls based on interpersonal abilities. Such control is more particularized than the use of power. The subordinate must role-take accurately if he is not to destroy his potential control say, by misjudging the superordinate's response and becoming identified as an "ingratiator."

The general proposition is that persons in higher power positions do not need to and therefore do not use role-taking to the same degree as persons of lower power positions. This is not to say that controlling others through power does not involve role-taking. However, once a person in power has correctly identified a resource desired by the other, it is more important for the subordinate to act according to the desires and orientations of the power holder than vice versa. Rose (1969) has enunciated a similar idea using the term *empathy* instead of role-taking:

by raising his status, or invoke norms that restrict the other person's alternatives." They go on to discuss interpersonal attraction as a source of "power," while we would prefer the term *influence*.

¹ See Secord and Backman (1964) for a summary of the main concepts concerning power relations used here. "... an unbalanced (power) relation is unstable since it encourages the use of power, which in turn sets into motion processes that Emerson, (1962) has called cost reduction and balancing operations." (Parentheses are ours.) The balancing operation of interest here is Emerson's *third* one.

² Secord and Backman (1964:293) emphasize this feature. "Various interpersonal strategies are employed to shift the balance of power either in fact or perceptually. People may disguise their dependency, pretend to have many desirable alternatives, increase the investment of the other

³ The emphasis on enduring sources of control is often isolated as an important feature of potential power in a structural framework. Since behavior on the interactional level is situated and episodic, power, to be enduring, must be seen as having the quality of potential. Structural power in our terminology has this enduring quality because it is transsituational. For a discussion of this see Dennis Wrong, 1968. Our use of the term power is similar to Emerson (1962) and Wolfe's (1959) "authority" concept, which has connotations similar to "legitimate power" as used by Smith (1970) and others.

It may be that persons who are members of minority groups are forced, because of their underprivileged positions, to develop an empathetic ability as a condition of their adjustment to the subtle expression of dominance by members of superordinate groups. Thus, Negroes in minority situations have been characterized as having a more accurate perception of white mentality than whites have of Negro mentality. . . . One of the privileges of power, perhaps an often overlooked one, is the privilege of insensitivity to the negative attitudes of others. (p. 476)

Rose also generalizes the basic idea by proposing that women should be more accurate role-takers than men. Subordinate status in a situated role-set should be characterized by role-taking accuracy among members of different social categories.

Hypotheses. One could deduce from this that children would tend to be more accurate in intrafamilial role-taking than their parents. Furthermore, in families in male dominant societies, the females should be more accurate in their role-taking ability than the males. By combining the two propositions, one can deduce the following hypothesis: *Accuracy in role-taking will exhibit the following pattern among family members: father < mother < male child < female child.*

Note that the hypothesis that children will be better role-takers than their parents is not a common-sense notion. One could maintain that their greater fund of knowledge and experience about theirs and their children's lives would make parents better role-takers. Indeed, parents are often forced to anticipate their child's behavior to protect against possible danger or produce a desired outcome.

A second hypothesis can be generated from this basic link between role-taking and power which will allow for an investigation of the effect of personal power style on role-taking. Individuals in a particular social position will vary on the degree of power they exercise and hence on the degree to which others in that social group perceive them as powerful. If personal power style is related to role-taking as structural power is in the above theoretical rationale, then *children who perceive their fathers and mothers as highly powerful individuals should be better role-takers than children who perceive their fathers and*

mothers as low power individuals. By the same reasoning husbands and wives who exercise a high degree of power in the conjugal relationship should be less accurate role-takers than those with low conjugal power.

METHODOLOGY

Sample. In order to test the hypothesized relationships, data were gathered from both parents and children in intact nuclear families. A "face-sheet" requesting necessary information about the respondent and his family was administered to around a thousand undergraduate students at a large western state university. From these responses a sample of the student population was drawn which represented the student body on the following characteristics: sex, year in school, major field of study and grade point average. Further requirements for inclusion in the study were intact families composed of mother, father, high school aged child and college aged child. A random sample of three hundred qualifying families was drawn from the families meeting the necessary criteria.

During the summer a packet of materials was sent to each family chosen. Each packet contained four questionnaires, one for each family member, along with a cover letter and instructions to assist respondents in completing and returning questionnaires. Separate return envelopes were included for each respondent to allow as much privacy as possible. Four mailed follow-ups were sent to families at approximately ten-day intervals, the first being sent ten days after the family received the original packet. At the beginning of data analysis, questionnaires had been returned by 82.8% (994) of the sample's 1,200 family members (four members in each of three hundred families). Comparisons were made with the non-respondents using information gathered on the initial "face sheet," and those families not returning their questionnaires were found not to differ significantly on SES level, family size, religious affiliation, and parents' education level.

Since the current analysis required the use of both parents and children's responses, we decided to use only the scores of "com-

pleted" families, i.e., scores from all four family members. For the present analysis, 222 completed families were available necessitating the use of only the role-taking scale. This represents 74% of the three hundred families in the original sample. Where analysis required using both the role-taking and power (as measured by the semantic differential) scales, 70.7% (212) of the completed families were available. This difference occurs because some persons completed only one of the two scales, invalidating the use of that family for the particular analysis.

Finally, a measure of conjugal power was obtained in the second phase of data collection, a telephone follow-up six months after the beginning of the first phase. A number of families could not be contacted during this follow-up; thus, for the analysis requiring both role-taking and conjugal power scores, 56.3% (169) of the completed families could be included.

Role-Taking Measure. This scale was developed specifically for the current work. Judgment tests usually employ either a "trait-rating" procedure (Bronfenbrenner, Harding and Gallwey, 1958; Fiedler, 1954; Fiedler, Blaisdell and Warrington, 1952) or a direct prediction of another's response to attitude items (Byrne and Blaylock, 1963). The present effort differs from these and most other types of "judging instruments" (see Cline, 1964:224) in its attempt to approximate role-taking more closely. It is a modification of the dilemma resolution technique previously used in parent-child research (see Thomas and Weigert, 1971). The role-taking measure is most easily understood by considering the following item from the scale.

A young man about to graduate from college is opposed to military service but is sure he will be drafted shortly after he graduates. He is trying to decide whether to allow himself to be drafted and serve in the armed forces or to unlawfully resist the draft, thereby risking the chance of arrest and imprisonment.

a. I would advise this young man to allow himself to be drafted and serve in the armed forces.

1. no
2. probably no
3. probably yes
4. yes

b. In my opinion, my father (older child) would advise this young man to allow himself to be drafted and serve in the armed forces.

4. yes
3. probably yes
2. probably no
1. no

c. In my opinion my mother (younger child) would advise this young man to allow himself to be drafted and serve in the armed forces.

1. no
2. probably no
3. probably yes
4. yes

Each of the ten items presents a hypothetical situation in which an abstract other, i.e., neither actor nor the specific person whose role he is taking, is placed in a behavioral dilemma. The abstract other is shown as perceiving two possible responses to his dilemma. The actor must indicate how he would advise the abstract other to respond. He must then indicate how each of two significant others, persons whose roles he is taking, would advise the abstract other to respond. Parents were asked to take the role of each of the two participating children and each child was asked to take the role of each parent. The names of specific family members were written into the questions so that the person would know whose role he was taking.

A role-taking accuracy or difference score is computed for each respondent by comparing the other's actual response with the actor's prediction of that response. Thus the range of absolute difference between any two subjects on any item is from zero to three. *Higher scores* denote greater role-taking *inaccuracy*.

These absolute difference scores were factor analyzed. Table 1 presents the factor loadings obtained by means of principal axis factor analysis.⁴ From these results,

⁴ Since our primary concern was to identify a major underlying role-taking factor from the difference scores on each of the ten items, we used the principal axis factor analysis. Three factors were produced in this analysis with the values in Table one coming from the first. The second and third accounted for less total variance in the role-taking scale than did factor one, and some items (marriage, parole, search warrant) loaded on more than one factor. In an attempt to analyze for other possible factor structures, the verimax

Table 1. Principal Axis Factor Analysis of Role-Taking Scale

Item	Factor Loading
1. Military Draft ¹	.59*
2. Homosexual as Security Risk	.55*
3. Communist Teacher	.52*
4. Search Warrant	.34*
5. Marriage at College-Age	.32*
6. Spanking of Children	.28
7. Employee Theft	.18
8. Atheism of College Student	.11
9. Prisoner as Parole Risk	.08
10. Police Shooting of Burglar	.06

*Those items used in the role-taking score (see Footnote 5).

¹For exact wording of the items used in this scale, write the authors.

role-taking scores⁵ were created by including those items with a factor loading $\geq .30$.

Power Measures. In the test of the first hypothesis, the individual's position in the family was taken as an index of his power in the family. The position rank on the power dimension is father > mother > male child > female child.

In the test of the second hypothesis, two power measures were used. In the first, each

factor rotation solution was used to identify orthogonal factors. The first orthogonal factor consisted of three items: draft, homosexual, communist. These three items had the strongest loadings on the first factor from the principal axis factor analysis. The second and third orthogonal factors were a mixture of the remaining seven items. Thus the results of the orthogonal factor analysis seemed to support the decision to use the principal axis factor analysis in constructing the role-taking scores.

⁵Scale scores are created by first standardizing each variable (item) to have a mean of zero. Then, for any subject, the value of each variable in his original score which is to be included in his scale score is multiplied by its corresponding factor loading. The products are then summed to constitute his scale score. This weights each item in the score by the size of its factor loading. In addition, the mean of the factor analysis scale scores is set to zero; thus, for the present work, the more positive an individual's scale score the more inaccurate is his role-taking; and the more negative his scale score the more accurate is his role-taking.

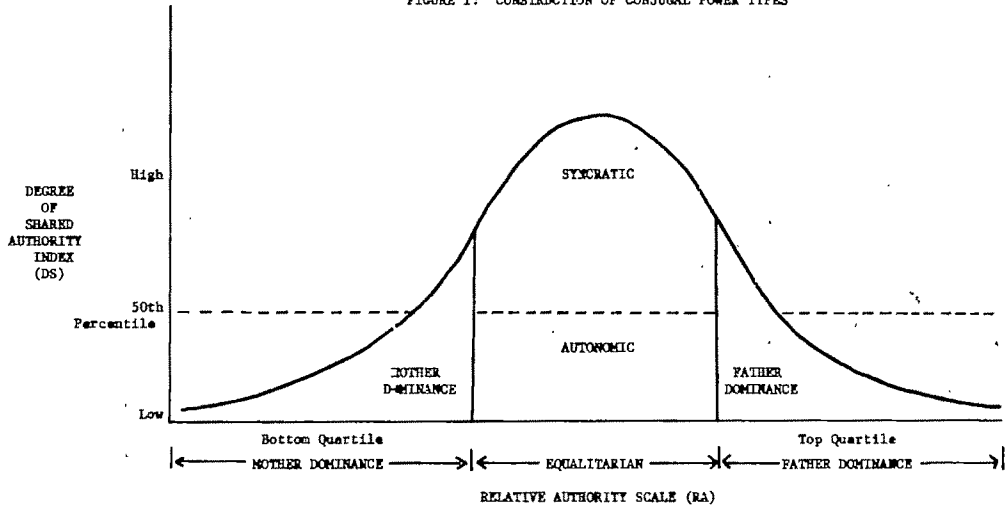
family member rated himself and each other member on ten adjective pairs. Through factor analysis of the semantic differential, two adjective pairs, stern-mild and powerful-powerless, emerged for constructing a power measure.⁶ Fathers and mothers were divided at the median into high and low power groups according to the children's perception. For the present work, role-taking scores of children who perceived their mothers and fathers as high on power were compared with children who perceived their parents as low on power.

A second power measure (conjugal power) was constructed by using the responses to four questions asked of the wife in four areas of decision making: when you and your husband disagree over (money, religion, furnishing the house, and disciplining the children) who has the final say? 1. mother always, 2. mother more than father, 3. mother and father about equal, 4. father more than mother, 5. father always. A second scoring scheme (DS scale or degree of Shared Authority Index) was developed where the 1 and 5 response categories received a one, the 2 and 4 response categories received a two; and the 3 response category received a three; these two indices of power were combined (RS-Relative Power Index) to produce the four conjugal power types developed by Wolfe (1959): namely, mother dominant, father dominant, syncratic and autonomic.⁷ These four types are pictured graphically in Figure 1. The father is hypothesized as being most powerful in the father dominant type and least powerful in the mother dominant.

⁶Factor analysis of the ten adjective pairs produced a potency factor (using three factor varimax solution) consisting of these two items having the highest loadings, .81 for stern-mild and .51 for powerful-powerless.

⁷Since four items were used in the present study, different cut off points from either the Detroit Study (Blood and Wolfe, 1960) or the Los Angeles Study (Centers *et al.*, 1971) were used. In this research (see Figure 1) Mother Dominance was defined as the bottom 25% on the relative authority scale (RA) and Father Dominance was the top 25% on the RA Scale. Syncratic was defined as anyone scoring in the middle 50% on the RA Scale and the top 50% on the DS Scale. Autonomic was anyone falling in the middle 50% on the RA Scale and the bottom 50% on the DS Scale.

FIGURE 1: CONSTRUCTION OF CONJUGAL POWER TYPES



The mother is seen as most powerful in the mother dominant and least powerful in the father dominant families. The autonomic and syncratic types are conceptualized as middle power types for both the father and mother, since they both tend to make decisions in different areas (autonomic) or share in joint decision making (syncratic). As indicated previously, 169 families were available for this phase of the analysis.

FINDINGS

The first hypothesis (on the basis of power differences) predicts the following order on role-taking across family positions: father < mother < male child < female child. Table 2 presents the mean role-taking scores and significance levels for t-tests between adjacent positions on the power continuum. The pattern of the data supports the hypothesized relationship. Fathers are significantly less accurate role-takers than mothers ($p < .001$) and mothers significantly less accurate than male children ($p < .001$; section A, Table 2). The difference between the mean role-taking scores of male and female children is in the predicted direction, but it is not statistically significant.

The literature on the sociology of the family contains the idea that order of birth is important in ascribing positions of power to children (see Thomas and Calónico, 1972; Reiss, 1967:154). Older children in families are seen as occupying positions of

both power and responsibility compared to younger children. If this reasoning is correct, younger children should be better role-takers than older children. To further test the proposition relating role-taking to power, the effect of birth order on sibling role taking was investigated. Table 2, section B, presents these results. The pattern between the means indicates that younger children are better role-takers than older children, but again the difference is not statistically significant. At this juncture we reasoned that if both order of birth (older and younger) and sex were combined, the oldest male child might be significantly less accurate in role-taking than the youngest female child. Section C in Table 2 presents the mean scores. It can be seen that the absolute difference in means is increased when the effects of order of birth and sex are combined, but none of the differences between adjacent means is statistically significant. When a separate t-test was calculated between the mean score of older male children with that of younger female children, the difference was not statistically significant.

The second hypothesis predicts that children in families with high power fathers and mothers will be significantly better role-takers than children in families of low power fathers and mothers. Table 3 presents the mean role-taking scores for high and low power fathers and mothers as perceived by their children. The relationships among the

Table 2. Mean Role-Taking Scores and Significance Levels for T-Test by Position in the Family.

Position in the Family						
Mean Role-Taking Scores ¹	Fathers		Mothers		A Male Children	Female Children
		***		***		
N	222		222		221	223
Mean Role-Taking Scores	Fathers		Mothers		B Older ² Children	Younger ³ Children
		***		***		
N	222		222		222	222
Mean Role-Taking Scores	Fathers	Mothers	Older Male Children	C Older Female Children	Younger Male Children	Younger Female Children

N	222	222	97	125	124	98

*** = $p < .001$ for one tailed t-test computed between adjacent means.

¹When factor analysis scale scores are created, the distribution of the scores is reconstituted to set the mean of the distribution at zero. For the analysis here, the more negative a score is, the more accurate the role-taking and the more positive a score is, the less accurate the role-taking.

²These children are college age: mostly Freshmen and Sophomore with a mean age of 19.5.

³These children are high school age: with a mean age of 15.9.

mean role-taking scores for the female children are as predicted. None of the differences in means is statistically significant, but the difference in mean role-taking scores for

female children in low and high power mother homes approaches the .05 level of significance. The difference in means for male children is small, and their role-taking scores of father and mother are in the opposite predicted direction.

Table 3. Children's Mean Role-Taking Scores by Parental Power and Sex

	Level of Perceived Parental Power			
	Father's Power		Mother's Power	
	Low	High	Low	High
Female Children	-.278	-.344	-.008	-.246
N =	113	95	117	91
Male Children	-.163	-.115	-.314	-.238
N =	109	107	111	105

The difference in means of female children in high and low power mother groups is the only comparison which approaches significance at the .05 level ($t = 1.52$).

The test of the second hypothesis using conjugal power scores as a measure of the husband's and wife's relative power in the conjugal relationship is presented in Table 4. The hypothesized relationship that fathers (husbands) with low power (wife dominant conjugal relations) will be more accurate than fathers with high power (father dominant conjugal relations) is not supported. Fathers with low relative power in the conjugal relationship are not the most accurate role-takers. Neither are fathers with high power in the conjugal relationship the least accurate. None of the differences between the means is statistically significant.

It will be noted from Table 4 that the hypothesized relationship that mothers

Table 4. Parents' Mean Role-Taking Scores by Conjugal Power Type

	Husband Dominant	Conjugal Power		Wife Dominant
		Syncretatic	Autonomic	
N	(40)	(76)	(18)	(35)
Fathers' Mean Score ¹	.660	.469	1.132	.704
Mothers' Mean Score	.295	-.013	-.507	.414

¹None of the differences between any of the adjacent means are statistically significant.

** = $p < .025$ for one tailed t-test computed between adjacent means.

(wives) high on power would be the least accurate role-takers is supported. However, the most accurate role-takers are mothers (wives) who come from autonomic conjugal power structures. The difference in means between wife dominant and autonomic groups is statistically significant at the .025 level.

DISCUSSION AND CONCLUSIONS

The major findings of this research, that fathers are significantly less accurate role-takers than mothers and mothers significantly less accurate role-takers than their children, support the theory guiding this research.⁸ The data trends for children, that positions having the least power in the family (younger female children) are more accurate role-takers than positions having more power (older male children) also support the theoretical proposition that role-taking ability varies inversely with the degree of power ascribed to social positions. This basic theoretical proposition should now be tested on other social groups to determine the generalizability of the findings.

⁸ However, one should not claim too much for power as indexed by position in the family in this research. Given the relatively large N's used here, one should not overestimate the importance of the statistical significance. Computing Omega Squared [W^2] (see Hayes, 1965:327) as a measure of association based on the t value between mean role-taking scores of fathers and younger female children, yields a value of .035. Thus knowing that a person is a father or a female child will allow one to account for about nine percent of the variance in role-taking scores. Clearly much work remains for the researcher interested in accounting for variance in role-taking ability.

The family as a social group differs on important dimensions from other social groups, and it is possible that the findings reported here apply only to variations in role taking ability among different family members. The family is a social group having a history important in the functioning of its members. It is also characterized by strong emotional bonds between and within generations. Therefore, attempts to generalize these findings to other social groups are risky. Despite these differences between the family and other social groups we believe the findings are not group specific.

This judgment derives from previous analyses of the data along with possible interpretations of it presented in this report. The emotional dimension characteristic of the family was not found to be an important predictor of role-taking ability (see Calonic and Thomas, 1972). Variation in the amount of emotional support children received from parents was only related to role taking when parents and children did not share similar value orientations. Such children from families with high parent-child affect were not accurate role-takers. The explanation offered for this finding was that with high emotional support from parents, children could not accurately take the role of parents because they assumed parents were like themselves, when in fact parents and children did not share similar values. That finding combined with the data in this research suggests that persons in low power positions who share similar values with persons of high power, and who have a high positive emotional bond with people in power will be accurate role-takers. However, if persons in low power positions

do not share similar value positions with persons in high power positions, but have high positive emotional bonds with them, they will be inaccurate role-takers. This relationship is posited to hold for any group which has interacted long enough for bonds of emotional support to have developed.

The major findings in this research cannot be explained by differences between the generations. True, the data show that children are better role-takers than parents but mothers are significantly better than fathers a fact which argues against differences in generations being the cause of the findings. Also, mothers in wife dominant families are less accurate in role-taking abilities than mothers in autonomic families. Differences between generations would not help explain this finding; thus, the power explanation becomes more credible.

The finding for wives that the autonomic conjugal power structure rather than the husband dominant produces the most accurate role-taker requires some comment. The theoretical underpinnings may require modification. Further research is needed to untangle methodological problems associated with different power measures from theoretical inadequacies. The conjugal power measure asks about decision making in the husband-wife dyad, while the role-taking measure is between the parent's and children's generations. It is not clear why this power measure is related to the wife's but not the husband's ability to accurately take the role of their children. Research concentrating on exercise of power and role-taking across and within family generations could contribute important knowledge about the functioning of families as well as other groups.

Our theoretical orientation leads us to assume that rather than individual characteristics the superordinate-subordinate characteristic of social structure should predict differences in role-taking ability. This interpretation receives some support in the data. The nuclear family positions arrayed on a power continuum produce significant differences in role-taking, while the group members' perceptions along the powerful-powerless dimension are not consistently related to role-taking. The trend in the data

seems to suggest that perception of others' power characteristics may be important for women's role-taking ability but not for men's (note that it is the wife's perception in the conjugal power dimension and the female child's perception of parents' power that seem related to role-taking.) This fact suggests that in American society women may become more adept at perceiving their own as well as others' power characteristics in order to role-take more accurately. Thus the autonomic wife must role-take accurately to make and carry out decisions in her areas of power. Her position as wife does not automatically carry this power with it. The power must be won. Accurate role-taking is one way of winning it. Further research could clarify these and other relationships.

An important aspect of this research is the type of investigations that it points toward. It calls for research to isolate the personal dimension from the structural. If our theorizing is accurate, any individual should be a more accurate role-taker of people in superordinate rather than subordinate positions in the group. Academic institutions, hospitals or any other hierarchically organized structure would provide ample tests for a number of propositions. Would department chairmen be better role-takers of deans than vice versa? Would deans better take the role of vice presidents than of their department chairmen? If an individual tends to develop these different abilities to take the other's role on the basis of power differences emerging in specific role sets, then one should probably not emphasize personality traits or characteristics but rather systemic qualities of the social structure. Such research could begin to identify the contributions of the individual and social structure in the basic social psychological role-taking process.

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MIGRATION AND THE ECOLOGICAL COMPLEX¹

DAVID F. SLY

Florida State University

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Following the suggestions of Hawley, Duncan, and Schnore, this paper investigates the possibility of viewing the variables subsumed under the population rubric of the ecological complex as dependent variables. The demographic processes of fertility, mortality, and migration are defined as a set of alternative means available to populations which seek an equilibrium between their size and sustenance organization. Focusing on migration, a general model is developed which suggests that neither environmental nor technological factors affect migration directly; but rather, that their effect on migration is produced through changes in organization which they generate. A modest empirical test of the model is carried out through an examination of Southern black migration rates, and general support for the model is obtained.

INTRODUCTION

IN his volume on community structure and ecological theory Hawley (1950) focuses on organization and develops the strategy that changes in the organization of populations are to be sought in the population's technology and environment. While this work has been a bench mark for most ecological theory and research, it has been criticized by ecologist and nonecologist alike. The latter's criticisms have been aimed at ecology in general and have added little to its theoretical development (Wilhelm, 1964).

The most fruitful refinements have come from the former. These began with Duncan's (1959) specification of the "ecological complex." One of his major points, and the one we will focus on, concerned Hawley's argument that ecology was interested in "... the development and organization of the community" (1950:73). Duncan, in specifying the four major axes of the ecological complex (population, organization, environment, and technology), noted that Hawley tended to view organization as the sole dependent variable of interest to the ecologist. Duncan

argued that organization was only one of the problems that could be approached within the broader context of ecology. He takes the position that variables subsumed under any of the rubrics can be treated as dependent. Following this lead, Schnore (1958; also see Duncan and Schnore, 1959) argued that many problems are best approached by treating organization as an independent variable.

This paper builds on the ideas of Hawley, Duncan, and Schnore. It develops a model for migration analysis which suggests that migration is a population demographic response to differences in sustenance organization and is unaffected directly by either technology or environment. A modest test of the model based on data on Southern Negro migration for the decades 1940-1950 and 1950-1960 and using correlation and path techniques is offered.

THE ECOLOGICAL VIEW OF MIGRATION

Migration may be defined as either an individual or a population phenomenon. Viewed as an individual phenomenon, the focus tends to be on individual migrants rather than on the social process of migration. (For example see Zipf, 1946 and Lee 1966) This leads to arguments that migrants are mobile because they possess certain characteristics or that migration can be explained by reference to the motives or values of the migrants. In the former case we end up with "descriptive propositions" of the sort sug-

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gested by Lee (1966) or Miller (1966). For instance, it may be argued that people in the labor force are more likely to migrate than people out of it.

Explaining migration in terms of motives or values puts one in a similar situation. In this case one locates the causes of migration in the individual and only secondarily in the social structure. Typical of such "propositions" is Dorothy Newman's (1965a; 1965b), that migrants move because they have higher aspirations for upward mobility. While it is legitimate to approach migration analysis from this "individual perspective," one must bear in mind that values and motives are themselves part of the behavior, and as such, should be explained rather than be used as the explanation (Davis, 1963: 354). "Characteristic types" of explanations likewise are seldom tied to more general frames of reference and tend to be ad hoc, descriptive or ex post facto.

Ecologically, migration may be viewed as a component of areal population change (Hawley, 1950:328-332); it is a response through which a population can maintain an equilibrium between its size and sustenance organization. When migration is viewed from this perspective, the important questions become whether it increases or decreases populations, what the size of the population change is in relation to changes in sustenance activities and what the other factors affecting sustenance organization are.

Thus, migration is the other than natural increase or decrease in areal population size. This definition bases population geographically (or areally). If it did not, population size could change through such processes as annexation or consolidation. Furthermore, viewing migration as a population response forces us to seek the causes of movement in the physical and social environment. This means for our purposes individuals, characteristics, values, and motives need not enter the analysis. It does, however, restrict us to specifying the *conditions* under which migration takes place.

THE ECOLOGICAL COMPLEX AS A FRAME OF REFERENCE

Population: Human ecology assumes that populations are reference points for all analyses. It directs our attention to the aggre-

gate which may be described in terms of location, size, composition and territorial distribution. Ecological units may be identified by these static attributes of populations. While it is possible to describe a population at any point in time by any or all of these attributes, it is also possible to follow populations through time and the changes it brings. Changes in physical location result from either an expansion or contraction of a population's physical base; while changes in size, distribution, and composition result from demographic processes: fertility, mortality, and migration.

It is assumed that location, size, distribution, and composition represent (or move toward) an equilibrium which is a balance of the ecological complex (Hawley, 1967). The demographic processes are then a set of alternative responses available to a population seeking equilibrium.

Organization: Populations organize in many different ways, and several definitions of organization exist which embrace a number of organizational dimensions. We have selected a *narrow view of organization* realizing that this will limit what can be said about it. Our definition derives primarily from the work of Gibbs (1959:29-36; 266-277).

Organization is viewed as a property of population and refers to the constellation of its activities which provide its livelihood. These activities we call sustenance activities, and Gibbs argues that they ". . . are abstracted from the total human behavior and specifically exclude all activities not directly related to livelihood" (Gibbs and Martin, 1959:30). Thus, the important question is whether the activity helps the population to sustain itself. The organization of activities is of prime interest to the ecologist. In modern societies where money is the medium for sustenance, any activity yielding money is a sustenance activity. With this definition, activities, not individuals, are relevant; and the activities and their organization are the units of analysis.

A population's sustenance organization can vary along two interrelated dimensions: (1) the number of occupational niches and the population's distribution among them, and (2) the degree of interdependence be-

tween occupations. The latter is conceived of as depending on the former because we assume that as the number of occupations increases, the degree of specialization in each increases. Thus, to study changes in population organization, we must focus on changes in the structure of the occupational pyramid and changes in the interdependence among occupations.

Environment: Hawley has defined environment as "... all external forces and factors to which an organism or aggregate of organisms is actually or potentially responsive ... environment refers to the medium in which an organism exists" (1950:12). This medium includes other populations. Schnore (1966:31-58) refers to those factors directly related to the population's physical location as site factors, and those related to a set of external exigencies not directly related to physical location as situational factors.

Schnore's major situational variable is population potential which is best conceived of in terms of the size of other populations, their level of development, and the distance separating populations. Thus, it refers to the potential for contact between populations and the potential of other populations to threaten the population under observation.

Technology: The definition of technology used in most ecological discussions focuses on a population's tools and techniques for adapting to its environment. Technology varies with respect to (1) the volume and variety of tools and techniques in use, and (2) their efficiency (Cottrell, 1955). Tools and techniques which lower the amount of energy expended by the population to accomplish a given adaptive task are viewed as more efficient. A population's technological structure at a point in time can be described in terms of these two dimensions.

THE ECOLOGICAL COMPLEX AND POPULATION MIGRATION

The preceding paragraphs merely specify the broader concepts of interest to human ecology and their variable properties. The major shortcoming of the ecological complex is that, other than specifying the interde-

pendency of the four concepts, little has been done to relate them and their properties to one another systematically (Duncan 1959:681-685). Hence, we have few testable ecological propositions. Furthermore, as noted above most ecological studies tend to view organization as the dependent variable. In accordance with Duncan's (1959:683-684) observation that any of the rubrics can be treated as the dependent or independent variable, we think it feasible to view population as the dependent variable. We have defined migration as a demographic response, and noted that it can be viewed as a variable property of population structure. We will now discuss the place of migration in the ecological complex.

Hawley has argued that populations seek equilibrium between each population and organizational property and that while no causative power can be assigned to population: "Demographic structure contains the possibilities and sets the limits of organized group life" (1950:78). Thus, while the potential (for) and limits (of) organization are self contained in the population, "The place of occupation poses the basic problem of adaption" (Hawley, 1950:80).

The fundamental question seems to be how do such imbalances come about? To view imbalances, we must examine the interdependence between rubrics. Every population must adapt to its environment; and we assume that adaption is mediated through the population's organization and technology. The environment contains site and situation factors, both of which influence the population's sustenance organization. Site factors limit the sustenance organization because they dictate a population's activities ($E \rightarrow O$). That is, if there is no iron there can be no extraction, production, and distribution of steel. Likewise, there can be no steel plow ($E \rightarrow T$); and agricultural organization will be restricted to small, selected plots of cultivated land ($E \rightarrow T \rightarrow O$). Furthermore, restricted land cultivation influences the size of the population which can be supported ($E \rightarrow T \rightarrow O \rightarrow P$).

Now assume some new technological breakthrough makes raw material available from another population ($T \rightarrow E$); this time, however, the environmental factor is a situ-

ation factor. This means that steel production and distribution become possible ($T \rightarrow E \rightarrow O$); creating a larger sustenance organization makes possible the support of a larger population ($T \rightarrow E \rightarrow O \rightarrow P$).

These overly simple examples illustrate Hawley's (1967) argument that all changes in a population's functioning organization result from changes in the environment or technology. To this we now add that changes in a system's organization produce disequilibrium between population and organization. Remembering that population is a mere abstraction lacking causative power, we see that only one thing can happen internally when such an imbalance exists. The population can alter itself through a demographic component of change—that is, a demographic response.

This is not to say that demographic components of change cannot influence organization. For instance, certain technological (contraception, for example) or environmental (disease) factors can operate directly on the demographic processes; and these in turn can influence organization by changing population structure, setting new *limits on and potential for* organization. Here we have a second situation in which a demographic response effects equilibrium between population and organization. Whether the demographic response is positive (to increase population size) or negative, depends on whether the change expands or contracts the organization, whether the change in environment or technology directly increases or decreases population, and what the state of equilibrium between population and organization happens to be.

Thus, the same kind of change in two different equilibrium situations will not elicit the same degree of demographic response. For instance, a new technological invention in population "A" which is in a state of equilibrium may expand the organization, and thus elicit a demographic response increasing population; however, the same invention in population "B" which is already too large for its organization may elicit no response, or a lesser response.

We have noted that each demographic component of change can be viewed as a demographic response, and we have observed the types of situations eliciting a

demographic response. Our next question is, under what conditions does a population employ the migration response.

Populations can respond demographically by changing their birth and death rates or by migrating. It is important to realize that demographic responses are highly interrelated: if one changes the others change also. This interdependence often makes it difficult to specify which demographic response is primary. We know that the time involved for a specific effect to occur varies depending on which response is used. The time available primarily determines which response is most heavily relied on. However, we know that to achieve a given effect with any response also requires a certain level of technology.

In the short run, migration appears to be the most efficient response. It can increase or decrease population more rapidly than can changing fertility and is more efficient in that it can be selective. Its selective aspect also indicates that population composition and distribution can be changed more rapidly through migration than through the fertility response. A simple example should make this clear. Let us assume that a population's sustenance organization expands as the result of a technological discovery. To fill these niches by increasing fertility would require many years. The pace of change, particularly in modern societies, demands that populations respond more rapidly than this. Moreover, migration saves the population the cost of sustenance and training for new members; whereas, with the fertility response the population must absorb these costs until new members can occupy the new positions.

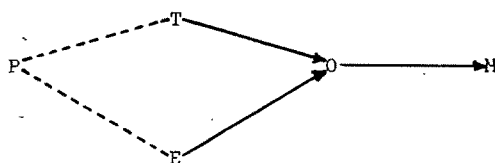
This simple example does not account for many complexities of change. For instance, technological discovery may destroy old niches in creating new ones. In this case, the population may redistribute its members in the new niches. However, if the activities of the new niches differ sufficiently from those of the old, the population may accept migration with the requisite capacity to perform the activities of the new niches. Moreover, if the sustenance organization has no niches open for those left without a niche by the new technology, the population must remove members or provide free sustenance

for them. *Population migration* then, is the effect of these counter patterns, and results from an imbalance in the relationship between population and organization.

While the major reason for reliance on migration may be timing, a prerequisite for its use is a technology sufficient to affect the response. That is, a population must have a technology which can put it in contact with other populations. It would seem that as technology improved, and made such contact easier, the migration response would be used more often, for less threatening changes. That is, a population can be less discriminating with respect to the magnitude of change needed to elicit the response.

Populations do respond directly to changes in the environment; however, such responses are always reactions to cataclysmic changes like wars or epidemics. Environmental change of a lesser magnitude effects migration by altering the population's ecological organization. Populations never respond directly to technological change because technology is effective to the extent that it alters organization. Thus, where increasing control over the environment is attained, demographic responses increasingly effect organizational change.

The above discussion suggests a model having two major propositions. First, we shall test Hawley's proposition that the causes of organizational change can be found in a population's environmental and technological conditions. Second, we shall test our proposition that migration is a response to changes in population organization. In short, our model argues that environment and technology do not operate directly on migration but effect migration through changes in organization. Thus, variations in the migration rates of small and large aggregates alike can be explained by observing the effects of variations in the external factors on population organization. Schematically, and ecologically this model may be represented as follows:



where T represents technology, E represents the environment, O represents the organization, M migration, and P the population. Note that no causative power has been assigned to population. The lines connecting P with T and E specify only that the population exists under these conditions.

SOUTHERN NEGRO MIGRATION: A PRELIMINARY TEST OF THE ECOLOGICAL MODEL

To test this model we have focused on a relatively unexplored, but socially significant, population movement: The migration of the Southern Negro population from the "old cotton belt."

To be included in the sample a county had to have 25,000 acres in cotton as reported in the 1890 census. This criterion yielded a sample of 253 counties stretching in a nearly contiguous belt from South Carolina to Texas. The continued rural character of this area is indicated by the fact that by 1960 only eight percent of the counties contained a city larger than 50,000. Estimates of net migration were calculated for two decades (1940-1950 and 1950-1960) for each county's black male population, using the forward census survival ratio technique.² The estimates were converted into rates per thousand population at the beginning of each decade, and each rate was multiplied by a negative one for the statistical analyses.

The latter operation merely transposes the values and allows us to refer to those areas having the largest relative losses as having the highest net migration rates.

The above model focuses on relationships between rubrics; and thus, we selected the most salient organizational, environmental, and technological factors suggested by others to test our model. (Vance, 1929; Thomas, 1934; Johnson, Embree and Alexander, 1935; Vance, 1936; Lively and Taeuber, 1939; Street, 1957; Hamilton, 1964; Rose, 1964; Dillingham and Sly, 1966; Woafter, 1967; Stinner and De Jong, 1969). Under the *organizational rubric* we included

² A complete discussion of the procedure used to select the survival ratios and the technique used to estimate net migration can be found in Sly (1972).

(1) the differential stability of positions in the agricultural labor force, and (2) lack of access to nonagricultural employment; under the *environmental rubric* we included (1) the exigencies of cotton production, (2) acreage control programs, (3) competition from whites, (4) concentration of land holdings and (5) increasing farm size; and under the *technological rubric* we included the mechanization of agriculture.

The southern agricultural labor force is grossly composed of laborers, croppers, tenants, and owners. These positions increase in stability as one moves from laborer to owner. Laborers work fewer days out of the year, are offered no residence while employed, and their position can be created or removed as the situation dictates. Croppers, on the other hand, while susceptible to exploitations are guaranteed housing and are employed for the entire growing season. For their labor they receive a fixed proportion of the crop. Tenants rent their land for cash value and receive full benefit for their labor.

We obtained a crude indicator of the structure of the agricultural labor force by affixing an arbitrary value to each position, which can be isolated from data in the 1940 and 1950 agricultural censuses. Thus, we multiplied the total number of owners by one, the total number of tenants by three, the total number of croppers by five, and the total number of wage laborers by seven. We obtained a grand total by summing the resulting four values, and this was divided by the total number of persons employed in the four categories. The resulting figure we refer to as the "Index of Agricultural Structure."³ This index gives us an idea of the instability of the agricultural organization of each

county population. As the number of cropper and laborer positions increase, the instability of the organization increases and the value of the index changes accordingly.

Lack of access to nonagricultural employment can have two different meanings. It can imply that there are simply no jobs outside of agriculture, or it can imply that other jobs are available from which blacks are systematically excluded. With the measure suggested below there is no way to distinguish these possibilities. However by lack of access we simply mean being unable to secure a position outside agriculture i.e., we refer to the fact that blacks are concentrated in only a few positions.

The index developed here is based on the classification of employed black workers by major occupation groups in the 1940 and 1950 censuses. The classification "major occupation groups" was slightly different in 1940 than in 1950. For maximum comparability, we combined the 1940 categories "professional workers" and "semi-professional workers" into one category more closely corresponding to the 1950 category "professional, technical, and kindred." Likewise, the 1950 categories "clerical and kindred workers" and "sales workers" were joined to more closely correspond to the 1940 category "clerical, sales and kindred workers." Combining these categories left us with eleven categories of major occupation groups for each census.

At each census date the percent distribution in each occupational category was compared with the percent expected on the assumption that if workers had equal access to all occupations, they would be equally distributed among categories. The "index of occupational dispersion," then, is obtained by taking the difference between the actual and expected percent in each occupational category, squaring the differences, summing the squared differences, and extracting the square root of the sum.⁴

³ It is important to recognize that this index does not account for changes in the structure of the agricultural labor force. We use the term stability to refer to the relative permanence of the various types of niches. Thus, owners' positions are more stable than tenants', tenants more stable than croppers, and the position laborer least stable in that it is created and removed most easily. The referent then is to the position and number of positions, not to individuals occupying the positions. The index value is arbitrary and a function of the values assigned each position. The major point to note is that we assume equal intervals between niches.

⁴ One could use an index of dissimilarity, but this would require adapting some arbitrary standard population. While assuming an equal distribution among categories is also arbitrary, it seems more expedient than to argue that all counties in our sample should resemble some standard when we know so little about organization. That is, we know very little about what an ideal distribution would be.

The index of occupational dispersion tells us the relative degree of population dispersion among occupation categories. The alternative to measuring nonagricultural employment opportunities would have been to work with the single category percent of workers in agriculture or its residual. However, counties with a minimum distribution among the eleven categories tend to have a very large concentration in the agricultural category. Counties with successively lower proportions in agriculture tend to have higher proportions in each of the other categories, thus reducing the amount of variance for all categories. If we work with the single category, say the proportion of workers in agriculture, we would be working only with the variance for a single category. Thus, if a large proportion of workers are concentrated in another category (say laborers) this is not taken into account. With the technique proposed here each category makes an "actual" contribution to the overall score for any single county; and if blacks move out of agriculture and are merely segregated in another single category of occupations, this is taken into account.

The environmental indicators are relatively straight-forward and require little elaboration. The first we used was simply the percent of whites living in each county during 1940 and 1950. It assumes that the white population constitutes an external threat to the black which varies with the relative size of the white population. Our second environmental indicator is the white-nonwhite acreage ratio. This ratio was calculated from data in the 1940 and 1950 Censuses of Agriculture which reported acreage cultivated by color of operator.

At first glance this may seem merely another indicator of white-black competition, and it probably is to a large extent; however, we also expect it to reflect the exigencies of cotton production, acreage control programs, the increased concentration of land holdings, and the increasing size of farms because each of these tends to create a preference for white tenants and owners over black croppers and tenants.

Two technology indicators are also used. These are the average number of tractors per farm in each county and the average

per farm expenditure for gasoline. Both averages were calculated from data reported in the 1940 and 1950 Censuses of Agriculture.

THE HYPOTHESES AND THEIR TEST

The six specific hypotheses to be tested are listed below. Each states a relationship between one of our indicators and the migration rate:

1. The higher the index of agricultural structure, the higher the net migration rate.
2. The higher the index of occupational dispersion, the *lower* the net migration rate.
3. The larger the proportion of whites occupying the same area as blacks, the higher the net migration rate.
4. The higher the ratio of white to nonwhite land in agricultural production, the higher the net migration rate.
5. The larger the average number of tractors per farm, the higher the net migration rate.
6. The larger the volume of gasoline consumed for agricultural purposes, the higher the net migration rate.

The hypotheses are not stated in causal terms because they are designed to examine differences in net migration between populations. Despite this, they do carry a causal implication in that they state how Southern Negro migration varies with each factor purported to be responsible for it.

If our model is to receive support, four of the hypotheses (3, 4, 5, and 6) should reduce toward zero when the appropriate controls are introduced. Note that we are not concerned with the variance explained by the particular set of variables under consideration, but with testing a research model which states that relationships between environment and migration, and technology and migration can be reduced when examined, with other organization factors controlled for.

Our model implies several additional hypotheses. Each of these relates an environmental or technological factor to our organization indicators. While these relationships are not explicitly stated above, they will be discussed below. Table 1 shows the mean and standard deviation for each variable included.

To test the six above hypotheses the rates of male net migration have been correlated

Table 1. Means and Standard Deviations for the Dependent and Independent Variables: 1940-1950 and 1950-1960

Variable ^a	1940-1950		1950-1960	
	Mean	Standard Deviation	Mean	Standard Deviation
Net migration	.441	.311	.683	.232
I.O.D.	.936	.152	.861	.136
I.A.S.	4.71	.72	4.12	1.07
W.N.A.R.	27.5	10.9	43.3	14.9
%W	57.5	21.4	60.2	20.5
GAS	16.8	12.5	96.9	67.7
TRACT	.06	.04	.30	.20

^aThe following notation is used to designate the independent variables in this and successive tables: I.O.D. = the index of occupational dispersions; I.A.S. = the index of agricultural structure; W.N.A.R. = white-nonwhite acreage ratio; %W = percentage white; GAS = farm per capita gasoline consumption for agricultural purposes; and TRACT = the average number of tractors per farm.

with each independent variable as measured at the beginning of the decade (Table 2). The coefficients are all in the predicted direction, though low or moderate in magnitude. The model is supported to the extent that during the two decades, the index of occupational dispersion is the first and second most highly associated factor with migration. It is not supported to the extent that the environmental and technological factors are also related to migration.

If the environmental and technological factors operate as predicted, we would expect them to be more closely associated with the organization indicators than with migration. As we see, (Table 3) this tends to be the case. Moreover, these relationships have

a definite pattern. During each decade the environmental variables are more closely associated with the index of occupational dispersion and the technological variables with the index of agricultural structure.

Furthermore, this pattern indicates where the environmental and technological variables are more closely associated with migration than with the organization variables. That is, the white-non-white acreage ratio and the percent white tend to correlate higher with the index of occupational dispersion *and* migration than with the index of occupational dispersion.

While this may simply result from a selection of variables, it appears to support our general model in that we would expect

Table 2. Zero-order Correlation Coefficients between Rates of Male Net Migration and Six Independent Variables: 1940-1950 and 1950-1960

Period	Organization		Environment		Technology	
	I.O.D.	I.A.S.	W.N.A.R.	%W	GAS	TRACT
1940-1950	-.492 ¹	.177 ²	.086	.167 ²	.229 ¹	.314 ¹
1950-1960	-.302 ¹	.154 ³	.160 ²	.435 ¹	.117	.248 ¹

¹F ratio significant at the .001 level.

²F ratio significant at the .01 level.

³F ratio significant at the .05 level.

Table 3. Zero-order Correlation Coefficients between Two Environmental Indicators, Two Technology Indicators, and Two Organization Indicators

Environment and Technology	1940-1950			1950-1960		
	I.O.D.	I.A.S.	MIG	I.O.D.	I.A.S.	MIG
White-nonwhite acreage ratio	-.160 ¹	.117	.086	-.448 ²	.102	.160 ¹
Percentage white	-.404 ²	.117	.167 ¹	-.319 ²	.165 ¹	.435 ²
Gasoline consumption	-.160 ¹	.504 ²	.229 ²	-.027	.471 ²	.117
Tractors	-.220 ²	.499 ²	.314 ²	-.190 ¹	.377 ²	.248 ²

¹F ratio significant at the .01 level.

²F ratio significant at the .001 level.

farm technology to have greater impact on the organization of agriculture in particular than on sustenance organization in general. Furthermore, we would expect those environmental variables indicating white dominance to be more closely associated with the occupational dispersion of Negroes, since Negroes compete from a disadvantaged position and since whites dominate in more developed areas.

These considerations appear to support the idea that controlling for organization variables will reduce the association between technology and environmental variables and migration. To test our model further, we performed a partial correlation analysis. Since our organization indicators are crude and because they measure only the sustenance dimension of organization, we decided that to expect the partials to reduce to zero was unrealistic. We therefore anticipated merely a reduction toward zero.

The partial correlation analysis allowed us to examine the association between each environmental factor and migration, and each technological factor and migration, controlling for each organization indicator individually and in combination.⁵

Table 4 shows that the partials all reduce in the predicted direction at each successive level of control. Applying a statistical cri-

terion, we find that during the 1940-1950 intercensal period statistically significant zero-order associations existed between one environmental (the percent white) and both technological factors and migration. The partial correlation analysis reduced two of these associations (those between the percent white and migration, and gasoline consumption and migration) below the level of significance. During the 1950-1960 period the partial correlation analysis reduced only one of three statistically significant zero-order associations (that between the white-nonwhite acreage ratio and migration) below the level of significance. In short, with the argument that the observed relationships should merely move toward zero, we find total support for our model, while with the more stringent argument we find only partial support for the model.

DISCUSSION

The above analysis leads us to conclude that broadening our concept of organization to include more than sustenance may have further reduced the zero-order associations between environment and migration and technology and migration. For instance, certain legal tactics have been used to keep Negroes on the land; perhaps in more mechanized areas where the need for croppers and laborers is less, such legal restraints are reduced as the need for black labor is reduced. Or, perhaps new laws promote migration, e.g., new welfare laws.

The technology indicators and the index of agricultural structure were most highly

⁵ All the zero-order correlation coefficients needed to reconstruct complete matrices are included in Tables 2 and 3 above and path diagrams I and II below. We opted to present the zero-order coefficients in table form because this helps make more salient the theoretical considerations which are the paper's major focus.

Table 4. Zero-order Correlation Coefficients between Four Independent Variables and the Rate of Male Net Migration, and First- and Second-Order Partial Correlation Coefficients Controlling for the Index of Occupational Dispersion and the Index of Agricultural Structure: 1940-1950 and 1950-1960

Correlation Coefficient between the Rate of Male Net Migration and:	1940-1950			1950-1960		
	Zero- order	First- order Partial	Second- order Partial	Zero- order	First- order Partial	Second- order Partial
White-nonwhite acreage ratio	.086			.160 ¹		
Controlling I.O.D.		.008			.029	
Controlling I.A.S.		.066			.157 ²	
Controlling I.O.D. & I.A.S.			.008			.019
Percentage white	.167 ¹			.453 ³		
Controlling I.O.D.		-.040			.375 ³	
Controlling I.A.S.		.149 ²			.446 ³	
Controlling I.O.D. & I.A.S.			-.153			.371 ³
Gasoline consumption	.229 ³			.117		
Controlling I.O.D.		.174 ¹			.116	
Controlling I.A.S.		.164 ¹			.051	
Controlling I.O.D. & I.A.S.			.116			.059
Tractors	.314 ³			.248 ³		
Controlling I.O.D.		.242 ³			.204 ¹	
Controlling I.A.S.		.264 ³			.206 ¹	
Controlling I.O.D. & I.A.S.			.194 ¹			.171 ¹

¹F ratio significant at the .01 level.

²F ratio significant at the .05 level.

³F ratio significant at the .001 level.

associated during the 1940-1950 period. This could support Hawley's argument that the sources of change in organization are external, because it was during this period that the mechanization of Southern agriculture really began. It does, however, raise the question of how technological and environmental factors are to be handled once they are introduced in a population. Hawley argues that they are always external. It appears, however, that every population has a "technology in use" which is part of the system and cannot be defined as external to it.

What appears important, however, is that our research demonstrates the feasibility of viewing the population rubric as the dependent variable in the ecological complex. A next step would be to investigate the possibility of viewing the other demographic processes as responses to organizational change. This step poses several problems. For instance, we already know that mortality can be substantially reduced among certain populations by mere technological intervention from others. However, at a

certain point organizational change seems necessary to further reduce mortality. In certain populations where infanticide or gericide is practiced, mortality can be viewed as a response to organization.

Nor does this research clarify the situation with respect to migration. We have applied our ideas to a specific, non-typical, situation. What would have happened with areas of high in-migration or with a population not specifically Negro?

It will be fruitful to apply this model to relatively small areas with fairly large in-migrations. For instance, we could apply our ideas to the northern and western cities which have experienced large in-migrations of Negroes, or to areas associated with the rise of the space program. In these situations we would, of course, need to specify different sets of environmental and technological factors; but the general idea that these effect migration through their effect on the areas' organization could be tested.

While we cannot extend our analysis to other areas and situations, one other approach is open to us.

We have throughout our work followed the "Blalock methodology" (1960:337-343; 1968b, 1968c:155-198), and Duncan (1966:1) has noted that path techniques are particularly well suited to clarify the types of linear causal models Blalock suggested.

The path diagrams should not be expected to add anything new to what we have said, but as Duncan (1966:7) noted, they are invaluable, "As a pattern of interpretation . . ."⁶

The path diagrams, however, will allow us to consider more variables simultaneously, and to specify more clearly *how we see them* as operating. The reader should not overemphasize the import of these diagrams. They are based on cross-sectional data; and therefore, the time referent is inferred.

With this in mind we applied a multiple regression program to the data for all 253 counties, using the total Negro male net migration rate as our primary dependent variable. The path coefficients in the diagrams (see Figure 1: Path Diagrams I and II) correspond to the standard partial regression coefficients obtained through the multiple regression equation. The residual factors operating outside the system are the coefficients of alienation of the multiple regression on each intervening variable and the dependent variable, and indicate the variance in those variables left unexplained, with the system's variables accounted for.

The independent variables' intercorrelations (indicated by the curved, symmetrical arrows) on the one hand, tend to validate the rubrics and their variables. That is, each rubric's variables are more closely associated than the variables under different rubrics.

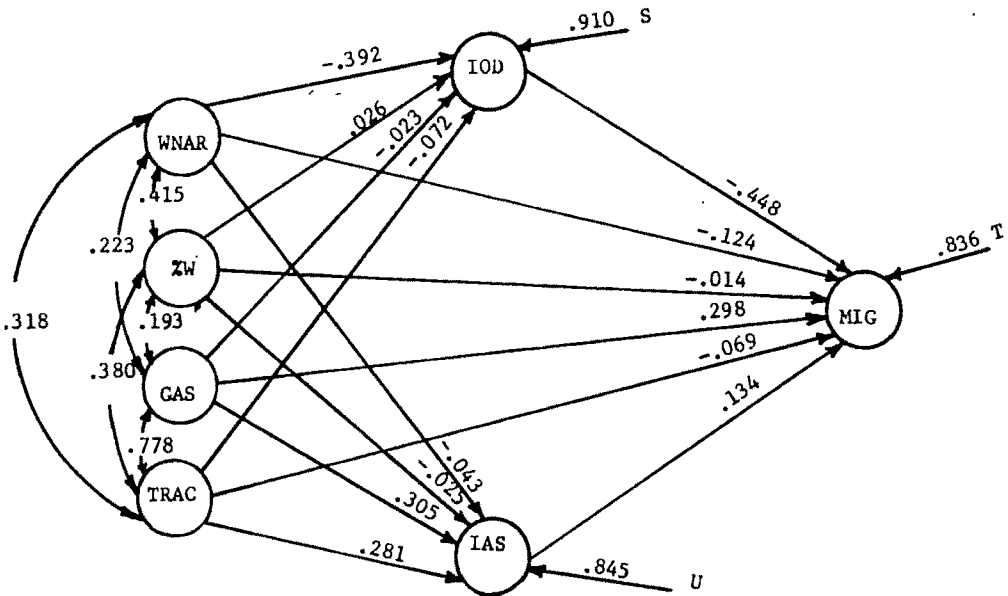
On the other hand, the high intercorrelations among variables under the same rubric violates the independence assumption of

multiple regression analysis, and as we will see below causes interpretation problems.

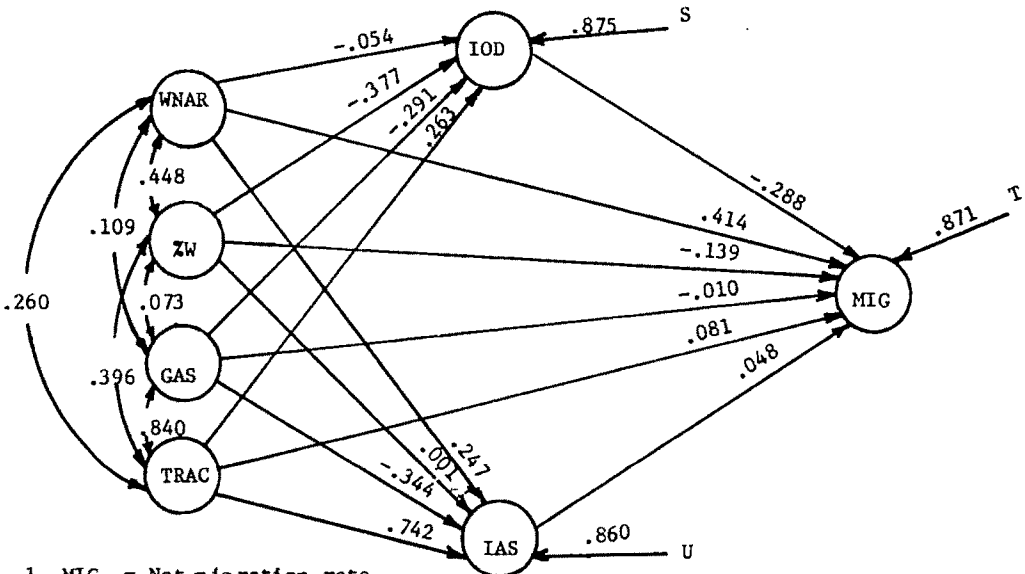
If we draw from both diagrams, several interesting observations can be made. In general both tend to support the model. For during both decades the paths connecting environmental and technological factors are stronger than those connecting these factors to migration. Note in particular the relative strength of the paths from the environmental factors to the index of occupational dispersion, compared to the paths connecting environmental factors and migration. Similarly, much stronger paths connect technological factors to the index of agricultural structure than connect technological factors to migration. Also at this general level, the occupational dispersion of the black population has a much stronger effect on the migration rate than does the agricultural structure. The latter observation suggests that where niches are available outside agriculture, Negroes are less prone to leave an area. Thus, perhaps not simply the structure of agriculture has been responsible for the out-migration of blacks as much as the lack of access to niches outside agriculture. If we compare the two diagrams, we can make several added observations. First, for the index of occupational dispersion we see that in the '40's only the white-nonwhite acreage ratio appreciably effected occupational dispersion; whereas in the '50's the percent white and the two technological factors had the greater effect. Thus in the '40's, where whites had relatively more land, blacks were more dispersed in the sustenance organization; however, in the '50's, it is among counties with the greatest relative number of whites that we find the greatest dispersion of blacks. It could be that modernization is more likely to occur where whites predominate, and blacks pose a lesser threat when relatively fewer in number. These possibilities are further suggested by the positive path connecting tractors to occupational dispersion. That is, in counties where tractors have been most strongly adopted, blacks are less occupationally dispersed; and the sign of the relationship actually changes over the two decades.

The structure of agriculture yields similarly interesting observations. For both

⁶ The path technique is particularly useful for the goals we set for ourselves earlier; namely, looking at the pattern of relationships between variables to decipher how the various factors act on the dependent variable. More ambitious projects such as trying to explain a large proportion of the variance or to reproduce the original correlation matrix, should await more rigorous operational indicators.

FIGURE 1.¹ PATH DIAGRAM I: 1940-1950.

PATH DIAGRAM II: 1950-1960



1. MIG = Net migration rate
 WNAR = White-nonwhite acreage ratio
 ZW = Percent white
 GAS = Gasoline consumption
 TRAC = Tractors
 IOD = Index of occupational dispersion
 IAS = Index of agricultural structure

decades the agricultural sustenance structure is most strongly influenced by technological factors; however, note the change in sign between decades of the paths between "GAS" and "IAS." Given the high correlation between "GAS" and "TRACT" for

both decades, one can assume that the bulk of gasoline consumed goes into tractors; and this raises the question of how gasoline use could have different effects during the two periods.

This is perhaps best approached by

noting the "WNAR's" increased effect on the structure of agriculture in the '50's.

That is, we can see that the ratio of white to nonwhite land greatly effects the organization of agriculture; and it could be that where this ratio has increased more laborers are employed and mechanization less needed. Note also the relatively low correlations in the '50's between the "WNAR" and "GAS" and the "%W" and "GAS."

Perhaps a more plausible explanation is that some counties diverted more gasoline to self-propelled cotton pickers and adopted more tractor accessories, reducing the need for wage laborers, croppers, and tenants. That is, land holdings became increasingly concentrated in white hands; and with increasing mechanization fewer blacks were needed in traditional roles.

Finally, we can see that for both periods the general sustenance organization has a greater effect on migration than does the agricultural organization. In the '40's, the only path from an environmental or technological factor to migration stronger than one of the organizational factors comes from "GAS"; whereas in the '50's the "WNAR" has the greatest effect. The reduced paths from the organization factors to migration in the second diagram indicate a leveling off effect from the '40's. This could, in fact, partly indicate the effectiveness of the migration response in the '40's.

Separate analysis of migration estimates for these counties, however, indicates that the average rate of migration continued to rise, and that the standard deviation continued to decrease. Thus, we are led to believe that perhaps migration in the '50's was more random and less a response to organizational differences. Spilerman (1971) reported more discontent among Southern blacks in general in the '50's, and this may indicate that migration is relatively less a response to organizational change.

Finally, we would note that attributing Southern Negro migration to a decline in agriculture greatly oversimplifies a complex problem. In fact, perhaps we confront not a decline but a reorganization of agriculture and a concomitant lack of opportunity outside of it. In this sense the areas studied and their subpopulations resemble many underdeveloped areas of the world.

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SYSTEM SIZE AND STRUCTURAL DIFFERENTIATION IN FORMAL ORGANIZATIONS: A BASELINE GENERATOR FOR TWO MAJOR THEORETICAL PROPOSITIONS

BRUCE H. MAYHEW
Temple University

ROGER L. LEVINGER
Temple University

J. MILLER MCPHERSON
University of Nebraska

THOMAS F. JAMES
Vanderbilt University

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Blau's (1970) two major theoretical propositions relating size and structural differentiation in formal organizations can be produced by the logical possibilities for differentiation that are generated by different values of size. The relationships specified by these propositions are illustrated with calculations from a computer algorithm that enumerates all possible structural forms for each size and assigns the same probability of occurrence to each structure. Theoretical implications of this model are discussed.

INTRODUCTION

BLAU (1970) has inductively derived and tested two major theoretical propositions relating size and structural differentiation in instrumental formal organizations. Ignoring the "causal" implication in his statement, these propositions are: (1) structural differentiation is an increasing function of size, and (2) the rate of increase in structural differentiation with respect to size is a decreasing function of size.¹ These propositions are of considerable importance because (1) a large number of more specific theoretical propositions can be derived from them, and (2) they appear to be empirically accurate (e.g., Hickson *et al.*, 1969; Blau and Schoenherr, 1971; Mayhew *et al.*, 1972). The object of the present paper is to note that, when other variables that may be expected to affect structural differentiation are held constant, Blau's two propositions can be produced by the

logical possibilities for differentiation that are generated by different values of size.²

Following Blau (1970), we define size (S) to be the number of personnel who comprise the organized labor force of the formal system and structural differentiation (D) to be the number of system parts in which employees may be assigned formal positions. Thus, D may refer to the number of different occupational roles, the number of levels of formal authority, the number of departments, etc. Observe that D is restricted to integer values in the range $1 \leq D \leq S$.

THE MODEL

A formal organization of size S can be structurally differentiated in exactly as many ways as there are integer sequences that sum to S (cf. Liu, 1968:41-45).³ For

¹ Although Blau originally stated these as one proposition, the discussions by Meyer (1971) and Hummon (1971) make it clear that we are dealing with two. It is not necessary to review the Meyer and Hummon analyses here. Meyer was concerned with deduction of several propositions from one of Blau's findings, an exercise which, Blau (1971) asserts, was based upon an erroneous assumption. Hummon was attempting to establish a purely deductive system, but, as the model that will be presented here illustrates, an elaborate set of assumptions such as those employed by Hummon are not required to generate Blau's propositions.

² The first proposition indicating a positive relationship between size and structural differentiation can be deduced from the baseline model presented by Childers *et al.* (1971), but their model does not consider all the structural forms generated by size. Mayhew *et al.* (1972) observe that the generalized harmonic series model permits them to deduce the division of labor interpretation of Blau's two propositions. The model to be presented here is more general in that it deals with structural differentiation. The relationship between the present model and the generalized harmonic series is examined in a subsequent report.

³ These integer sequences are generated ignoring order.

$S=3$, we have

$$\begin{aligned} 1+1+1 &= 3, \\ 1+2 &= 3, \text{ and} \\ 3 &= 3. \end{aligned}$$

In the first sum, three individuals (population elements) are equally distributed in three system parts (such as occupational roles, authority levels, etc.) so that $D=3$. In the second sum, three individuals are distributed in two parts, with one individual in one part and two individuals in the other, so that $D=2$. In the third sum, all three individuals are in one part, so that $D=1$. For $S>3$, the number of such sequence structures rapidly increases, as illustrated in Table 1 for $S=5$ and $S=7$. The structural forms shown in Table 1 illustrate how the sample space for the present model differs from that proposed by Childers *et al.* (1971).

For a specified value of S , let s be a distinct sequence of integers that sums to S , i.e., a structure, and let N be the number of such structures that can be defined for S . Let p_s be the probability of s and D_s the value of D assigned to s . For a specified value of S , we wish to construct a model

Table 1. All Distinct Sequences of Integers That Sum to $S=5$ and $S=7$, With Associated Values of D_s .

$S=5$		$S=7$	
sequence	D_s	sequence	D_s
5	1	7	1
4,1	2	6,1	2
3,2	2	5,2	2
3,1,1	3	4,3	2
2,2,1	3	3,3,1	3
2,1,1,1	4	4,2,1	3
1,1,1,1,1	5	5,1,1	3
		3,2,2	3
		2,2,2,1	4
		3,2,1,1	4
		4,1,1,1	4
		2,2,1,1,1	5
		3,1,1,1,1	5
		2,1,1,1,1,1	6
		1,1,1,1,1,1,1	7
$N=7$		$N=15$	

which permits us to calculate the expected value of D when each of the N structures is assigned the same probability of occurrence. Accordingly, we define

$$p_s = 1/N, \quad (\text{Eq. 1})$$

so that the expected value of D becomes:

$$E = \frac{1}{N} \sum_{s=1}^N D_s, \quad (\text{Eq. 2})$$

for any given S . Equation 2 defines a baseline model of structural differentiation, a model which holds constant (via equation 1) all factors that may be expected to affect structural differentiation except S , and it permits S to affect D only through the number of structural forms logically generated by S . Equation 2 gives the value of D we expect to observe in a large sample of organizations of size S even if no factors are operating in a causal fashion to produce patterns of structural differentiation. That is, equation 2 is a baseline model for S because it gives the expected value of D under the assumption of *random structuring*.

The following two propositions are generated by equation 2:

PROPOSITION 1: *Ceteris paribus, structural differentiation is a monotone increasing function of size.* That is, from the conditions specified in equation 2:

$$\partial E / \partial S > 0. \quad (\text{In. 1})$$

PROPOSITION 2: *Ceteris paribus, the rate of increase in structural differentiation with respect to size is a monotone decreasing function of size.*⁴ That is, for the conditions specified in equation 2:

$$\partial^2 E / \partial S^2 < 0. \quad (\text{In. 2})$$

⁴ We have illustrated the relationship between E and S only to $S=70$ due to computer time limitations (see Table 2 and Figure 1). Technically, therefore, we can claim that the model generates Blau's propositions only within this range of S . The high degree of stability that develops in the D_s distributions and the tendency of $\partial E / \partial S$ to shift toward zero at higher values of S gives adequate grounds for assuming that the same results will continue to hold for $S>70$. It is always appropriate to raise the question of whether an arithmetic mean of the D_s distribution (E) is a better description of system tendencies than the median or some other value. Analysis of the D_s distributions shows that they develop marked central tendencies as S increases. Accordingly, E is an appropriate descriptive measure. Copies of the computer program which generates E are available upon request from the authors.

The *ceteris paribus* condition in these propositions is generated by equation 1. This does not distinguish these propositions from those stated by Blau (1970); all propositions in a scientific theory are *ceteris paribus* statements.

We do not have a calculating equation for E in a form simpler than equation 2. Accordingly, we have constructed a computer algorithm which (1) enumerates all instances of s for a given value of S, and (2) calculates E via equation 2. The results of these calculations are shown in Table 2 and Figure 1 for S in the range 1-70. Table 2 lists values of E and the standard deviation of the D_s distribution for each S. Figure 1 illustrates the relationship between E and S, from which the two propositions listed above will be immediately apparent. The relation between E and S in the range of the calculations shown is given by

$\ln E = -.043 + .680767(\ln S)$, (Eq. 3)
with a coefficient of determination of approximately 1.0, and for $b = .680767$ we have

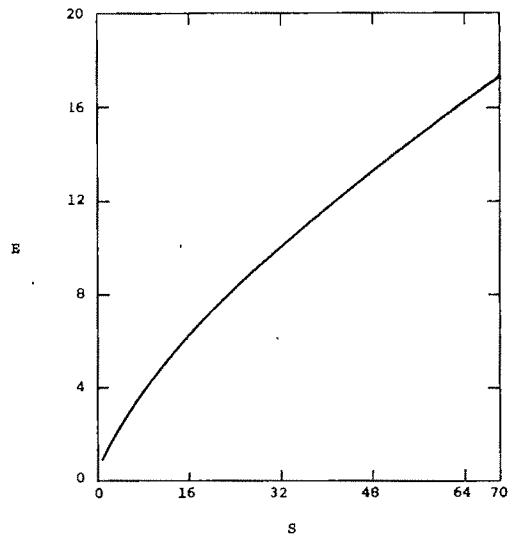


FIGURE 1. RELATIONSHIP BETWEEN E AND S IN THE S RANGE 1-70.

$$\delta E / \delta S = b(E/S), \quad (\text{Eq. 4})$$

and

$$\delta^2 E / \delta S^2 = - \frac{(bE)(1-b)}{S^2}. \quad (\text{Eq. 5})$$

Table 2. Expected Values of Structural Differentiation (E) and Standard Deviations in D_s Distributions in the Size Range 1-70.

S	E	Std.Dev.	S	E	Std.Dev.	S	E	Std. Dev.
1	1	.00	25	8.61	3.61	49	13.55	5.40
2	1.5	.50	26	8.83	3.70	50	13.74	5.46
3	2.0	.82	27	9.07	3.78	51	13.92	5.52
4	2.4	1.02	28	9.29	3.87	52	14.10	5.59
5	2.86	1.25	29	9.52	3.95	53	14.28	5.65
6	3.18	1.40	30	9.74	4.03	54	14.46	5.71
7	3.60	1.58	31	9.96	4.11	55	14.64	5.77
8	3.91	1.73	32	10.17	4.19	56	14.82	5.83
9	4.27	1.88	33	10.39	4.27	57	14.99	5.89
10	4.57	2.01	34	10.60	4.35	58	15.17	5.95
11	4.91	2.15	35	10.81	4.42	59	15.34	6.01
12	5.18	2.27	36	11.01	4.50	60	15.51	6.07
13	5.50	2.39	37	11.22	4.57	61	15.69	6.13
14	5.78	2.51	38	11.42	4.64	62	15.86	6.19
15	6.07	2.63	39	11.63	4.72	63	16.03	6.25
16	6.33	2.73	40	11.83	4.79	64	16.19	6.30
17	6.62	2.84	41	12.02	4.86	65	16.36	6.36
18	6.87	2.95	42	12.22	4.93	66	16.53	6.42
19	7.14	3.05	43	12.42	5.00	67	16.69	6.47
20	7.38	3.15	44	12.61	5.06	68	16.86	6.53
21	7.64	3.24	45	12.80	5.13	69	17.02	6.58
22	7.88	3.34	46	12.99	5.20	70	17.19	6.64
23	8.13	3.43	47	13.18	5.26			
24	8.36	3.52	48	13.37	5.33			

COMMENT

The use of a baseline model such as the one proposed here requires cautious selection of the social systems to which it is intended to apply. That is, if we wish to see how far some empirical system departs from the baseline's predictions, we must know that the unit of analysis in research is in fact at the level of organization where basic differentiation processes occur.

To illustrate, consider a study of formal organizations (such as Blau and Schoenherr, 1971) which has the opportunity to compare differentiation at the level of the total organization, at the level of major divisions (or departments) within organizations, or at the level of local offices. Assume also that the type of structural differentiation under investigation is the division of labor (that D is interpreted to mean the number of different occupational roles). The relationship between observed D and observed S may be examined for any of the levels of organization mentioned above. However, a comparison of these observed values with those generated by our baseline model is meaningful only for the types of systems (or subsystems) where differentiation into occupational roles is effected. If local offices structure their own division of labor, then it is at the level of local office systems that the basic differentiation process occurs. Under these circumstances, it would be meaningless to compare the relationship between observed D and S with the predictions from the present model at the level of the total organization.⁵ In such cases, differentiation into departments, divisions, offices, hierarchical levels, and similar macro-parts may be the only structuring process that occurs at the level of the total organization.

In summary, some prior knowledge of the system level at which differentiation processes occur is required in order to evaluate departures from baseline models. Research into decision making processes in

the organization being studied should provide clues as to the appropriate units of analysis.

Having made these precautionary observations, we may consider the theoretical implications of the baseline model defined by equation 2 for the relation between size and structural differentiation. As indicated, the model yields the expected value of structural differentiation for any given size when other determinants of structural differentiation are controlled. It allows size to operate on structural differentiation only through the number of logically possible structural forms generated by each size. Accordingly, assessing the "causal" effects (if any) of size upon structural differentiation requires that a measure of structural differentiation defined by

$$\Delta = |D - E| \quad (\text{Eq. 6})$$

be regressed on S . Only if this measure exhibits a functional relationship to size can we say that size affects structural differentiation in any way other than by creating a sample space within which D_s varies randomly. If we take the signed value of Δ , we have a measure that permits us to see where (if at all) the effects of size shift from producing increments (above chance expectation) to decrements (below chance expectation) in structural differentiation.

In other words, we can see where the effects of size lead to greater heterogeneity than chance expectation, on the one hand, and to greater homogeneity than chance expectation, on the other. Until such analyses are performed, we are without knowledge of any "causal" effects of size on structural differentiation. At the present time it appears that the two propositions central to Blau's theoretical framework can be generated by random structuring over the range of S .

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⁵ The values of E in Table 2 do not appear to be a bad estimate for the relationship between size and the division of labor—in the S range 1-70—at the level of local offices shown in Figure 2 of Blau's (1970:206) report. However, the degree of departure from the baseline can only be determined by a comparison with the actual data values.

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COMMENTS

COMMENT ON "SEX AND SOCIAL PARTICIPATION"

Booth (1972) provides an informative and useful report of his survey data with which he wishes to test my hypothesis about the relationship of species-specific sex differences and human social structures (1969). He identifies a set of regularities in the population under study which would appear to disprove the hypothesis in question. However, I would propose that his test is not in fact of the hypothesis outlined, and that his results paradoxically enough question the utility of the survey method in testing propositions about collective social behavior.

Booth gathered data from "... 800 non-institutionalized adults forty-five years and older residing in two Midwestern urban areas. . ." on their social participation in friendships, voluntary associations, and kin groups. There are three important differences between this population and that of the original hypothesis—namely the species in general as reflected in cross-cultural evidence from specific communities on male-female differences in social participation.

1. The first difference is in age and its effect on the sex ratios of the population. The 1960 ratio of men to women as shown by the census of the areas in question was .89. However, Booth's sample yielded a .52 ratio, so that the universe within which the social processes under question occurred was decisively different from a community showing a normal age and sex distribution. The fact is (as with many other primates) human male and female mortality differ considerably; so that, in this country, for example, females may expect to live some five to seven years longer than males. The effects of this on social behavior are considerable; it is a virtue of Booth's report that it suggests the significance of this phenomenon in structuring social patterns. Nonetheless, given these sex ratios in Booth's markedly skewed distribution of the community, it is questionable to assert that the general hypothesis about sexual differences in social action is untenable. For example, among under-45's

the role of sports could have shown up significantly in these data.

2. My claim was that the social affiliations of males were functionally-specific—as were the females'. Males, the argument was, were likely to cohere for particular purposes, such as fighting, defending communities, managing economics, manipulating the higher offices of political structures, and participating in certain recreational activities by joining secret societies and playing sports. I indicated that a serious omission of the study was its focus on male associations, but I suggested that female societies differed in foci, longevity, and intensity. (For an example of differences in overt form and actual function, see Scott, 1957.) While Booth's data question the intensity differences, they appear to support the differences in focus, and do not test the longevity difference. The central problem in relating Booth's data to the hypothesis is that Booth has not questioned his respondents about their work, politics, or agonistic activity. Insofar as part of the hypothesis was that males by and large excluded females from groups dealing with power, danger, wealth, and even important rituals, then one needs to try to explain cross-culturally the extraordinarily non-random incidence of female exclusion from the groups in question and male dominance of them as reflected in a wide range of studies (Lipset, 1963; Porter, 1965; Mills, 1956; Duverger, 1955; O'Neill, 1967; Epstein, 1970; Holter, 1970; Dodge, 1966; Gross, 1968; Wilensky, 1968; Sullerot, 1971; and critically commented on by, e.g., Greer, 1971; Millett, 1970; Bird, 1968; Janeway, 1971.)

Along these lines, the hypothesis suggests that kin relationships will be stronger and more persistent among females than males. In a recent publication, Tiger and Fox (1971) have argued precisely this. The picture which emerges reveals marked sexual differences in social participation once the whole range of social activities is taken into account—which Booth's method does not do.

3. Booth's sample is biased decisively by age and the partial nature of the social action he reviews. One must also be cautious in asserting

that it permits tests of any theory claiming to account for behaviors and social forms which occur cross-culturally—even cross-specifically, among primates. Had Booth conducted an identical study among the Australian aborigines in Arnhem land (Shapiro, Warren, personal communication), or among Newfoundland fishermen (Faris, 1966), or working-class English miners (Dennis, *et al.*, 1956), his findings would have been very different, and would have supported a thesis about sex differences even in the categories of activity which concerned him. The essential point in testing any theory purporting to recognize genetically-dependent factors in social action is that some fair samples of these factors must be used, not a sample of one.

Though it is only an analogy with no explicit predictive value for human patterns, it becomes intriguing that examples of sex-based social participation occur in other primate communities along the lines of the hypothesis. This does *not* mean that human behavior is necessarily subject to the same dependence on species-specific genetic propensity as other primates. But it does offer the theoretical possibility that explanations based on the use of genetic-dependent variables are appropriate for understanding even complicated social systems (Tinbergen, 1963; Eibl-Eibesfeldt, 1970).

Even if eight hundred uninstitutionalized persons in one area of the United States displayed no sexual differences in social participation once a fair sample of all age groups and a complete range of social activities were taken into account, it would not necessarily invalidate the hypothesis. Studying primates in the wild as opposed to the laboratory reveals that no one community displays the modal pattern of the species; to an unexpected extent, within particular primate species, "cultural" traditions, demographic balances, ecological circumstances, and fortuitous occurrences affect members' social behavior. Nevertheless, a pattern of social structure for each species becomes discernible; exceptions and rules are sturdily united by the principle of the Gaussian distribution.

Booth's test helpfully points up imprecisions in the theory as it applies to a specific situation, and he focuses constructively on what may be its limitation with respect to functional specificity. But the overall validity of his test

is severely limited by the absence of comparative materials.

Two last points. What people say about their behavior and what they do need not be the same. In Booth's argument males are inhibited from showing strong affect in the community in question; they may also be less willing than females to express those feelings of warmth, spontaneity, and commitment, which for Booth mean strong bonding. It would be much more interesting and useful to know what these respondents actually did. For example, an interviewer querying Americans or Canadians about their attitudes toward female political participation and their perceptions of what females did in politics would not likely predict the extraordinarily limited role females have so far played in structures like the U.S. Congress or Canadian Parliament.

Finally, it must be noted that the hypothesis treated not only the question of male and female social patterns but also the relationship between what have been viewed traditionally as broadly biological factors and social behavior, with biological sex used as the dependent variable. In this context, Booth's report reflects the notion that an appreciable dichotomy exists between biological and cultural factors. A more sophisticated conception of contemporary behavioral biology assumes cultural variation as a feature of an animal's overall system. As far as humans are concerned, cultural variation is predictable in formal terms just as human bodies, faces, and metabolisms may vary even though the species is marked by a clear, central tendency. (Bowlby, 1970; Tiger and Fox, 1971; Chapple, 1970; Hamburg, 1971; Harlow, 1958; MacLean, 1962; Pfeiffer, 1969; Fletcher, 1957.) The same argument has been made about language, one of the most, if not the most, distinctive human characteristics (Chomsky, 1966, 1968, 1971; Lenneberg, 1967; Lancaster, 1968). So the issue involves a wide range of materials and conceptions, including even the Darwinian one of why sexes exist in the first place and what this may mean for systematic social behavior.

In taking only one case to test a theory about the species, Booth invites the following description of his assumptions: if eight hundred persons adequately represent the whole group of humans, then humans must be very much alike. If they are very much alike, the

chances are that this is not only the result of cultural diffusion, the effects of technology, communications, etc., but also a common genetic input into various human social systems. If so, the genetic control of social behavior is considerable. Should this in turn be so, then it becomes important to define this genetic input accurately. While Booth recognizes that such genetic factors may be important in social behavior, his method impedes teasing them out. This is not to say "genetically programmed behavioral propensities" exist with a consequence of compulsory interest to social scientists (Tiger and Fox, 1966). But the possibility that they must be taken elaborately into account remains one of the most venerable uncertainties in our profession.

LIONEL TIGER

Rutgers University

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REPLY TO TIGER

Tiger, in his thoughtful comment on my research, makes two points; one, that I have not really tested his central hypothesis; and two, that the method and data I use are not up to the task.

That men and women have functionally specific roles in all societies is not the argument I attempt to test. Nor is the idea that men and women systematically exclude each other from certain groups. The only aspect of Tiger's work that I critically examine is his notion that males have a propensity to form stronger and more enduring ties with each other more often than women bond with other females. My data demonstrate a contrary trend, at least in certain U.S. urban areas. The number, intensity, and longevity of the friendship and kinship ties of women often exceed those of men. Furthermore, while men unquestionably dominate certain types of groups (instrumental ones), female participation in a wide range of such groups was far more active than expected. Thus, it would appear that in industrialized societies which encourage female membership in the labor force, sex differences may be diminishing.

With respect to the methodological aspects of my paper, Tiger questions the representativeness of the sample, the validity of self report data and the omission of cross-cultural data. The fact that women were slightly over-represented in the sample has little bearing on the basic findings. The analysis of variance design used in the study permits one to com-

pare groups of unrepresentative size providing both are representative with respect to family, occupation, education, and minority status. Thus, one can place confidence in the sex differences revealed by the data.

While self report data are not always consistent with behavior, I believe one can place confidence in self report social participation data. A study of patterns of influence indicates that people will, independently of one another, acknowledge each other's role in reaching decisions (Katz and Lazarsfeld, 1955:153-161). That is, when an individual reports discussing political candidates, fashions, movies, or other topics with another, his conversation partner also mentions their exchange (without aided recall) in a high proportion of cases. This study's cross validation of social participation activities justifies the use of self report data.

Tiger's call for cross-cultural data to test the hypothesis in question strikes a responsive chord. Data from a sample of industrialized and pre-industrial countries would provide a more severe test of the sex linked bonding patterns of men and women found in my investigation. The absence of such data should not, as Tiger would apparently advocate, discourage scientists from using whatever sources are available to them. Otherwise, little research would be accomplished, since ideal circumstances are rarely available.

Finally, I laud Tiger's call for a systematic study of human social organization. While attempts to show that differences in social initiative and aggression are rooted in physiological mechanisms have shown little promise as yet, few have examined biological, cultural, and social factors in concert. A substantial increase in such couplings will advance our knowledge of human social relations.

ALAN BOOTH

University of Nebraska

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HIGH SCHOOL CONTEXT AND COLLEGE PLANS: A COMMENT

To those not acquainted with the research literature on school context and college plans, Joel Nelson's article (1972:143-148) may give

an exaggerated impression of its originality and significance. The following observations are intended to help place the study in proper perspective.

First, the thesis that school contexts may have negative effects, as well as positive effects, on student aspirations has been suggested in many previous studies (e.g., Davis 1966, Werts 1968, Meyer 1970, Hauser 1971). Nelson's acknowledgement (144) of these is limited to the statement that "previous research suggests that the higher the average level of student intelligence in a school, the less likely any one student will feel academically competent (Myer [sic] 1970)." It should be noted that the focus of Meyer's article is *not* on feelings of competence but rather on college plans, just as in Nelson's study. Meyer reports that "the status and/or community resources of a high school have only a slight effect upon the student's decision to attend college—independently of the characteristics of himself and of his family" (59). He contends that:

The small observed effect of school status upon college intentions masks two contrasting forces. There is a larger supportive effect of school status than has usually been reported. But there is a negative effect also; since higher status schools have more able students, negative ("frog pond") comparisons for any given student tend to lower aspirations. (68-69).

Nelson appears to do little more than duplicate Meyer's finding and, in any case, fails to adequately report the relationship of his study to the former.

Nelson's article is also misleading to the extent that it gives the impression of being a critique of the Sewell-Armer (1966) study which tested the popular thesis that neighborhood status has an important positive influence on educational plans of youth in addition to that of sex, measured intelligence, and socioeconomic status. This is not the central issue in Nelson's study; and, in fact, he acknowledges toward the end of his article that his "findings agree with Sewell and Armer's observations that school status has no significant *net* effect on aspiration variability" (146). He then argues, however, that Sewell and Armer "explain the case differently" and that they suggest "that school status is theoretically unimportant because it fails to predict aspirations—over and above other variables" (146). For the benefit of those unfamiliar with the original paper, Sewell and Armer's conclusions were as follows:

Consequently, it may be concluded that although neighborhood context makes some contribution to the explained variance in college plans over and above that made by traditional

variables, its added contribution is indeed small. These results should not be interpreted to mean that neighborhood context can be dismissed as a factor in educational aspirations of youth. . . . Nevertheless, the results of the analysis reported in this paper indicate that past claims for the importance of neighborhood context in the development of educational aspirations may have been considerably overstated. Whether more direct measures of normative climates than the socioeconomic level of the neighborhood or school, or other measures of neighborhood or school climates, would reveal a closer relationship with educational aspirations must await new evidence based on actual assessment of these climates. Such evidence was not available in this study, nor has it been presented in any of the past studies. Whatever the prospects for future research, the present study clearly casts doubt on the popular notion that the socioeconomic status of the neighborhood in which the youth resides has a substantial influence on his educational aspirations that cannot be explained in terms of his sex, ability, and socioeconomic status. (167-68).

Sewell and Armer do not suggest that "school status is theoretically unimportant"; rather they point out that it does not have the strong positive influence on college plans that, at the time, many claimed it to have. Among the possible reasons for this negligible effect may be (a) measurement limitations, (b) contradictory influences as suggested by Meyer (1970) and repeated by Nelson (1972), (c) less extensive and intensive pressures of school and neighborhood contexts relative to other more proximate influences, (d) variability in school and neighborhood influences on individuals corresponding to differences in ability and background, and (e) other factors. In short, the door is left open for alternative theoretical interpretations of the small effects discovered in the original study. Indeed, before settling on any single interpretation, one would need adequate evidence supporting it *and* ruling out remaining alternatives; such evidence is not yet available.

Finally, Nelson's claim that "the above data are critical for reopening the debate on the effects of high school status" (147) seems exaggerated. In general, his findings tend to replicate those of Sewell and Armer (1966) in reporting negligible net influences of school context on college plans, and those of Meyer (1970) in testing one plausible interpretation of these results. Without detracting from the merits of the article, these observations will hopefully contribute to its proper evaluation by other readers.

MICHAEL ARMER

Indiana University

WILLIAM H. SEWELL

University of Wisconsin

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REPLY TO ARMER AND SEWELL

Numerous studies have examined the influence of the socio-economic status of schools on college aspirations. Although some results have been mixed, a number of recent, carefully designed research efforts (Hauser, 1969; McDill, Myers and Rigsby, 1967; Sewell and Armer, 1966) have reported only meager correlations between the two variables, lending weight to the argument that school status does not explain college aspirations. The assumption implicit in these results has been clear: the search for understanding aspirations had best be directed elsewhere.

My own work was directed to these recent efforts. I argued that the low correlation between school status and college aspirations did not necessarily mean that school status was without consequence. The potential of complex and contradictory effects could not be ignored. My research data suggested that "the direct effects of school status on aspirations are being cancelled out by negative indirect effects of school status on aspirations related through rank placement" (Nelson, 1972:147). School status did appear to have consequences for aspirations—multiple consequences which cancelled each other out.

That these results "duplicated Meyer's findings" is a trivial criticism irrelevant to the scientific process. A central concern of science

is to repeatedly document the stability of findings. Whether results are first or last is a matter of gamesmanship—an issue that intersects extraneous criteria into the evaluation of scientific research. In addition, Meyer's (1970) study, the only other piece on the negative effects of high school status on aspirations, does use measures of academic competence as the chief mediating variable. I argued that these are not merely perceptual effects but are probably related to such behavioral indicators of competence as academic rank. Incidentally, the other references cited by Sewell and Armer (Davis, 1966; Wert, 1968) are to college quality and graduate career choice, a somewhat different issue.

I intended to use these findings to critique existing research—particularly Sewell and Armer's which represented a major, widely quoted attempt to analyze the utility of school status. I did not intend to criticize their methodology, which has been treated at length elsewhere (Boyle, 1966; Michael, 1966; Smith, 1969; Spady, 1970; Turner, 1966). I merely argued that their explanation of the low correlation between school status and aspirations was incomplete. The major thrust of the Sewell-Armer piece is that school status can be discounted. My article took pains to show that while this was in one sense true it was also probably clear that school structure had rather complex effects. Such effects are not to be minimized. The routes between prediction and explanation can be traveled two ways: we use such variables as school status to predict and understand college aspirations; if aspirations are predicted successfully, they reveal something about the workings of school structure. The Sewell-Armer piece failed to reveal some important dimensions of school structure. Hence, their understanding of school status was incomplete. I see no reason to change my judgment on that.

Nor will I withdraw my suggestion that the questions on school structure be reopened. As I argued, small correlations cannot be discounted. Furthermore, most literature in this area has had difficulty in not explicitly advancing theories as to why social structures should exert an effect. In the absence of theory, seemingly negative evidence comes to be handled in a bewildering way. Testing for the effects of school structure does not substitute for an explicit test of a theory. In this sense, the debate on school effects has yet to be settled.

JOEL I. NELSON

University of Minnesota

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ERRATUM

Professor Seymour Spilerman's article, "The Analysis of Mobility Processes by the Introduction of Independent Variables into a Markov Chain," published in the June 1972 issue of *ASR*, contained the following printing errors. On page 282, equation (6) $n_{j,R}$ should read $n_{i,R}$; also, C_R refers to the set of observations over which the summation is taken. On page 293, at the bottom of the left column, matrix brackets should enclose the array of Y 's. Also, the second column of dots should be moved right to begin below the right parenthesis of the upper line.

ITEMS (Continued)

versity. He is interested in the effects of industrial systems on their physical and social environments, and he is studying the effects of occupation on political attitudes. **Darwin L. Thomas** is Associate Professor, Child Development and Family Relations, and a member of the Executive Committee of the Family Research Center, Brigham Young University. His major research is in socialization, and particularly in moving beyond the parent-child link to other dyadic and group characteristics of family interaction. **David D. Franks** is Assistant Professor of Sociology at the University of Denver. His interests center on the effects of social structure on individual selves, including analysis of structural influences on forms of interpersonal interactions conducive to the generation of knowledge, evaluation of open schools on self-concept and self-esteem of black elementary children in Kansas City, and effects of college experiences on changes in student attitude responses. **James M. Calonic**, Assistant Professor of Sociology, Louisiana State University in New Orleans, is involved in research on interpersonal perception, especially as this involves conjugal, intergenerational and sibling relationships in the nuclear family.

■ **David F. Sly** is Assistant Professor of Sociology at the Population-Manpower Research Center of the Institute for Social Research, Florida State University. Currently, he is on leave to the Fertility

Studies Section of the Population Division, United Nations. He is studying current trends in modernization and demographic behavior, universalistic and particularistic sexual structures and fertility, components of demographic change in U.S. metropolitan areas, 1960-1970, and a special case of racial vs. ethnic segregation: Blacks and Cubans in Miami.

■ **Bruce H. Mayhew** is Associate Professor of Sociology at Temple University, where **Roger L. Levinger** is a graduate student. **J. Miller McPherson** is Instructor of Sociology at the University of Nebraska and **Thomas F. James** is a Ph.D. candidate in sociology at Vanderbilt University. All share research interests in complex organizations and mathematical models of social phenomena.

■ The issue closes with comments and replies concerning two previously published papers and, alas, with an erratum.

■ One final note: research obligations in Chicago require Deputy Editor Lois DeFleur's presence in that city a great deal these days. We have therefore added to the editorial staff Lee Freese as a second Deputy Editor. Professor Freese has worked with us since last summer but his name first appears on the masthead in this issue. We welcome him aboard, with thanks for his excellent services.

J.F.S.

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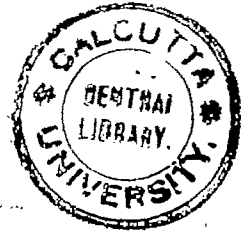
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Volume 37 Number 6

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Format of References in Appendix

List all items alphabetically by author and, within author, by year of publication, in an appendix, titled "REFERENCES." Use no italics and no abbreviations. For typing format, see the following examples:

Davis, K.

1963a "The theory of change and response in modern demographic history." *Population Index* 29(October):345-66.

1963b "Social demography." Pp. 204-21 in Bernard Berelson (ed.), *The Behavioral Sciences Today*. New York: Basic Books.

Goode, W. J.

1967 "The protection of the inept." *American Sociological Review* 32(February): 5-19.

Moore, Wilbert E., and Arnold S. Feldman.

1960 *Labor Commitment and Social Change in Developing Areas*. New York: Social Science Research Council.

Sanford, Nevitt (ed.)

1962 *The American College*. New York: Wiley.

MODIFICATION OF INTERRACIAL INTERACTION DISABILITY: AN APPLICATION OF STATUS CHARACTERISTIC THEORY *

ELIZABETH G. COHEN AND SUSAN S. ROPER

Stanford University

American Sociological Review 1972, Vol. 37 (December):643-657

This experiment attempted to produce equal-status interaction among four-person interracial groups of junior high school boys by assigning a high level of competence to the black subjects on two related tasks. Each treated group played a criterion game where the probabilities of whites and blacks being active and influential could be measured. Previous research has shown that in untreated groups the racial status characteristic becomes activated, i.e., whites are much more likely than blacks to be active and influential.

Results: unless the expectations for black competence held by both whites and blacks are treated, whites will dominate the interaction in the criterion game. When expectations of the whites are treated by having the whites serve as students of the black teacher, behavior on the game approximates an equal status pattern. The strongest treatment involves spelling out the relevance of the training tasks to the criterion game.

Implications for school desegregation are discussed.

THIS research concerns "equal status interaction" experimentally produced in interracial groups of junior high school boys. The underlying theory originates in a long-established tradition of social psychology beginning with Cooley and Mead. We assume, as did Cooley, that objects of the social world are constituents of the subject's mind and self (Coser, 1971:305). Thus, in a small group setting of black and white boys, we do not see a clash and combination of independent personalities; each person holds some internal image of the other.

Furthermore, this internal representation of the other is much affected by the visible fact of differing status, in this case, different races. As Mead said, through a process of "taking the role of the generalized other" one comes to be influenced by the attitudes of those about him (Mead, 1934). In an interracial group, each member has an idea of the expectations of every member of a different race toward him as well as ideas

of the expectations of every member of his own race toward each other.

Theory of Status Characteristics and Expectation States

In more recent work, these ideas have been expressed systematically in "The Theory of Status Characteristics and Expectation States" (Berger, Zelditch, and Cohen, 1966). This theory explains how the power and prestige order in a newly constituted group of high and low status members comes to parallel the relative ranking on a status characteristic initially held by members of the group. Berger *et al.*, cite a body of empirical research showing that initial differences in social status produce a power and prestige order resembling the ranking in status on new group tasks where competence at the task has no rational relationship to the state of the status characteristic (Berger, Cohen and Zelditch, 1966).

Status characteristic theory explains the small group ranking on power and prestige by proposing that group members use the

* This research was supported by NSF Grant GS-1833.

knowledge of each other's status (when other bases for estimates of competence are lacking) to evaluate probable competence. These evaluations become the basis for a self-fulfilling prophecy, whereby the high status member expecting to be more competent at the new task, initiates more remarks from the beginning of the interaction and becomes more influential as the members interact. The low status member, expecting to be less competent than the high status member, initiates less at the outset, over-evaluating the contribution of the more talkative high status members and underevaluating his own contribution. These initial evaluations of self and other are described in the theory as "general expectations based on diffuse status characteristics" and are activated under specified task conditions.

Race is seen as an instance of a diffuse status characteristic. Race meets the requirements of the definition of this concept because it is associated in people's minds with a set of specific beliefs involving valued and disvalued characteristics. In addition to such specific beliefs there are general expectations about how well actors will perform in a wide range of situations. The belief that blacks are generally less competent than whites is imbedded in contemporary culture. At any point in time we cannot estimate what proportion of whites and blacks hold this belief; but theoretically speaking, a consistent tendency for blacks to be less active and influential than whites in interracial work groups meeting the theory's task specifications indicates the existence of "racist" beliefs in a significant proportion of the members of both races.

Evidence of White Dominance

Empirical evidence exists of white dominance in social situations where races interact or situations where blacks compare themselves to "significant others" of the two races. In two studies conducted at a northern university, Katz, Goldston and Benjamin found that black students felt socially inhibited by and inferior to white partners in a cooperative problem-solving situation. They made fewer proposals and spoke more to whites proportionately than to each other. Even when matched on intelligence and made

to display equal ability on certain group tasks, blacks ranked whites higher on intellectual performance, preferred one another as future work companions and expressed less satisfaction with the group experience than did whites (Katz and Benjamin, 1960; Katz, Goldston and Benjamin, 1958).

Preston and Bayton (1941) found that when students at a black college were told that their scores on intellectual tasks matched the average scores of white students, they tended to set their goal levels *lower* on the next few trials than they did when told that their scores equalled those of other blacks. Other studies highlight the finding that blacks can behave in an entirely different way when faced with white as compared to black frames of reference (Hatton, 1965; Katz, Epps and Axelson, 1964; Katz, Roberts and Robinson, 1965).

Interracial Interaction Disability: Previous Research

In our earliest research we named the phenomenon of white dominance in interracial work groups "interracial interaction disability." To determine if race were a clear instance of a diffuse status characteristic, we designed a game, called "Kill the Bull," meeting the conditions specified by the theory of status characteristics. In fourteen of nineteen groups of two black and two white junior high school boys matched on social class and attitude toward school, we found that a white actor was the most active on the task; and in thirteen groups a black member was the least active (Cohen, 1972). Whites were also more influential according to an independent measure of whose suggestion was taken as the group's final decision.

Note that subjects were given no other basis for evaluating relevant competence for this game, a scope condition of the theory. Other important conditions were the game's emotionally involving nature and its collective structure, which demanded that the group come to a series of team decisions about which way to proceed on the game board. Thus to reach the consensus demanded by the rules, the group was forced to evaluate each individual's suggestion and to select one as best.

The results were interpreted as a function of general expectations based on race infecting this new situation; the process starts with the higher activity rate of whites, who are expected to be and expect themselves to be more competent. This initial difference in activity rate, in turn, led to stronger white influence over decision-making and leads to whites being more active and influential than blacks: a self-fulfilling prophecy.

Modification of White Dominance

Finding a way to modify white dominance in problem-solving groups is central to the attempt to integrate schools and vocational settings. If the status ordering is a mirror of American society, interracial contexts, whether educational or vocational, are in serious trouble. The oft made assumption that one has only to join blacks and whites on an officially "equal" footing in the same building for "equal status" relations to develop is not sound. If we are correct in assuming that general cultural expectations about the relative competence of blacks and whites are likely to infect supposedly "equal status" situations, then it is distinctly possible that white dominance will continue in affirmative job action programs and in political settings.

For the past five years we have used the theory of status characteristics as a basis for conceiving ways to produce the desired types of "equal status" interaction in interracial work groups of schoolboys. An obvious inference from the theory is to disrupt the process by altering the players' expectations at the start of the game. This might be accomplished by giving group members an expectation training task at which the blacks are clearly more competent. When members play the game, they will have two prior bases for evaluating competence; knowledge of the other person's race and knowledge of the black member's strong competence on the training task. As the theory implies, the training task should arouse no specific cultural expectations for black competence, as might music or athletics.

Katz's earlier experiment with assertion training is an instance of this type of intervention; and it succeeded somewhat in making the black college students more assertive

(Katz, Cohen, 1962). The first attempt to modify racial status ordering by this method at Stanford's School of Education was not successful. In this study, (Expectation Training I) biracial four-man groups built a radio crystal set together as a training task. Unbeknownst to the subjects, the blacks saw a different and better film on how to build the set. Their instructional videotape included the opportunity to practice steps with real parts; while the film shown to the whites gave no such chance and was not as directive. The group then reassembled and built one large model of a radio crystal set together. After this training task they were given the instructions on "Kill the Bull" and played the game, as subjects had done in the early study.

Experimental and control groups in this study did not differ, and racial imbalance was as strongly evident in the experimental groups as in the first study (see Cohen, E. G., Lohman *et al.*, 1970). Analyses of the results led the investigators to conclude that the treatment had not produced a competence difference between the races that was *recognized by the subjects*. In groups where the black subjects (as rated by observers) did perform on the crystal set with a higher level of competence than whites, there was a marked tendency for blacks to underrate their own performance and to overrate white performance which carried over into the game situation.

HYPOTHESES AND DESIGN

The experiment reported here re-tested the hypothesis that the subject's knowledge and experience of superior black competence on a training task would alter the status structure in the criterion task. Superior black competence on a training task should produce less white dominance on the criterion task of the game than would be predicted for games not preceded by a training task. Compared to that of Expectation Training I, the manipulation of competence in Expectation Training II was stronger in impact and more complex in organization.

In this experiment, black subjects taught white subjects how to build a two transistor radio. This training task preceded the criterion task of "Kill the Bull." (1) The

blacks were given superior competence on two specific tasks that imply each other (building the radio and teaching how to build the radio). (2) The difference in competence was greatly strengthened by giving the black and white subjects roles implying a sharp difference in competence: the black subjects were teachers and the white subjects their students on the training task. (3) Clear evidence of black competence was presented to both black and white subjects by the use of videotapes which recorded and played back the competent behavior of the black subjects on each two training task components. Furthermore, the high-status, self-confident nature of black behavior was pointed out and reinforced for black subjects by the host experimenter before both black and white subjects. It was, therefore, very difficult for black subjects to deny their competence. (4) The technique of role modeling was used to produce self-confident, competent teaching behavior. In Lohman's study of the use of role modeling, he was able to produce more assertive black behavior by a role modeling film of effective blacks playing the game (Lohman, 1970). This study uses a role modeling film to achieve competent teaching behavior by black subjects in the shortest possible time. 7 The film shows a black teacher (the same age and background as the subjects) teaching a white student. Black subjects view it during their "teacher training" period. (5) Finally, a black college student is used to instruct the black students in the training task.

The training task has two parts: building a two transistor radio and teaching someone else how to build one. The rationale for using two tasks which imply each other is taken from the work of Freese who found that an assignment of competence on two directly related task characteristics was more likely to become relevant to a new task than an assignment on two inversely related specific task characteristics (Freese, 1969). Such a specific task characteristic would be the ability to speak well in public, compose music or throw a football accurately.

Definition 1

Specific Task Characteristic: An attribute

of an actor is a specific task characteristic if and only if:

- (1) there are at least two states, such that each state corresponds to a different probability of successful task performance, and
- (2) the states of the characteristic are differentially evaluated.

When competence is assigned on two specific task characteristics which imply each other (as in expectation training) Freese and Cohen speak of the creation of a general performance characteristic.

Definition 2

General Performance Characteristic: An attribute of an actor is a general performance characteristic if and only if:

- (1) there are at least two states, such that each state of the characteristic consists only of a set of symmetrically related states of specific task characteristics having the same evaluation, and
- (2) the states of the characteristic are differentially evaluated, and
- (3) each state corresponds to a different probability of successful task performance.

Freese and Cohen define some of the above terms as follows:

By *set* is meant a collection of at least two or more states of characteristics. By *symmetrically related* or symmetrically relevant, is meant that possession of any one member of the set implies the expected possession from p's point of view, of all other members of the set (Freese and Cohen, forthcoming).

This is a relatively new concept developed by Freese and Cohen in a recent experiment. They show that assignments of superior competence on a general performance characteristic become the basis for performance expectations rather than expectations coming from differences on a diffuse status characteristic. This formulation and experiment were partly a response to our early attempts to modify the effects of a racial status characteristic. The experiment was run concurrently with Expectation Training II (Cohen, Roper *et al.*, 1971). When information on a general performance characteristic is available to subjects, the diffuse status characteristic is no longer relevant to expectations on a new task, even if subjects are not informed that the new task is similar to the performance characteristic manipula-

tion tasks (Freese and Cohen, forthcoming).

In order to create a general performance characteristic for Expectation Training II and to make an assignment of competence, black subjects came to the experiment well in advance of white subjects. They had been matched with white subjects on general social status and attitude toward school by means of a recruitment questionnaire. Matching requirements produced a predominantly working class sample. They were approximately the same height as white subjects and were drawn from an area wide enough so that subjects did not know each other before the experiment. The trainer taught black subjects to build the radio and to explain the parts and their functions as they assembled it. The boys were taught two at a time without being given the chance to see or talk to each other during the training. When each individual reached a criterion level of competence, his performance in building the radio was videotaped.

Previous studies provided a body of data on black and white game behavior without expectation training. These materials were used as a control comparison for intervention studies.

There were three different treatments designed to test two separate sub-hypotheses: (1) Racial imbalance in interaction and influence is less likely to occur on the game if both black and white expectations are treated, than if only black expectations are treated. Interracial interaction disability is seen as a problem for both blacks and whites stemming from a general cultural set of beliefs. It is, therefore, critical to treat both blacks and whites to modify the racial imbalance on the game. If black subjects experience a success never witnessed by whites, whites will use only race and its associated general expectations to evaluate the game performance. Since blacks know that their competence will be judged by race only, their new self-confidence should be weakened. Treating black expectations alone by providing marked success experiences, is analogous to many attempts in the field of compensatory education.

The second sub-hypothesis is as follows: (2) Racial imbalance in interaction and influence is more likely to occur in the game

if the relevance of the training task to the game is not specifically stated than if it is. This hypothesis was formulated to discover rules for the transfer of training from one task to another. Because the long-range goal is to modify racial imbalance over a series of planned group experiences, we must find a basis for predicting transfer on a task series. Only when the transfer conditions are understood can a sufficiently powerful training situation be created for a generalizable and lasting carry over into a variety of situations.

Theoretically, diffuse status characteristics, general performance characteristics or specific task characteristics can become activated and relevant to new tasks.

Relevance is defined as follows:

An element e_1 is relevant to an element e_2 if it is the case that x_1 possesses e_1 , then x_1 is expected to possess e_2 .

If the experimenter were to tell subjects that their performance on the training task was relevant to their performance on the game task, we reasoned that the probability of expectations for the transfer of competence would be raised. This feature tested the value of spelling out relevance to strengthen the transfer of expectations from training task to criterion task. The hypothesis predicts that specifying relevance will raise the probability of transfer by this principle of perceived similarity (Berger and Fisek, 1970). In Expectation Training II, to avoid subject deception, competence in the building and teaching task was defined as self-confident verbal behavior. When the subject's behavior on videotape was pointed out and praised, his "self-confidence," his "speaking up" and his "explaining ideas" were reinforced. Being a good teacher was also defined in these terms. When relevance was spelled out, subjects were told that the same self-confident verbal behavior used in building and teaching radio, could make them a good team member. They were to use it in the game.

Comparison of Procedure on Three Treatments

Because the trainer and experimenter gave black subjects such elaborate and in-

tensive attention in preparing them for their role as teachers of white subjects, it was necessary to devote an equal amount of time to black subjects before the game in all three treatments. All three treatments required the use of a black teacher, a videotape for reinforcing behavior and both phases of the training task—successfully building and teaching the radio. To test the hypothesis involving the treatment of black expectations only (Treatment A), whites were not students of blacks but arrived just in time to play the game. Blacks learned to build the radio, were videotaped doing so and were reinforced before other blacks, but not before whites. They were taught how to teach, but when they had reached criterion, they were filmed teaching staff members; one taught the host experimenter¹ and the other the trainer. Videotapes of this successful teaching were again reinforced with the whites absent. Thus the whites went untreated in the black expectation treatment.

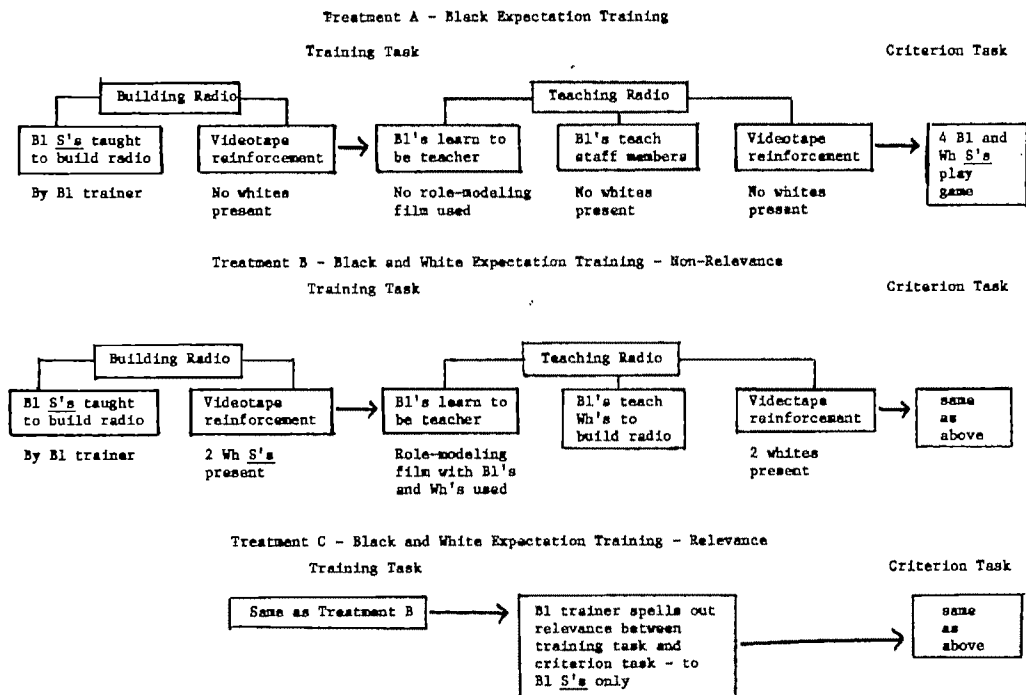
In the other two treatments, the whites

were shown the film of two competent black children building the radio; and the behavior on the film was reinforced by the host experimenter before all subjects. The two blacks then taught the two whites to assemble the radio. These sessions were filmed and reinforced before all four subjects. Black subjects were thus clearly assigned a high value on a general performance characteristic in the minds of subjects of both races; white subjects received *no* assignment on the general performance characteristic. After the assignment of competence, all four subjects played the game.

The second two treatments were designed to test the hypothesis of relevance. The only difference between the non-relevance (Treatment B) and relevance treatment (Treatment C) was that in the relevance treatment, the black trainer took black subjects aside just before the game, and explained that the self-confident verbal behavior they had exhibited as teachers would make them a good team member. The non-relevance treatment omitted this step.

The basic design of this experiment is diagrammed below.

Figure 1



¹ The host experimenter was Filipino.

MEASUREMENT PROCEDURE

Treatment of Data from the Game

Videotapes were made of the four subjects as they played the game. These were played back to one black and one white observer who scored each player's task-related verbal acts. The scoring system, developed in previous studies, contains four types of task acts: performance outputs, action opportunities, positive evaluations and negative evaluations. In addition to these categories, the scoring system designates the person making the response (initiator) and the person receiving the response (recipient). Previous experience with data from this system has shown that the most sensitive and reliable index of the power and prestige order of the players is the simple index based on number of task acts initiated by the four subjects in a given group.

Inter-Observer Reliability

Inter-observer reliability checks were made by having the two observers score the same groups independently. A chi square statistic was used to compare total initiation scores for subjects in a given group across observers. At the end of training, the observers' data yielded a chi square of .60, $df=3$. The probability that this difference occurred by chance is estimated to be above .85. Randomly selected checks over the scoring period yielded probabilities above .35 that the disagreement between observers was due to chance.

Post-Meeting Questionnaire

After the game, subjects were interviewed individually by staff members using a standardized questionnaire. Among other questions, subjects were asked to rank order team members, including themselves on contribution of best ideas, guidance and overall leader.

RESULTS—INITIATION

Rank Order Analysis of Acts Initiated

There were fifty-seven four-man groups, nineteen in each condition. The simplest way

to examine the results is to rank order the four players in each group on the number of acts they contributed to the game. The player who contributed the most acts receives the top rank. For this analysis, acts scored as performance outputs, action opportunities and positive and negative evaluations are all combined as "acts initiated" for each person. These four figures are then rank ordered within each group. For each treatment, one can then examine the probability of a black or white member holding top rank, second rank, etc. Table 1 presents the frequency of whites and blacks holding various ranks in the three conditions.

In the black expectation condition (Treatment A) whites are much more likely than blacks to hold top rank position; in thirteen groups a white member holds top rank. Blacks are much more likely to hold bottom rank than are whites; in fifteen groups a black member holds bottom rank and in four a white holds bottom rank. Overall, this rank order analysis resembles the earliest descriptive study of interracial interaction disability. It also resembles groups in the Expectation Training I experiment in which the treatment failed. Thus black success experiences accompanied by a powerful reinforcement procedure are *not sufficient* to modify the pattern of white dominance.

In the black and white expectation condition with non-relevance (Treatment B), the rank order pattern on initiation is greatly changed; probabilities of holding each rank do not differ by race. If a difference is discernible, it favors the blacks. Thus, in Treatment B which treated both whites and blacks, the differences between races in the probability of holding a given rank order position in initiation disappears.

When relevance is introduced as in the third treatment of black and white expectations (Treatment C), the probability of holding top rank is again unrelated to the race of the top-ranking member. Race once again, however, becomes a predictor of holding bottom rank: thirteen groups have a black member holding bottom rank as compared to six groups with a white member holding bottom rank. Using this particular measure of initiation, Treatment C does not appear to have altered white dominance as strongly as Treatment B.

Table 1. Frequency of Blacks and Whites Holding Each Rank in Task-related Interaction for Three Conditions

Rank in Group	Blacks	Whites
	Number of Groups	Number of Groups
Treatment A		
1 or 1.5	6	13
2 or 2.5	8	13
3	9	8
4	15	4
Treatment B		
1 or 1.5	11	9
2 or 2.5	7	11
3	10	9
4	10	9
Treatment C		
1 or 1.5	11	9
2 or 2.5	8	12
3	6	11
4	13	6

Analysis of Percent of Acts Contributed by Members

To examine the size of the discrepancy between players on initiation rate, we calculated the percent of all task-related acts contributed by each member to the group total. Previous experience in analyzing this type of data led us to rank the two members of each racial pair as the more active and less active of the two. In these biracial four-man groups, members of a racial pair often differ greatly in initiation rates. Both blacks are not consistently less active than both whites.

Table 2 presents the means of the percents of acts initiated for group members in each condition. The difference between black and white members ranked by activity appears to narrow when the expectations of both are treated (Treatments B and C), compared to the difference between race members in the black expectation treatment. The difference between the more active members of each race in Treatment A is 6.7%; while, in Treatment B, the difference dwindles to 2%. In Treatment C there is virtually no difference: the active black is

less than 1% higher than the active white. For the comparison between the less active members, there is a dramatic difference for the black expectation treatment (A) where the mean percent of the whites is 12% higher than that of the blacks. In Treatment B which treats both black and white expectations, the figure for whites is only 1.8% higher than that for blacks. In Treatment C, the mean percent of the less active black decreases slightly, so that the black-white difference again favors the whites by 4.31%.

Figure 2 shows graphically how the differences between the more and less active pairs narrow in Treatments B and C compared to Treatment A. According to this view of the data, Treatment C seems to be associated with the strongest boost in the black activity rate, though only for the more active black, unlike the rank order analysis previously presented. Similar to the results of the rank order analysis is the lowered activity rate for the less active black with relevance introduced.

Table 2. Average Percentage of Acts Initiated in Each Group by More and Less Active Member of Each Racial Pair

Activity Rate	Race	Average Percentage of Acts Initiated	N
Treatment A			
High	White	34.6	19
	Black	27.9	19
Low	White	25.4	19
	Black	13.4	19
Treatment B			
High	White	33.4	19
	Black	31.4	19
Low	White	18.3	19
	Black	16.5	19
Treatment C			
High	White	33.2	19
	Black	33.9	19
Low	White	18.7	19
	Black	14.4	19

Two kinds of statistical comparisons were made on these percents of acts initiated by blacks and whites in three conditions. One was a randomization test for two independent samples (the non-parametric equivalent of the *t*-test).² The difference in means between treatments was examined, two at a time.

The second compared the size of black-white differences *within each group* between conditions. This comparison was made with a Kruskal-Wallis Rank Test for independent samples.

Comparison of Mean Percentages Across Conditions

The randomization tests for two independent samples reported in Table 3 are based on the differences between treatments in the percent of all acts initiated in each group by black subjects. Comparisons are made of the means of all black subjects in each treatment and of the black subjects separated by activity rates.

These statistical tests illustrate what was pointed out in Figure 2: the size of the more active black contribution is boosted progressively higher from Treatments A to B to C until the comparison between Treatments A and C becomes statistically significant with

$p < .025$. The differences between A and B do not quite reach the .05 level. The size of the less active black contribution is not significantly altered between treatments. When the two black members of each group are combined, the contribution of the blacks in Treatment B is greater than that of blacks in Treatment A; but the difference does not reach the .05 level. The contribution in Treatment C similarly is bigger than in Treatment A, but the difference is not statistically significant.

Comparison of Black-White Percentage Differences

A study of Figure 2 in conjunction with the statistical tests reported above leads one to believe that the group members most modified by the treatment were the less active white and the more active black. It appears relatively difficult to change the behavior of the more talkative white or the less active black. What seems to have happened in Treatments B and C was that the relative contribution of the more active black was stimulated and the relative contribution of the less active white was depressed, so that he behaved like the less active black in number of acts contributed in a single group. If this trend, apparent in the picture, is not an artifact of having calculated averages of the nineteen groups in each treatment, then the difference between the more active black and the less active white should grow typically larger proceeding from Treatment A to B to C. The Kruskal-Wallis rank test can be used for independent samples on the difference scores for nineteen groups under each condition. This test examines the overall rank order of the difference scores on thirty-eight groups in two conditions. If the scores from the group in one condition are consistently higher in the rank ordering than those from groups in the other condition, the value of *H* reaches statistical significance. A simple inspection of these difference scores reveals the interesting fact that in seven of the nineteen groups of Treatment A both blacks are less active than both whites. Such a pattern yields a "minus" value on the difference score. Treatment B has four such groups; and Treatment C has only three.

² Siegel, S. *Non-Parametric Statistics for the Behavioral Sciences*. New York: McGraw-Hill, 1956, p. 152.

Table 3. Randomization Test for Two Independent Samples: Mean Percentages of Group Initiation Contributed by More and Less Active Blacks

Treatments Compared	<i>t</i> Score		
	More Active Blacks	Less Active Blacks	All Blacks
A X B	1.358*	n.s.	1.384*
A X C	2.148**	n.s.	1.299*
B X C	n.s.	n.s.	n.s.

* $p < .10$

** $p < .025$

Note: Scores based on 19 more active and 19 less active blacks in each treatment.

FIGURE 2

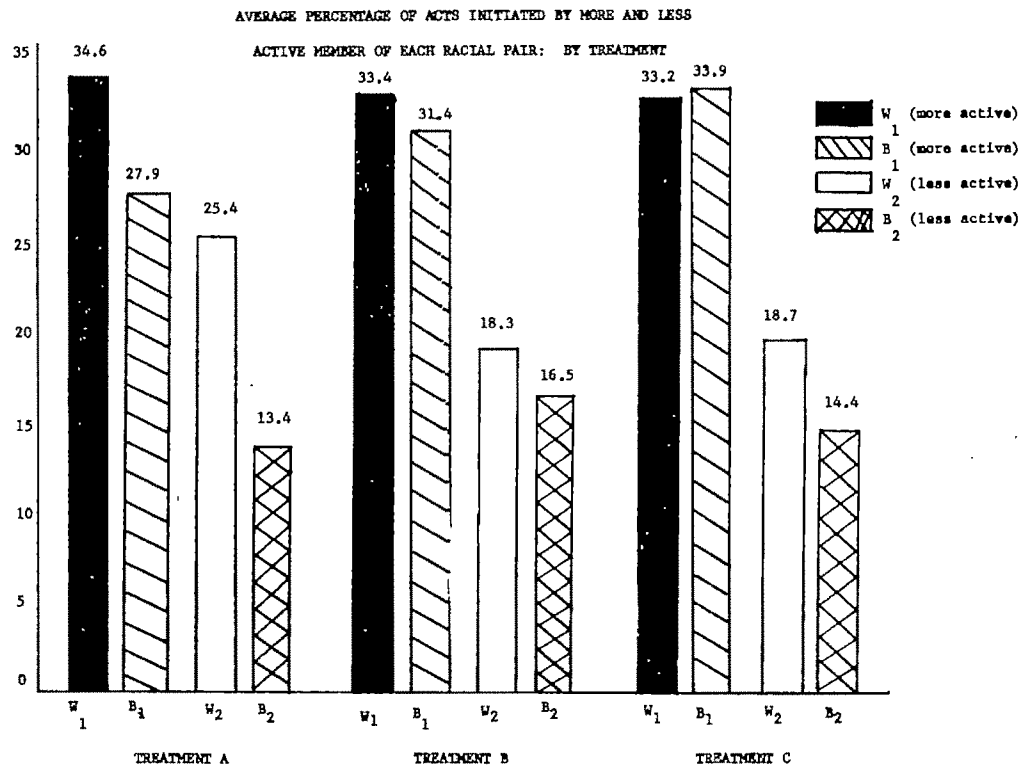


Table 4 gives the values of H for the comparisons between treatments. The comparison of Treatments C and A reaches statistical significance with p less than .02. The comparison of Treatments A and B does not quite reach the .05 level of statistical significance.

An analysis of the means of the percent of all acts initiated by black and white members highlights three features of the data.

a. Apropos of the size of the more active black's contribution, Treatment C is greater than any other; the means of Treatments C and A are significantly different.

Table 4. Kruskal-Wallis One-Way Analysis of Variance: Difference in Percentage of Acts Initiated by More Active Black and Less Active White

Source	H	P
A X B	3.33	p < .10
A X C	6.13	p < .02

- b. Apropos of the less active black, there are no significant mean differences between treatments. The mean contribution is somewhat larger under Treatment B than in the other two treatments.
- c. The increase in percent of acts of the more active blacks appears to be at the expense of the less active white's activity rate. The difference between these two members is significantly greater under Treatment C than Treatment A. Treatment B is clearly intermediate on this effect.

The Inactive Black and the Overactive White

The pattern of interracial interaction disability in Treatment A closely resembles that of earlier studies. As described in the rank order analysis, thirteen of the nineteen groups have a white as top-ranking initiator; and fifteen have a black as bottom-ranking initiator. Two hallmarks of white dominance in four-man groups have been the presence of a very quiet black and a very talkative white.

Observations of untreated groups often include the comment that the talkative white

is task-oriented and seems insensitive to the others. Observers of the inactive black often point out that though he remains involved in the game, he seems almost invisible to the others. From time to time he makes abortive comments to which no one responds. The analysis of mean percents of total group initiation in the previous analysis does not show a sharp change in these features of the interracial interaction disability pattern; rather it shows change in the *relative* initiation contribution of the more active black and the less active white. These changes appear in Treatment B and become very strong in Treatment C.

The size of individuals' total scores on acts and the number of acts scored by each group vary greatly. Table 5 presents a simple frequency count of "very quiet" subjects of both races who were scored for less than eleven acts. It also presents subjects who acted fifty-one or more times—certainly talkative performers by any standards.

Table 5 shows that the very quiet blacks do not disappear in any treatment, but in Treatments B and C they have been joined by an increasing number of very quiet whites. This is what we would expect from the above analysis of the percent of total acts. But the same thing does not happen to the very active blacks whose mean contribution to the group's total appeared to increase in Treatments B and C in the last analyses. Rather, the frequency of highly talkative whites drops to only one instance in Treatment C. There are just as many talkative whites in Treatment A as B. The frequency of very talkative blacks in Treatment C only increases to two. As a matter

of fact, few blacks have been observed making as many scorable remarks as certain hyperactive whites.

Why does the picture appear so different for the overactive white than the underactive black? The major reason is that Treatment C groups have a lower group total of task acts. It has only four groups with over 116 acts compared to seven groups with as many acts for Treatments A and B. Nine groups in Treatment C had less than seventy-six acts. From this simple analysis of raw numbers, Treatment C is seen to have the effect of quieting down potentially overactive whites.

RATINGS OF LEADERSHIP

In addition to the ongoing game measures, this study included a post-meeting questionnaire which asked subjects to pick the group member who appeared to be a leader. Table 6 presents the number of blacks and whites chosen as leader in the three treatments.

The results of this analysis parallel the results on mean percent of acts initiated. In Treatment A whites are much more often chosen leader than blacks. Treatment B is again intermediate, with about the same number of whites and blacks chosen. Treatment C again appears to have the strongest effect on black leadership with a much higher likelihood of the black being chosen leader than the white; and of the blacks, the active was chosen much more often than the less active.

In a separate study, the following two major questions were considered in detail: (1) how do white subjects respond to being put in the role of student and exposed to assertive black behavior, and (2) how do

Table 5. Frequency of Very Quiet and Very Talkative Players by Race

Type	Race	Treatment		
		A	B	C
Quiet	Black	8	6	8
	White	2	7	6
Talkative	Black	1	1	2
	White	6	6	1

Note: Very Quiet--10 or fewer acts;
Very Talkative--more than 50 acts.

Table 6. Subjects Chosen as Leader: By Race

Source	Black	White	Total
A	28	43	71*
B	33	32	65*
C	45	26	71*

*Missing cases are the raters who would not choose a single person as leader.

white reactions affect the behavior of black subjects? (Roper, 1971). With a socio-emotional scoring system, Roper found no evidence of a hostile response by white subjects to black teachers.

Roper's scoring system can be used to characterize the emotional tone of the three treatments. Combining all types of socio-emotional behavior considered scorable and comparing total group scores in all three treatments, we find that the non-relevance treatment groups exhibited the highest rates of socio-emotional behavior.

Some groups in the non-relevance condition showed the liveliest behavior in the four years of tapes collected on these interracial games. The players indulged in mock fights, made jokes, and offered joint prayers for the hoped-for roll of the die. A striking cohesion of group members was often evident; and they were often reluctant to leave after the experiment, enjoying the spectacle of themselves on TV. Having watched so many "invisible" black players in previous studies, we were struck by the lively participation of the black players. Clinical judgments were suspect owing to obvious biases in the investigator viewers. Rigorous observations were carried out by observers who were ignorant of the hypotheses and the differences between treatments.

INTERPRETATION

The most general hypothesis underlying both this and the last attempt at expectation training was that assigning a high degree of competence to low status subjects on a general performance characteristic would modify the effects of that status characteristic on a criterion task, i.e., that having high competence on the training task

would raise expectations for competence on the game (the criterion task). This general hypothesis was supported where both black and white expectations for black performance were treated (Treatment B and C), but not when only black expectations were treated.

In Treatment A, the racial status ordering called interracial interaction disability appears unchanged from previous studies. Providing black subjects with success experiences was not enough to change the interaction imbalance of blacks and whites.

With both black and white expectations treated in the two other conditions, sharp changes in the data appear. Thus the sub-hypothesis, that racial interaction imbalance is less likely to occur in the game with both black and white expectations treated was upheld. Rank order analysis reveals what happens in Treatments B and C most dramatically. Other analyses also show departures in the direction of "equal status" interaction in these two conditions compared to Treatment A and previous studies.

Furthermore, the relevance and non-relevance treatments differ. As hypothesized, spelling out the relevance between the training and criterion tasks, further decreases the probability of white dominance compared to omitting the "relevance" step. As for statistical significance, the non-relevance treatment does not always differ significantly from the black expectation only treatment; whereas the relevance treatment does. The non-relevance treatment does, however, consistently show an improved black status according to such indices as perceived black leadership, rank order of blacks in initiation rate, mean percent of acts initiated by the less active white. This treatment is clearly intermediate in its ability to modify racial status ordering between the black expectation treatment and the relevance treatment.

The hypothesis which treats the superiority of spelling out relevance is confirmed by the following evidence: (1) More black subjects are chosen as "overall leader" in the relevance treatment than in the non-relevance treatment. (2) Relevance is the only treatment in which hyperactive white subjects virtually disappear. (3) The difference between the more active black and the less

Table 7. Frequency and Proportion of all Acts Scored as "Socio-emotional"

Condition	Total Number S-E Acts	Percentage of All Acts That Are S-E
Treatment A	1283	37.2
Treatment	1565	43.1
Treatment	1080	37.5

active white is significantly greater than Treatment A; whereas the non-relevance condition does not differ significantly in this respect from Treatment A.

Two features of the data require special interpretation. One is the superiority of Treatment B with respect to rank order analysis of initiation rate. The other is the decrease in the mean percent of acts initiated by the less active black in Treatment C compared to Treatment B.

These two features of the data are inter-related. A special check was made of the training videotapes of less active blacks in Treatment C. The trainer recalled that they were much slower to learn the radio task than their black partners. It is possible that when relevance was spelled out, an invidious comparison (they could hear each other during training) became pertinent to the less active black's expectations for his own performance. In future interventions it would probably be wise to avoid invidious comparisons during training. A decrease in mean percent of acts initiated by less active blacks in Treatment C once more made it highly probable that they would hold the bottom rank. In the non-relevance treatment, where they show the greater percent of acts contributed, the dramatic increase in the number of very quiet whites helps insure an equal probability that blacks and whites will hold the bottom rank.

Interpreting the decrease in activity of the less active black in the relevance condition is a difficult task. This relatively small effect might well disappear in a replication. However small, if it proves reliable, it means that the treatment is successful in many groups for only one of the two black members; we will need to eliminate this pattern.

Effects in the non-relevance condition would probably have been stronger if whites had been assigned a low rather than a zero value on the general performance characteristic, as Freese and Cohen (forthcoming) do in their most recent experiment. On the other hand, if whites had been told they were definitely incompetent compared to blacks on the general performance characteristic, we would probably have run into markedly hostile responses to the black teachers. The design avoided such a possibility.

IMPLICATIONS

This experiment has theoretical and practical implications for a society anxious to produce "equal status" situations with different racial, ethnic, age, or social class participants. Developing a general method of preventing the spread of expectations from a diffuse status characteristic is the key to future work in applied research.

Theoretical Implications

1. Theoretically speaking, results show that the effect of the diffuse status characteristic may be modified by assigning low status members a high rating on competence on a general performance characteristic. Though we wished not to risk hostility from whites and therefore did not assign them a low value on the general performance characteristic, effects of race were nonetheless greatly weakened on a new task.

2. If relevance between the general performance characteristic and the criterion task is spelled out to low status members, the status effects on interaction in the criterion task will be further weakened. Some weakening occurs in any event.

3. The assignment of competence to the low status member must be realistic and convincing. It may require, as in our case, the use of powerful techniques; the black subject's competent behavior was recorded with videotape and played back to blacks and whites.

4. Finally, most important is the transfer of effects across tasks as dissimilar as the radio and game tasks even when relevance was not made explicit. We treated expectations for competence on the radio task, but were able to change the typical game task behavior pattern even when the game was introduced as a new and different experience. Previous laboratory studies in status characteristic theory have used "tests" of various abilities for both the manipulation and criterion experiences. They have thus been open to criticism that transfer took place because the tasks were similar.

Results of the Freese and Cohen (forthcoming) experiment using age and our experiment using race suggest the general nature of the method of altering status effects.

This study does not deal with some peculiar behavioral characteristics of black people which must be treated. On the contrary, it highlights a general phenomenon of interaction in mixed status groups. The problem arises from the expectations of both low and high status members. *It is not a deficiency of the low status member* as so much literature on race and education argues. The effect of expectation training in this study was seen as much in the lowered task initiation rate of the whites as in the heightened initiation rate of the relatively active black.

Practical Applications

What does this experiment imply for applied research in race and education? The most impressive and important finding is the need to treat both black and white expectations to attain "equal status interaction" in the integrated group. The failure of Treatment A to modify the white dominance pattern is most instructive, because it used the most often recommended method of increasing the self-confidence of people low in self-esteem. Treatment A used two major success experiences and much reinforcement from older, high status role models. The experience is analogous to recommending compensatory education for minority groups who do poorly in school. Low self-esteem will supposedly yield to a treatment of success, confidence-building and reinforcement. A deficient self-concept is an individual trait. In this study, the problem does not reside in the individual. White dominance is seen as a social product of the evaluation of self and other transmitted through cultural beliefs about the competence of blacks and whites, often reinforced in social interaction. From this diagnosis, we formed the hypothesis that treating expectations of the low status individual only, will not be effective. One must also treat the expectations for his performance held by the high status member. The experiment's results clearly supported this conceptualization.

In many social situations the mixed status membership of the work group impedes full member participation. For example, Hall found that male student teachers tended to dominate the discussion in mixed-sex student teacher teams (1972). Deal found that parents dominated the discussion of

what type of drug education program would appeal to junior high school students and ignored what the students themselves had to say (1971). These are not situations where some group members have specific task knowledge; in these tasks people have no specific relevant advantage over one another, and for them to participate on the basis of status interferes with satisfactory task completion.

The social situation most in need of treatment principles is the integrated school. We can see major difficulties in directly applying this treatment to that situation. Clearly, if the task involves specific academic skills and the minority group has a lower skill level, whites will dominate task performance. Second, small interdependent work groups are rarely used in the public schools today. Third, the public school social structure uses competitive rewards; we have used cooperative criterion situations.

Analysis of the goals of proponents of school-integration reveals at least two separate aims. One is the improved academic performance of black students. The other is the need for social integration in a society split along racial lines. Social planners often think that bringing whites and blacks together in the same classroom will produce interracial understanding and the ability to work together. Clearly our entire line of work implies that this is an unlikely outcome.

Most exciting is the possibility of developing centers independent of the schools, where black and white students may be put through a series of problem solving sessions. The goal of these centers would be to teach students how to work with people of a different race. Expectation training would take place before members of different races were allowed to interact with and evaluate each other. In the overall design, expectation training would figure as a necessary though probably insufficient condition for long-term harmonious relations. Working over time in cooperative small group situations no doubt requires other kinds of learning than that which treats the problems caused by initial status differences.

The most practical short-term application is to adult mixed-status vocational teams that will remain together in the training and

work situation. A simple treatment at the start of training might well help high status members to listen more carefully and low status members to play a more assertive role.

Social scientists have generally thought that if people of different races could interact, stereotypical belief systems would yield to greater realism in people's judgments of each other. Our position is that belief systems concerning race and other status characteristics are so powerful that they will infect the interaction setting and will likely reinforce rather than change stereotypical beliefs. The experiment reported here *directly intervenes* in the social interaction; we designed and presented a situation of superior black competence rarely experienced by whites in American society. Relying on the proposition that behavior and attitudes can be greatly affected by being placed in a context containing new and different behaviors, we manipulate the behavior in social interaction to alter the underlying belief structure. How long the experimental effect will last or how many different situations the effects will transfer to are questions for future research.

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RACE, CLASS, AND CONSCIOUSNESS *

CHARLES E. HURST

The College of Wooster

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Past studies reveal weakness in their analyses of class and race consciousness. They fail, for example, to separate out the effects of race and class on components of race and class consciousness. This study discusses the roles of class and race in developing class and race consciousness and interest in politics. It emphasizes the need to clarify the roles of race and class as well as those of age and size of birthplace in each type of consciousness. The main findings show that race and class are sometimes differently related to these dimensions, and suggest that the colony model of American society for which race is decisive is more complete than the class model of race relations, although both are similar.

The Issues at Hand

THIS study relates the independent variables "race," "class," "size of birthplace" and "age" to class and race consciousness, and interest in politics. Some authors have argued that race has a significant and independent effect on these variables (Blauner, 1969; Pinkney, 1969; Broom and Glenn, 1965; Memmi, 1965, 1968; Boggs, 1970); while others inextricably link the concepts of race and class (Nisbet, 1968; Banfield, 1968; Berger in Rainwater, 1970). Still others would discard the concept of social class (e.g., Brodie and Suchman, 1968). What one attributes to social class, others would attribute to race (Boggs, 1970; Banfield, 1968:68-76).

There has been no systematic attempt to analyze the independent effects of class and race (Segal and Schaffner, 1968; Marston, 1968; Billingsley, 1971), partly because the vast majority of blacks occupy positions in the working and lower classes (Pinkney, 1969:71-90; Anderson, 1971:83-102, 260-88). But just as the caste and colony models of race relations differ from the class model (see Berreman, 1960; Cox, 1942, 1948; Blauner, 1969; Metzger, 1971), race and

class are separate phenomena. If the colony model is more adequate than the Marxist in its concept of race relations, then most blacks, regardless of class, apprehend their underprivileged position in society. However, the fact that such spokesmen for the colony position as Fanon (1968) and Memmi (1968) acknowledge a debt to Marx suggests that the class and colony models overlap.¹

The problems of isolating race, class, and related structural models are similar to those in understanding the relationship between the social-psychological phenomena of class and race consciousness. In the latter case, few studies (Leggett, 1968) have treated the problems of specifying the relationships between these social-psychological variables, let alone unravelled the independent effects of class and race on class and race consciousness.

While working-class consciousness has been studied and discussed extensively (see, for example, Leggett, 1968; Morris and Murphy, 1966; Centers, 1949; Hodge and Treiman, 1968; Glantz, 1958; Landecker, 1963), systematic studies of the dimensions of race consciousness have been sparse. Furthermore, the most extensive studies of working-class consciousness have tended to concentrate on whites. Finally, studies of

* I am indebted to Rosalio Wences for critical reading of an earlier version of this paper. The four-sample surveys reported in part here are components of a larger three-phase study of five cities, supported sequentially by the Office of Education (HEW), the University of Connecticut Research Foundation, and the Connecticut Research Commission. The three studies were designed and directed by Irving L. Allen, J. David Colfax, and Henry J. Stetler at the University of Connecticut.

¹ The basic similarities between the two models include the notions of exploitation of a lower group and justifications for the existing power relationships. Also central in both are the importance of rising consciousness, increasing polarization and the volatility of relationships in the system. The basic difference, of course, involves the amount of stress placed on race and property in each model.

class consciousness have tended to examine one level of the class system rather than several levels at once. The possibility that the different aspects of working-class consciousness may vary by class is little considered.

The concept of class consciousness in several studies, especially those on working-class consciousness, includes the notions that class conscious persons (1) tend to perceive differences in their life chances largely in group rather than individual terms, and to see their positions as structurally rather than personally determined. They also (2) view their class as hostile to and politically controlled by the higher classes. These separate dimensions will be used here as the measure of working-class consciousness.

Race consciousness is defined as identification with and allegiance to one's race, which is seen as conflicting with the dominant or, in this case, white race. This definition implies black togetherness or an anti-assimilation attitude. It also implies conflict with the white race or militancy, and belief in discrimination. Conventional militancy, the desire to assimilate, and belief in discrimination will be dealt with separately.

Finally, while working-class consciousness may be one type of class consciousness, individuals in different classes are conscious of different things (Runciman, 1966). In general, those who adopt the middle-class life style are more concerned about social prestige and style than the classes below them. They are also more likely to support the dominant political system and participate in it, rather than advocate its destruction. Having an interest in politics may suggest a greater concern with prestige than with economics. Interest in politics will be incorporated here as a final dependent variable.

Structural Correlates of Consciousness

Studies on class consciousness, including those of Marx and Trotsky, have found that race and working conditions appear related to the development of working-class consciousness (Leggett, 1968; Manis and Meltzer, 1954; Murphy and Morris, 1961). Investigations have also related a higher class position to interest and participation in politics (see Milbrath, 1965). This relationship is often attributed to the individual's

greater education, confidence, stake in politics, exposure to political stimuli and interaction with politically-relevant persons in the middle and upper classes. Consequently, we would expect work status to be differently related to interest in politics than it is to working-class consciousness. Work status is also likely to be related to whether blacks complain about discrimination. Zeitlin notes that for unemployed and non-unionized blacks "the fact of *being Negroes* is the significant aspect of their lives, to which they probably attribute their situation as unemployed workers" (1967:84). Similarly, conventionally we expect more anti-establishment reactions among lower-ranked groups; while those in higher occupational groupings are more likely to work within and desire to be part of the system, even the militant ones. The militancy of this group is likely to blend in with their participation habits.

In addition to race and occupational status, several investigators have stressed the fact that being young may be related to radical concerns and militancy (McCabe, 1967; Keniston, 1968; Weiss and Riesman, 1966:606). Similarly, available evidence indicates that it is the relatively young blacks who are the most concerned, active, proud, and unconventionally militant in causes for advancing their race (Grimshaw, 1969:228; Broom and Glenn, 1965:102; Pinkney, 1969:200; Marx, 1967).²

Some authors have argued that the older a person, the more he tends to withdraw from social life (Cumming and Henry, 1961; Gergen and Back, 1966). In contrast, other studies have found that participation in political life rises with age until about sixty, when it begins to decline (Campbell *et al.*, 1960; Lipset, 1960). However, recent characterizations of youth and the appearance of several political movements among "concerned" youth suggest a change in the relationship between age and political interest.

Some have also postulated that agrarian workers who move to a city are, for many

² Powdermaker contends that older blacks are more likely to share a belief in their inadequacy and inferiority, and have learned to be dependent on whites (see Simpson and Yinger, 1965:173). Consequently, one would expect higher assimilation desires and lower discrimination perception among them.

reasons, under stress that makes them conscious of their position vis-a-vis their employers (Leggett, 1968:11-12). Zeitlin (1967) notes that peasants who enter the urban working class are easily radicalized. Similarly, the black and the rural white city migrant have in common a lack of money and skills, and agrarian experiences of discrimination and exploitation. Pinkney (1969:194) suggests that such blacks are aware of or accustomed to white intransigence, and less likely to trust conventionally militant ("civil rights") techniques for change. Largely lower-class, these blacks are not likely to enter the middle-class civil rights movement both because their class lacks organized activity, and because their relations with middle-class blacks are apt to be strained. Such blacks, as outsiders, are likely to "stick together."

Finally, several studies have found positive relationships between such factors as education, income, sophistication, cosmopolitanism, the size of one's area, and political interest and participation (see Milbrath, 1965). Generally, those living in smaller areas tend to rank lower on each of the first four factors mentioned above (McPhee and Glaser, 1962).

We will test several hypotheses based on the foregoing analysis. To conserve space, in four of these, we relate an independent variable to several dependent variables.

1. Occupational status is negatively related to working-class consciousness and belief in discrimination and positively related to interest in politics, conventional militancy, and assimilation desires.
2. Blacks tend to score higher than whites on working-class consciousness and lower on interest in politics.
3. Age is negatively related to working-class consciousness, interest in politics, and perceived discrimination, and positively related to assimilation desires.
4. Age and conventional militancy are little related.
5. Size of birthplace is negatively related to working-class consciousness and belief in discrimination, and positively related to interest in politics, conventional militancy, and assimilation desires.

Methodology

The hypotheses proposed will be tested against data from the four-city survey collected by the University of Connecticut

"Community Structure Project" in 1966-67. The four cities chosen (Bridgeport, Hartford, New Haven, and Waterbury) were thought to roughly represent a variety of middle-sized U.S. cities, especially in the North and Midwest. Between 10-25% of the central cities population was black in 1966 (Allen and Colfax, 1968). Differences between the cities will not be discussed.

Probability samples were drawn from the adult populations in each city. These were two-stage area samples of households from which one respondent at least twenty years of age or married was randomly selected. The smallest of the cities, Waterbury, had the smallest proportion of blacks. To inflate the sample, a supplementary sample of 108 blacks was drawn at random, to make inter-racial comparisons possible.³ No separate comparison of blacks with whites in Waterbury will be made here. A total sample size of 2,310 was obtained, 1,870 of whom were white and 434 black.⁴

A fifty-page precoded and field-coded interview schedule was used to study the attitudes and behavior of the sample respondents. In addition, a supplementary interview schedule was administered to all black respondents in the four regular samples and the Waterbury supplementary sample. Before being given, the schedules had been pre-tested on twenty-five "typical" respondents, and then revised extensively. The interviewers, "predominantly mature-appearing women," trained beforehand, carried out the interviews during those months in 1966 which were "relatively uneventful in these cities in the areas of intergroup relations, schools, and other community problem areas that were the focuses of these surveys" (Allen and Colfax, 1968:10). The completion rate, high for this type of survey, ranged from 85-89%.

The scales measuring feelings of economic subjugation and political manipulation, interest in politics, conventional militancy,

³ Since the present study is concerned with blacks in general, and since the black samples in each city will be grouped together, this supplementary sample was included in the total black sample even though its size does not represent the percent of blacks in Waterbury.

⁴ The six orientals in the sample were included in the analyses only when the total sample was being analyzed.

Table 1. The Influence of Occupational Status on Selected Dependent Variables

	Occupational Status					
	Total Sample		Blacks		Whites	
	Low	High	Low	High	Low	High
Political Manipulation						
Low	33%	36%	25%	26%	37%	38%
Medium	32	33	33	38	32	32
High	35	31	42	36	31	30
N	100% (654)	100% (1013)	100% (215)	100% (109)	100% (439)	100% (903)
Chi-square	3.81		1.43		0.24	
Gamma	-0.08		-0.08		-0.02	
Economic Subjugation						
Low	12%	23%	4%	7%	16%	24%
Medium	43	54	35	46	47	55
High	45	23	61	47	37	21
N	100% (809)	100% (1197)	100% (281)	100% (122)	100% (527)	100% (1071)
Chi-square ^a	114.39	(.001)	6.75	(.05)	50.55	(.001)
Gamma	-0.40		-0.26		-0.30	
Interest in Politics						
Low	27%	11%	27%	15%	28%	11%
Medium	58	64	56	61	59	64
High	14	25	17	24	13	25
N	100% (911)	100% (1318)	100% (299)	100% (128)	100% (612)	100% (1186)
Chi-square ^a	106.24	(.001)	9.06	(.02)	92.07	(.001)
Gamma	0.38		0.28		0.41	

^aThe levels of significance for the significant relationships are given in parentheses after the chi-square value.

assimilation desires, and belief in discrimination, were devised through correlational matrix analysis. Essentially, we examined several items suggested by past studies and theories relating to them and selected those most highly inter-correlated for use in the scales.⁵

⁵ Since most of the questions were long, only a brief but exact description of the items is included here. The scale on feelings of political manipulation was made up of items asking respondents if they thought that most big decisions are made without the public's knowledge and if those in power are more concerned about themselves than the public. "Economic subjugation" was measured by using items on the perception of one's family income level; the perception of the most important cause of poverty being structural (no opportunity, wealth not spread, unemployment, etc.) or personal (laziness, stupidity, ineptitude, luck, etc.); and the extent of the role that government should play in helping the poor. The scale on interest in politics was made up of questions treating the extent to which the respondent discusses public issues (taxes, foreign affairs, public schools, etc.) and mass media content, and the extent of his interest in

Findings

Tables 1 and 2 contain the relationships stated in hypothesis 1. Occupational status relates significantly to economic subjugation and interest in politics, but contrary to expectation no significant negative relationship

current events and elections. "Conventional militancy" was measured by items on willingness to picket in front of a store that discriminated, and on participation in civil rights activities. "Assimilation desires" were revealed by questions dealing with the desire to live in a neighborhood having a certain percent of blacks (33% or less; 34-50%; 51%-all black), and the willingness to buy a house in a white neighborhood. Finally, "perceived discrimination" was measured by asking if respondents thought that landlords make blacks pay more rent because they are black. Unfortunately, questions dealing with discrimination in areas other than rent, and additional items on assimilation desires were not available. The mean and median Pearson correlation for the scale items was .28.

In running the correlations, we dropped the "don't know" and "no answer" categories from the analyses, since they do not fit into the ordinal char-

exists between occupational status and political manipulation. The latter finding seems to support Shostak's contention that workers favor the existing government and are not class conscious in any political sense. However, a greater percentage of blacks in low-status occupations feel politically manipulated than blacks in higher status occupations. Furthermore, a higher percentage of blacks than whites feel politically manipulated, while a higher percent of whites than blacks feel unmanipulated politically regardless of occupational status. This suggests again that race and not occupational status may be more important in explaining feelings of political manipulation. Form and Rytina's recent study (1969) found blacks more likely than whites to believe that the U.S. political structure is dominated by the economically powerful. But to recapitulate, the evidence here does not support the general idea that lower-class persons feel more manipulated politically than those in higher classes.

As noted, a significant negative relationship exists between occupational status and feelings of economic subjugation, though it is slightly stronger among whites than among blacks. In the total sample, 45% of those in the low compared to only 23% of those in the high occupational category feel strongly subjugated economically. As Hodges, Jr. suggests (1964:195-220), blacks especially might be expected to feel this way and be less complacent about it than lower-class whites. Indeed, as the high percentages for blacks in Table 1's high category indicate, "race" has a reinforcing effect on them.

As predicted, the last relationship in Table 1 between occupational status and interest in politics is significant and positive for both blacks and whites, though stronger for

acter of the alternatives unless an assumption is made about the respondents' likely responses. Consequently, the N's for the tables vary. In each scale item, the alternatives were arranged ordinally. Scores were then tabulated for each scale, which were later collapsed into ordinal categories for tabular analysis.

With regard to the independent variables, occupational status as our measure of class position, was measured using the NORC scores for occupational prestige. Collapsed categories were selected by "natural breaks" in the data, and by there being enough numbers in each category to analyze.

Table 2. The Influence of Occupational Status on Selected Dependent Variables among Blacks

	Occupational Status	
	Blacks	
	Low	High
Conventional Militancy		
Low	19%	16%
Medium	72	65
High	9	19
N	100% (291)	100% (119)
Chi-square ^a	9.48	(.01)
Gamma	0.24	
Assimilation Desires		
Low	27%	21%
Medium	51	58
High	22	21
N	100% (260)	100% (107)
Chi-square	1.89	
Gamma	0.08	
Belief in Discrimination		
Low	39%	30%
High	61	70
N	100% (272)	100% (113)
Chi-square	2.72	
Gamma	0.20	

^aThe levels of significance for the significant relationships are given in parentheses after the chi-square values.

whites. This result supports the findings of many past studies (see Milbrath, 1965).⁶

Table 2 presents the relationships for blacks between occupational status and conventional militancy, assimilation desires, and

⁶Since "level of education of head of household" has often been a good predictor of class-related attitudes among blacks, we used it as an alternative to occupational status as an independent variable. We found that blacks with some high school or less were significantly more likely to feel politically manipulated than those with at least a high school education ($\chi^2 = 13.503(.01)$; gamma = $-.312$). This is as might have been expected, but a similar relationship involving occupational status, as has been noted, did not materialize. Significant relationships were also found among blacks between education and economic subjugation ($\chi^2 = 10.857$; gamma = $-.312$) and interest in politics ($\chi^2 = 18.815$; gamma = $.39$). Similar significant relationships among these variables were also found for whites, though the relationship involving political manipulation was much stronger for blacks.

belief in discrimination.⁷ As predicted, a significant positive relationship obtains between occupational status and conventional militancy. As past studies suggest (Killian, 1968), middle class blacks are associated with the civil rights movement. Studies further show that low ascribed status combined with high achieved status encourages a political rather than personal reaction to status inconsistency problems (Lenski, 1956; Jackson, 1962; Kramer, 1970). But overall the percent of blacks in Table's 2's high militancy category is small. The second relationship between occupational status and assimilation desires confirms findings that most blacks do not strongly oppose assimilation. Whatever their occupational status, the majority have moderate assimilation desires. Finally, whatever their occupational status, blacks tend to believe that they pay more rent because they are black. The lack of a significant relationship here suggests that even blacks who "make it" occupationally, believe that full assimilation is difficult.

In each case where a transracial comparison was possible, race and the dependent variable were significantly related, with blacks feeling more manipulated politically and subjugated economically, and less interested in politics. These relationships support hypothesis 2 and are shown in Table 3. The strength of the relationships for the first two sets of variables supports those who have found that blacks are more working-class conscious than whites, and that being black may be the most important determinant of reactions and attitudes (Leggett, 1968; Zeitlin, 1967).

However, this table should be used cautiously. As Table 1 indicated, social class (occupational status) continues to influence feelings of economic subjugation when race is controlled. Furthermore, when occupational status is controlled (Table 4), the relationship between race and interest in politics shown in Table 3 disappears. That is, of the two variables, occupational status is more important than race in accounting for interest in politics. In fact, Table 4 shows that a slightly greater percent of low occu-

Table 3. The Influence of Race on Selected Dependent Variables

	Race	
	Whites ^a	Blacks
Political Manipulation		
Low	37%	25%
Medium	32	35
High	31	40
N	100% (1355)	100% (325)
Chi-square ^b	18.21	(.001)
Gamma	0.21	
Tau-C ^c	0.09	
Economic Subjugation		
Low	22%	5%
Medium	52	38
High	26	57
N	100% (1619)	100% (404)
Chi-square ^b	161.99	(.001)
Gamma	0.58	
Tau-C ^c	0.24	
Interest in Politics		
Low	17%	24%
Medium	62	57
High	21	19
N	100% (1823)	100% (428)
Chi-square ^b	11.06	(.01)
Gamma	-0.12	
Tau-C ^c	-0.04	

^aThe 6 orientals in the sample are included with the whites.

^bThe levels of significance for the significant relationships are given in parentheses after the chi-square values.

^cTau-C values are given in addition to Gamma because "race" is most often viewed as a categorical or nominal variable.

pational blacks than whites are high in political interest.

Table 4 also clarifies the relationship between race and political manipulation and economic subjugation. Specifically, the relationship is strongest for those whose occupational status is low. Race is more important in explaining political manipulation "consciousness," and occupational status helps specify that relationship. Race is also significantly related to economic subjugation when occupational status is controlled.

Table 5 presents findings on relationships discussed in hypothesis 3 between age and political manipulation, economic subjugation,

⁷ Whites were not asked the questionnaire items pertaining to conventional militancy, assimilation desires, and perceived discrimination.

Table 4. The Influence of Race on Selected Dependent Variables, with Occupational Status Controlled

	Occupational Status			
	Low		High	
	Whites ^a	Blacks	Whites ^a	Blacks
Political Manipulation				
Low	37%	25%	38%	26%
Medium	32	33	32	38
High	31	42	30	36
N	100% (439)	100% (215)	100% (904)	100% (109)
Chi-square ^b	11.09	(.01)	5.98	
Gamma	0.22		0.18	
Tau-C	0.13		0.05	
Economic Subjugation				
Low	16%	4%	24%	7%
Medium	47	35	55	46
High	37	61	21	47
N	100% (528)	100% (281)	100% (1775)	100% (122)
Chi-square ^b	54.29	(.001)	51.66	(.001)
Gamma	0.47		0.55	
Tau-C	0.26		0.13	
Interest in Politics				
Low	27%	27%	11%	15%
Medium	60	56	64	61
High	13	17	25	24
N	100% (612)	100% (299)	100% (1190)	100% (128)
Chi-square	2.33		1.45	
Gamma	0.05		-0.06	
Tau-C	0.02		-0.01	

^aThe 6 orientals in the study are included in with the whites in this table.

^bThe levels of significance for the significant relationships are given in parentheses after the chi-square values.

tion, and interest in politics. This table shows no significant relationship between age and feelings of political manipulation. The most interesting finding for this relationship is the higher percent of middle-age blacks in the high political manipulation category. Relationships between age and feelings of economic subjugation are also not significant. Age and interest in politics are negatively related as hypothesized, but the relationship is low and significant for whites only. The statistics, which should be carefully interpreted, partly support Cumming and Henry (1961), and Gergen and Back (1966) who suggest that older whites tend to disengage from others in society.

Table 6 shows the relationships found between age and conventional militancy, assimilation desires, and belief in discrimination. As hypothesis 4 predicted, no relationship obtained between age and conventional militancy; nor was one found between age and assimilation desires. This finding sug-

gests that traditional ways of conceiving the correlates of our dependent variables may be inadequate.⁸ Mainly, the relationships between age and conventional militancy and assimilation desires, show the percentages in the latter two variables to be quite similar across the three age categories. This similarity is especially true for the relationship between age and assimilation desires. Less important is the fact that most blacks rank in the middle in each of these dependent variables, regardless of age. The last relationship in Table 6 is significant and indi-

⁸ Much of what might be said about the relationship between age and conventional militancy and assimilation desires, and the other variables, may pertain only to whites. In fact, we may be forced to re-examine our assumptions that blacks and whites are similar in values and influences. In the past, we assumed too often that the U.S. is a melting pot. And though we now recognize that it is not, our new understanding may not have carried over into sociological research and theories on blacks and whites in America.

Table 5. The Influence of Age on Selected Dependent Variables

	Age								
	Total Sample			Blacks			Whites		
	29 or Less	30-44	45+	29 or Less	30-44	45+	29 or Less	30-44	45+
Political Manipulation									
Low	36%	30%	37%	30%	18%	29%	38%	34%	38%
Medium	32	34	32	34	37	34	32	35	32
High	32	36	31	36	45	37	30	35	30
Total	100%	100%	100%	100%	100%	100%	100%	100%	100%
N	(388)	(500)	(786)	(107)	(132)	(83)	(281)	(367)	(703)
Chi-square		7.38			5.49			1.82	
Gamma		-0.04			0.03			-0.02	
Economic Subjugation									
Low	15%	19%	19%	6%	4%	4%	19%	24%	21%
Medium	50	49	50	36	40	38	55	52	52
High	35	32	31	58	56	58	26	24	27
Total	100%	100%	100%	100%	100%	100%	100%	100%	100%
N	(490)	(579)	(944)	(134)	(158)	(109)	(354)	(419)	(834)
Chi-square		5.63			1.13			4.25	
Gamma		-0.07			0.00			0.00	
Interest in Politics									
Low	18%	16%	19%	26%	21%	26%	15%	14%	19%
Medium	59	61	63	56	59	56	60	61	64
High	23	23	18	18	20	18	25	25	17
Total	100%	100%	100%	100%	100%	100%	100%	100%	100%
N	(500)	(635)	(1105)	(137)	(171)	(117)	(362)	(462)	(987)
Chi-square ^a		12.64	(.01)		1.82			16.309	(.01)
Gamma		-0.09			-0.01			-0.14	

^aThe levels of significance for the significant relationships are given in parentheses after the chi-square value.

cates that the middle-aged blacks are most likely to claim they pay more rent because they are black. But perhaps this group has had to pay more rent than the younger group, and would more quickly believe in rent discrimination.

As hypothesis 5 predicted, Table 7 shows a significant negative relationship between size of birthplace and feelings of political manipulation when the entire sample is analyzed. With race controlled for, however, no significant relationship exists between size of birthplace and political manipulation. The strong relationship for the total sample appears to result from a combination of the blacks strengthening the percentages in the "under 10,000" category and the whites strengthening and shaping the percentages in the "10,000+" category. If either is removed, the relationship ceases to be significant. A greater percent of blacks than whites

feel strongly manipulated politically, regardless of their size of birthplace. Table 7 also indicates for the total sample the expected significant negative relationship between size of birthplace and feelings of economic subjugation. When race is controlled, however, we find that blacks tend to think of themselves in these terms regardless of their birthplace; whereas for whites the size of birthplace does make a difference. The findings for blacks are what is important here, since almost 60% have strong feelings of economic subjugation.

Finally, Table 7 shows a significant positive relationship among whites between size of birthplace and interest in politics. This finding supports the rationale and findings discussed earlier. But the relationship does not hold for blacks, even though there is a slightly greater percent of blacks from larger birthplaces in the higher political interest

Table 6. The Influence of Age on Selected Dependent Variables among Blacks

	Age		
	29 or Less	30-44	45+
Conventional Militancy			
Low	19%	14%	24%
Medium	70	76	63
High	11	10	13
N	100% (133)	100% (163)	100% (112)
Chi-square		5.74	
Gamma		-0.03	
Assimilation Desires			
Low	22%	27%	26%
Medium	53	51	56
High	25	22	18
N	100% (120)	100% (147)	100% (98)
Chi-square		1.97	
Gamma		-0.08	
Belief in Discrimination			
Low	47%	29%	36%
High	53	71	64
N	100% (120)	100% (159)	100% (103)
Chi-square ^a		10.19 (.01)	
Gamma		0.17	

^aThe levels of significance for the significant relationships are given in parentheses after the chi-square value.

group. From the significant findings, however, one can infer that the relationships exist because urban residence may mean higher status. This latter factor and its concomitants may explain the interest in political matters, since political sophistication has been found to relate to higher levels of class (Milbrath, 1965:65, 120-8). This suggestion is tested in Table 8 which reports the relationship between size of birthplace and interest in politics with occupational status controlled. For those whose occupational status is high, the relationship remains about the same; while, for those with low occupational status the significant relationship disappears. In other words, occupational status helps specify the conditions under which the relationship between size of birthplace and interest in politics holds.

Finally, Table 9 presents the relation of size of birthplace to conventional militancy, assimilation desires, and belief in discrimination, presented in hypothesis 5. The findings here shed light on those in Table 7, which show that among blacks the relationship nearest significance is that between size of birthplace and interest in politics. As expected, in Table 9, a significant positive re-

lationship exists between size of birthplace and conventional militancy. In fact, of the seventy-one blacks who were low in conventional militancy, 70% came from places under 10,000 in size. This finding supports ideas that associate the larger city with higher levels of class and political interest. The relationship in Table 9 between size of birthplace and assimilation desires is not significant, lending support to those earlier mentioned findings that only a small percent of blacks totally oppose assimilation. The majority, however, are "medium" rather than high in their assimilation desires, a finding which indicates that support for assimilation is qualified. Finally, Table 9 shows no relationship between size of birthplace and belief in discrimination. Blacks, regardless of their size of birthplace, believe that they pay more rent because they are black. Race again appears more important than background.

In summary, the major findings indicate that while race is important for one's feelings of being manipulated politically, both race and occupational status play a part in one's feeling subjugated economically. Blacks generally score higher than whites in each

Table 7. The Influence of Size of Birthplace on Selected Dependent Variables

	Size of Birthplace					
	Total Sample		Blacks		Whites	
	Under 10,000	10,000+	Under 10,000	10,000+	Under 10,000	10,000+
Political Manipulation						
Low	28%	37%	20%	28%	32%	39%
Medium	35	32	40	31	33	31
High	37	31	40	41	35	30
N	100% (442)	100% (1150)	100% (164)	100% (151)	100% (278)	100% (999)
Chi-square ^a	13.59 (.01)		4.43		4.82	
Gamma	-0.15		-0.06		-0.12	
Economic Subjugation						
Low	13%	20%	6%	3%	18%	23%
Medium	45	51	37	40	50	53
High	42	29	57	57	32	24
N	100% (564)	100% (1331)	100% (216)	100% (178)	100% (348)	100% (1152)
Chi-square ^a	32.39 (.001)		1.23		7.97 (.02)	
Gamma	-0.24		0.01		-0.14	
Interest in Politics						
Low	23%	14%	27%	20%	21%	14%
Medium	61	62	57	57	64	63
High	15	24	16	23	15	23
N	100% (610)	100% (1479)	100% (227)	100% (190)	100% (383)	100% (1288)
Chi-square ^a	33.30 (.001)		4.92		21.22 (.001)	
Gamma	0.25		0.19		0.25	

^aThe levels of significance for the significant relationships are given in parentheses after the chi-square value.

of these scales regardless of their occupational status, size of birthplace or age. On interest in politics, however, blacks score lower than whites; but this relationship disappears when occupational status is con-

trolled. Consequently, occupational status rather than race appears to be the important factor for interest in politics. Finally, among blacks those high in occupational status and from a larger birthplace are more conven-

Table 8. The Influence of Size of Birthplace on Interest in Politics with Occupational Status Controlled

	Occupational Status			
	Low		High	
	Size of Birthplace		Size of Birthplace	
	Under 10,000	10,000+	Under 10,000	10,000+
Interest in Politics				
Low	31%	24%	16%	9%
Medium	55	61	67	63
High	14	15	17	28
N	100% (322)	100% (519)	100% (282)	100% (944)
Chi-square ^a	4.97		19.06 (.001)	
Gamma	0.12		0.28	

^aThe level of significance is given in parentheses after the chi-square value.

Table 9. The Influence of Size of Birthplace on Selected Dependent Variables among Blacks

	Size of Birthplace	
	Blacks	
	Under 10,000	10,000+
Conventional Militancy		
Low	22%	12%
Medium	66	77
High	12	11
N	100% (223)	100% (178)
Chi-square ^a	8.52	(.02)
Gamma	0.18	
Assimilation Desires		
Low	26%	24%
Medium	51	54
High	23	22
N	100% (200)	100% (160)
Chi-square	0.47	
Gamma	0.03	
Belief in Discrimination		
Low	39%	35%
High	61	65
N	100% (208)	100% (168)
Chi-square	0.58	
Gamma	0.08	

^aThe levels of significance for the significant relationships are given in parentheses after the chi-square value.

tionally militant. Regardless of background, blacks tend to have at least "medium" assimilation desires, and regardless of size of birthplace and occupational status tend to believe in the existence of discrimination against blacks. The middle-aged group is higher in this variable than the others, however.

It is important to keep in mind that the survey data reported here were collected in 1966-7, when the black movement was largely in its civil rights phases. Since the radical tendencies of the movement came later, responses to questions on militancy and related topics would probably be different today.

Relevance of Findings for Class and Colony Models

One of the central conceptual issues guiding this research is whether class is more im-

portant than race in accounting for the attitudes studied; and relatedly, whether the class or colony model is more valid in describing American race relations. Race and class have independent effects on the phenomena studied here, and often affect each other antagonistically. Still the fact that they are often reinforcing for such variables as feelings of economic subjugation leads one to believe that both the class and colony models of American society have degrees of validity, depending on the variables involved. The models overlap in the dimensions they use to analyze society, one of these being the experience of exploitation or privilege. But from the findings that race is important to one's feeling manipulated politically, while occupational status is not, it appears that one's sense of being exploited is more activated by the fact of being black than by being lower-class. In other words, if the experience of exploitation is crucial to an adequate model of American society, the colony model seems to apply more fully to the black's situation than the class model.

The class and colony models are both systemic views of society. In the extreme class model, social institutions largely reflect the economic institution, and the ruling ideas in society come from the top class. Similarly, in the colony model, the institutional ideologies resemble each other in encouraging a colonial relationship, white over and against black. That blacks by and large feel both politically manipulated and economically subjugated, as well as discriminated against, suggests that they see all institutions as intertwined and in the hands of a group whose efforts are against them. But, as with most research, these conclusions are tentative, and their verification must await more refined research.

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Otto N. Larsen
Executive Officer

CULTURE CREATION AND SOCIAL RECONSTRUCTION: THE SOCIO-CULTURAL DYNAMICS OF INTERGROUP CONTACT *

DAVID ROTHSTEIN

Alfred University

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This study considers culture and social structure as, respectively, an epistemic system of graduated abstractness for defining and classifying experience, and the possible and legitimate means for coping with experience. It presents two paradigms which inquire into the nature of cultural and social structural change and stability that result when two groups come into contact. Variations in the qualitative characteristics of contact are seen as producing change or maintaining given socio-cultural systems. The variables considered in the first paradigm are the degrees of cultural similarity and oppression; the second paradigm considers the degrees of social structural compatibility and coercion. Processes of stability and persistence in culture and social structure are traced to different configurations of the characteristics of intergroup contact.

AMONG theories of social change the commonest have been those employing the concepts of developmentalism and/or evolutionism, either as some general historical process or as a sociological process occurring in some specific situation. This is not surprising since the idea that history or societies develop or evolve is prominent and persistent in Western cosmology (Teggart, 1962; Nisbet, 1969); more specifically it is an aspect of sociological organicism. These theories have many critics. Croce (1949; 1966), for example, says that they view history as something closed and determined and risk reifying such ideas as stages of growth and development or classifications of phenomena. To Teggart (1962) the theories assume what they set out to discover. Nisbet (1969; 1970) suggests that they can simply become an abuse of metaphor or the use of an irrelevant metaphor and, after Ryle (1954) it can be suggested that they confuse logical with causal necessity. One remedy for these difficulties is a concentrated study of specific events in a given socio-historical context. This paper offers paradigms for such a study. Before discussing

the paradigms, it would be useful to explore briefly two of the central concepts involved, culture and social structure.

CULTURE AND SOCIAL STRUCTURE

Culture is a system of knowledge which exists on several interrelated levels. On the most abstract level, culture is the ontology and cosmology of a society or group; a system of knowledge which conceptualizes and describes the nature of the universe and the nature of man.¹ In some respects this is similar to what Kluckhohn and Strodtbeck (1961) call "value-orientations." In systematic form these conceptualizations are closely akin to Holzer's concept of an "epistemic community" (1968:69). Langer (1951:6) says that the distinctive thing about any epoch or society is the nature of its questions about the world rather than its answers to them. Every society comprehends a new idea, event or phenomenon, within its own conceptual frame-

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¹ By the terms cosmological and ontological, I mean the philosophical definitions (1) ideas about the nature of the universe and (2) ideas about the nature of being. Moreover, it is not implied that ontology and cosmology exhaust the kinds of idea systems contained within a culture; there are also a metaphysic, an epistemology, an ethic, and an aesthetic, among others. For simplicity, it is assumed that these latter can all be subsumed under cosmology and ontology.

work, i.e. with questions unique to that society's cosmological and ontological understandings. Ontological and cosmological conceptualizations determine, through the questions they allow one to ask, the answers to those questions as they are manifest in norms and values; and they have a determining effect on social action itself. Since these "answers" legitimize social roles, they determine which roles are possible by classifying all experience which is knowable and a range of appropriate action orientations toward that experience within a given society. Values and norms comprise the next level of culture. They represent the goals of social action and the rules for attaining those goals. They derive from ontological and cosmological conceptualizations and are specific to a given culture. Finally both the ontological and normative levels of culture are symbolized concretely on the third level which consists of artifacts or the tangible means to and products of culturally expressive activity.

The importance of symbols to social interaction has occupied an entire school of thought in sociology. While it is clearly beyond the scope of this paper to consider this school in its entirety, several points concerning symbols need to be made. Jaeger and Selznick (1964:653-69) define culture as ". . . everything that is produced by and is capable of, sustaining shared symbolic experience." Culture is the symbolizing of experience so it can be shared with others. They say, "an effective symbol combines economy of statement with richness of expression." Thus cultural symbols tend to be manifested aesthetically; there is a "strain toward the aesthetic." Cooley (1956:80) suggests four characteristics of symbols that facilitate social action: "Expressiveness, or the range of ideas and feelings it is competent to carry; permanence of record, or the overcoming of time; swiftness, or the overcoming of space; diffusion, or access to all classes of men." Mead (1947:87-90, 157, 158) points out that the boundaries of a group are constituted in part by its "universe of discourse," that is, the range and extent of connotative meaning attributed to a given symbol or system of symbols. Add to this spatial dimension the time dimension and one can consider groups and societies

as coherent, historical wholes, since the symbols which define experience and direct social action remain essentially the same over time. This would also enable us to investigate change more pointedly through the discovery that certain symbols became meaningless or took on different connotative meanings, or that new symbols came into existence over time.

If social action is behavior subjectively meaningful to two or more individuals (Weber, 1964:88), then subjective meaning provides the bridge between knowledge and action and finds its basis in ontological and cosmological conceptualizations which reflect the need to make sense of experience over time. Social action, thus considered, illuminates an understanding of the relationship between culture and social organization. Whitehead (1927:74-6) says, "The self-organization of society depends on commonly diffused symbols evoking commonly diffused ideas, and at the same time, indicating commonly understood actions." These "commonly understood actions" are social organization, those roles and institutions which a culture defines as possible and legitimate. Since in any situation social action requires the selection of certain roles, depending on the definition and meaning of an experience, then the sources of meaning would seem paramount in understanding why certain roles are selected over others and why, out of all the possible aspects of an experience, only certain features of it are reacted to. It is at the point where the definition of an experience and the possible reactions to it meet, that the relationship between culture and social organization becomes apparent. This connection is most easily illustrated in Whitehead's distinction between presentational immediacy and causal efficacy each of which are modes of perception (1929:184, 255-79). Presentational immediacy is simply the uncritical perception of sense data in any experience or encounter; it is the immediate data of experience. Causal efficacy, as a mode of perception, is an assessment of what features of an event or experience are causally relevant and causally related. Causal imputation is found within cultural idea systems and is a primary reservoir of those "commonly diffused symbols evoking com-

monly diffused ideas . . . indicating commonly understood actions." (Whitehead, 1927:76). Systematic variations in causal imputation are one of the primary factors which distinguish one group from another.

At the level of concrete social action the mode of causal efficacy, as determined by culture, prescribes for the individual the possible meanings of an experience and an adequate, relevant and legitimate range of values, norms and social roles through which he can relate to it. Furthermore, if within any culture there are finite limits to the attribution of cause, (and this would seem to be the case) then when new experience is encountered, experience within which causation cannot be attributed to any conventional understanding, breakdowns in cultural meaning systems and social structure occur. Holzner calls these occasions "reality shocks" when ". . . reality may collapse, and it must then be reconstructed in a sometimes frantic search for an appropriate new perspective." (1968:12). Thomas (1966) referred to this as social disorganization, and the process of bringing structure back into congruence with experience he called social reconstruction. Jaeger and Selznick (1964: 658) conceptualize the process by which new experience is given shared symbolic meaning as the "culture-creating act." Culture-creation is essentially "the transformation of an impersonal setting into a personal one." The question then is under what circumstances does culture-creation occur? It would seem that one clear situation in which old idea systems and roles may become obsolete is in the contact of two groups. The following paradigms address themselves to such contact situations.

THE PARADIGMS

In his work, particularly in *The Protestant Ethic and the Spirit of Capitalism*, Weber shows some of the complex interrelationships between ideas and systems of social organization. And it seems necessary at the outset to disclaim causal primacy for either culture or social structure in the present work. If forced to make any statements about causal primacy, I would maintain that changes in culture and social structure are ultimately the product of changes in ex-

perience, of events which must be made meaningful, and to which individuals as members of groups must react.

It is in the spirit of the Weberian approach to change that the following paradigms are presented. They do not purport to account for all changes in ideas or social organization, but rather, to illuminate the major processes which are likely to occur when two groups come into contact under differing circumstances. They are ideal types whose utility lies in their heuristic capacity. The first paradigm concerns cultural change and the second social structural change.

There are two variables in the first paradigm, the first is the nature of the social relationships between two groups seen as a continuum of power and dominance at most, and of social distance at least. Whenever two groups come into contact, typically one is in a superior position economically, politically, or militarily, and the other is forced into a subordinate position. The degree of force needed to maintain a satisfactory balance for the superordinate group will range from high to low. This force will be called oppression.

The second variable is cultural similarity, which shall be called coincidence. If one group has experienced a given social reality its culture will reflect and make sense of that reality, and the sets of possible relationships within that system. As long as individuals meet with no unusual or extraordinary experience, the cultural interpretations of experience will suffice to give meaning and order to the world. But what happens when

	Coincidence	
	High	Low
	High	Low
Oppression	1. Parallel similar cultures	4. Culture creation by subordinate group
	2. Acculturation, assimilation	3. Coexisting dissimilar cultures

Paradigm A

Figure 1. Cultural Exchange and Creation

traditional definitions can no longer sustain shared symbolic meaning? When contact between groups forces one group into a subordination that makes no cultural sense to it, its members may look to the superordinate culture for meaning. When the two cultures are highly similar, the superordinate culture can at least explain the experience. When they are highly dissimilar new meanings must be found. In this situation the normative and social alternatives for action are interfered with by the new order of experience. Old categories of meaning cannot make sense of it, and old roles and institutions do not allow the subordinates to interact on an equal basis with the superordinates. Clearly, we are describing general processes. Not all subordinates will respond in the same way. Their different responses probably represent a continuum from collaboration to violent counter action.

The second paradigm concerns social structural change which may occur in contact situations. Here also we have two variables. The first is the degree to which roles and institutions are interchangeable between groups. This is termed structural compatibility and appears in the ease with which the individual can acquire meaningful roles in the superordinate social structure or the possibility for superordinate institutions to function for the subordinate individuals.

The other variable is the nature of inter-

group relationships, which is conceived of as the degree of coercion needed to maintain the interaction patterns imposed by the superordinates. Another way of viewing coercion is to refer to it as the degree of authority or power, as these are defined by Weber, which characterizes the groups' interaction. Now the problem of compatibility itself determines whether the groups will interact within given systems of organization. The factor of coercion introduces complications for both groups. It requires the superordinates to legitimize their dominance and structural imposition. It forces the subordinates first, to use roles and institutions hitherto unknown to them, and second, raises the problem of mitigating the oppression. There is, therefore, a range of dimensions for both cultural and social reality, and we look to their interplay for an explanation of cultural and social structural stability and change.

CULTURAL AND SOCIAL STRUCTURAL STABILITY

In cell one of paradigm A., high oppression and high coincidence, one would expect parallel similar cultures, that the groups would co-exist, each striving to maintain the integrity of its culture. There is a high potential also for subordinate individuals to assimilate into the superordinate group because there is a high degree of similarity in their ideas about the world. But on closer examination one must conclude that this is unlikely to occur for two reasons. First, oppression is high; and to maintain that relationship, the superordinates must prevent such assimilation. The second reason is related to the first. Because the cultures are similar, the superordinates are oppressing their own kind as it were. Therefore any cultural differences will be seized on, emphasized, and made the official reason for the oppression. Further, the similarity might even force the superordinates to greater oppression to maintain their cultural purity and to reaffirm the subordinate's oppression as legitimate and deserved. With little or no assimilation and high oppression, subordinates will possibly come to share superordinate definitions of reality and to regard their own culture as inferior or primitive through much the same means that Lifton describes

	Structural Compatibility	
	High	Low
High (Power)	1. Segregation	4. Emergence of new patterns of social organization in subordinate group
Low (Authority)	2. Merging of roles and institutions toward superordinate group	3. Coexistence, persistence of roles and institutions in each group

Paradigm B

Figure 2. Social Structural Change and Reconstruction

in the process of ideological conversion (Lifton, 1961). As long as superordinates maintain power over the subordinates, high cultural coincidence rather than mitigating oppression, would, on the contrary, tend to increase it.

With regard to social structure, high structural compatibility and high coercion would likely produce segregation of both groups, as cell one of paradigm B indicates, since segregation is one means of keeping assimilation at a minimum. High structural compatibility generates a high potential for assimilation; but since assimilation would erase group distinctions and jeopardize the superior position of the superordinates, through the disappearance of a clear, distinct subordinate group, they could use segregation to preserve both groups and maintain their relative positions.

For subordinates, segregation at least would preserve those roles and institutions that the superordinates did not perceive as threatening. Moreover, high coercion is tantamount to high power, hence low legitimacy, and interaction between the groups is highly strained and quite specific. Therefore, the active acceptance of superordinate roles by subordinates, without a decrease in coercion, will be low. For it to be otherwise would constitute accepting the legitimacy of those roles and hence accepting coercion.

ACCULTURATION, ASSIMILATION

The second case in paradigm A combines high coincidence and low oppression. Here the prediction is that acculturation and assimilation will occur. Since the cultural definitions of reality are highly similar, subordinates can readily adopt the superordinate group's definitions if and when their own interpretations fail. The low oppression and relatively unstrained social relations facilitate this first, by freeing the superordinates from the need to find cultural differences, and second, by enabling the subordinates to make sense out of this new experience within its own categories of meaning. To speak here of a subordinate group is, in fact, misleading. It would perhaps be more accurate to speak of intruding and "host" or native groups. At worst, the native group would view the new, post-contact situation

as a temporary state of affairs. This situation, as has been noted, could be characterized by the relatively unrestricted flow of ideas from one group to the other, usually superordinate to subordinate. It is important to note, however, that this interchange is simplified, not because of high coincidence exclusively, but because of low oppression which first, introduces little or no strain into previously existing systems of social relationships and, therefore, does not call into question traditional definitions of reality and second, because low oppression allows the flow of ideas, modes of expression and products of culturally expressive activity from one group to another to occur.

The second situation in paradigm B combines low coercion with high structural compatibility. Here one would expect a merging of roles and institutions. The high compatibility obviously means that roles are almost interchangeable in the first place. Since low coercion implies high legitimacy, the desirability of superordinate role acquisition by subordinate individuals will be greatly facilitated. One would expect differences between such highly compatible institutions to fall away in time. Note, however, that structural changes will usually be in the direction of the superordinates because while coercion is low, the groups are not completely equal.

COEXISTENCE

The third case in paradigm A combines low oppression with low coincidence. Here both cultures are expected to coexist, possibly in isolation with little or no cultural interchange. Since oppression is low, there is little strained interaction. Hence, the subordinate group is not placed in the position of having its systems of classification opened to question. The subordinate is asked no unanswerable questions, and his place within the "natural" order of things is not threatened. If anything, low oppression and low coincidence would reinforce the subordinate's culture and maintain the subordinate's identity in the face of another interpretation of reality.

In paradigm B this situation takes the form of low coercion and low structural compatibility. Here one would expect dissimilar

systems of social organization to coexist and subordinates to persist in maintaining their distinctive roles and institutions. Since coercion is low one would expect little strain in intergroup interaction. However, because the basic roles and institutions are so dissimilar, possibilities for group interaction are limited; and since each group defines its own reality, the roles most suited or epistemologically "natural" to that reality would be preserved.

CULTURE-CREATION AND SOCIAL RECONSTRUCTION

The final situation combines high oppression and low coincidence. Such a situation would seem to lead to culture-creation within the subordinate group. Jaeger and Selznick (1964:659) say that culture-creation is the result of an individual's or group's attempt to make sense of some new experience, to give it normative meaning. When two hitherto unassociated groups come into contact, and one is highly oppressed and cannot escape, isolate itself or mitigate that oppression, and when the new situation represents a sudden, radical loss of normative meaning, then subordinate cultural definitions will give way. Since cultural dissimilarities deprive the subordinates of alternative explanations, they will have to invent interpretations and definitions of this new reality.

They can do so by reinterpreting old conceptions of the universe and man, or by adopting, in part at least, the superordinate group's definitions of the experience. The third alternative is to create wholly new ontological and cosmological conceptualizations, new values, new norms, new modes of expression. Now by saying that subordinates create wholly new definitions, we do not mean create literally in the sense of producing something which has no prior form or substance. Rather, we mean forging new definitions which may be found in part in either culture before contact but not as they are found in the post-contact situation: they are created in the sense that they are the product of a new situation. Culture creation occurs most often in situation four. At the same time, however, some ideas may be assimilated from the superordinate culture:

this is precisely what happens when subordinates come to feel contempt for themselves for accepting if only in part, the superordinate view of themselves. Of course, the entire subordinate culture is not changed or obliterated. Rather, those ideas and modes of expression will survive which contribute normative meaning to subordinates and which are also considered innocuous or helpful by superordinates. The point is that the degree of culture-creation depends on the degree of both oppression and coincidence. In the hypothetical situation of absolute oppression and absolute low coincidence, an entirely new system of cultural meanings would have to be created by the subordinate group.

The fourth cell of paradigm B considers the case of low structural compatibility and high coercion. In this situation, roles and institutions are highly dissimilar, and yet the groups experience a great deal of coercive interaction. Since subordinate group members cannot isolate themselves from the interaction, and since native roles are either suppressed by the superordinate group or are irrelevant to a new situation, one would expect subordinates to create new roles which would enable them to engage in meaningful interaction among themselves and with members of the superordinate group. Again, by creating new roles—one is compelled to call it social reconstruction in the manner of Thomas—we mean modifying old roles and creating new ones. Again, modifying old roles is seen as creation because the expectations and authority of the new roles are to be found in neither group before contact.

Recall that in situations one and four of paradigm A, subordinates could come in time to share the superordinate group's definition of themselves and their culture as inferior. When this occurs role creation will also follow this pattern so that some new roles in the subordinate group will manifest this evaluation in the kinds of behavior expected from certain roles as well as in structural subordination itself: witness the "sambo" syndrome in the American South (Elkins, 1969:132).

In the new, radically different reality of the post-contact situation, subordinates will view their native institutions as inferior, illegitimate, or simply inefficacious. If they

no longer function, subordinates will create new ones. From a situation which renders old rules inadequate and which forces interaction, new cultural definitions will emerge and with them new roles which lead to new institutions. As with culture-creation, as absolute incompatibility and coercion are approached, all old institutions will be replaced.

OTHER CONSIDERATIONS

The processes indicated in the cells of the paradigm are not absolutely exclusive of each other. That is, each cell indicated only the predominant or expected process that will occur. Again, if it were possible to go to the limit on either end of the continuum one might find some greater degree of exclusiveness.

Since we clearly have no objective, absolute concept of oppression, we must accept oppression as it is defined by the oppressed. That we do so is of special importance because such cultural definitions of experience give this analysis whatever dynamic it has. These considerations notwithstanding, it might be helpful to present some conditions which might be considered empirical indicators of oppression. To be useful they should also follow from the theoretical treatment of culture and social organization presented here. Consider, for example, other than physical kinds of brutality. The most important of these would appear to be being placed in a situation one could not make sense of within those categories of normative meaning and value into which one had been socialized and which had always worked. A drastic, unexplainable change of experience brought about by an alien group is clearly a kind of brutality. Other indicators might be whether one were taken as a member of a group or as an individual or whether the subordinates remained within their native territory or were moved to some new territory (Lieberson, 1961). In addition to Lieberson's treatment of such a situation, the discussion so far suggests that if one were taken as a member of a group, the oppression would be lower than if one were taken as an individual, because for the former the group can preserve or contribute some normative meaning to the new situa-

tion, while the latter would be left to his own devices. Apropos of territoriality, having to adapt physically or culturally to a new environment produces great demands for normative meaning. Finally it seems fundamentally important whether the oppressed people are considered the potential or actual ontological equals of the oppressor, whether they are given legal status as human beings, because without it there can be no escape.

The prime example of oppression containing all these indicators and more is the case of the European slave trade in the New World, particularly in the American South. Africans were taken in large numbers, placed in a drastically different physically oppressive situation, split up so that few members of the same tribe or linguistic community were ever in the same vessel, brutally removed to a new territory where they were legally defined as sub-human. (Pope-Hennessy, 1967; Elkins, 1969; Morison, 1971). If suggestions for empirical indicators are required, these would be most important.

While the present analysis has concentrated on processes of change or persistence likely to occur within the subordinate group after contact, similar processes will occur within the superordinate group. Unfortunately, an analysis of these superordinate group processes is beyond the scope of the present work.

CONCLUSION

This paper tries to deal with specific issues in the theory of sociocultural change and persistence. It suggests alternative explanations to those based on the misuse of metaphor and analogy, and urges that we consider those explanations of change and persistence which derive from such concepts as culture and social organization. As such these paradigms represent a beginning and do not pretend to explain changes occurring between groups or classes within a single society, though one would expect that many of the causal principles and processes would be the same. The paradigms are intended only to explain the likely outcomes of contact between groups or societies under the conditions specified. From a sociological viewpoint as long as the mutual reinforcement of definitions of reality and its meanings are un-

disturbed, change will be unlikely; as long as experience and group interpretation and prescribed role behavior toward experience are consonant, culture, social structure and socialization processes will persist.

We refer here to the proposition that aside from events there is no basis for predicting change; it is not "natural" or immanent or genetic in social and cultural systems (Nisbet, 1969; Teggart, 1962). With events, change will occur as the result of a discrepancy between new experience and old cultural explanations of and institutionalized means of reacting to it.

Nisbet points out that nothing in the history of any group would allow us to specify beforehand the kinds of changes which will occur in it, what given factors or characteristics will *cause* change (Nisbet, 1969:291-4). We must depend on the sociological principle that people or groups will tend to conserve idea systems and institutions which are adequate *within the framework provided by their understandings* (Nisbet, 1969:270-2; Thomas and Znaniecki, 1958). In specifying the kinds of questions social scientists should be asking about social change, Nisbet asks "... what are the conditions under which actual social change takes place in the history of a given institution or a given mode of social behavior or cultural area?" (Nisbet, 1969:275). That is the problem to which these paradigms are addressed.

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A CAUSAL MODEL FOR THE POLITICAL PARTY PREFERENCES OF AMERICAN MEN *

DAVID KNOKE

Indiana University

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Using data from a 1968 national survey, we undertook a causal analysis of political party identifications of American men. The resulting path diagram estimates parameters for five sociological and political socialization variables with direct paths to party preference-occupation, class identification, ethnoreligious group and parents' party choices. Education and other social origin variables are viewed as having indirect effects on partisanship through the "socio-economic life cycle." Alternative specifications of the subjective class identification variables are explored.

POLITICAL sociology has long been concerned with elucidating the social bases of political cleavage in society. A series of electoral studies have illuminated the importance of ethnic, racial, religious and other group identifications in determining the vote (Lazarsfeld, *et al.*, 1944; Campbell, *et al.*, 1960). Lipset's seminal work with cross-national surveys was instrumental in creating the image of elections as "the democratic translation of the class struggle" (Lipset, 1963:230). Despite the diversity of variables believed to influence political behavior, methodological restrictions prevented investigation of the simultaneous effects of more than a small number of variables. The present paper aims to rectify this state of affairs by using path analysis to determine the causal relationship of social characteristics to political party orientation in the United States.

Several works in the literature have made steps in this direction; a brief review is in order that will raise the issues to be confronted. A series of articles by Segal and his collaborators (Janowitz and Segal, 1967; Segal, 1968; Segal and Knoke, 1970) used the so-called tree-analysis method for determining the variables most predictive of individuals' party preferences. The result-

ing pictures of "social structural cleavage" in the United States pointed to the importance of race, religion, occupation, education, region of residence and class identification; while variables such as income, property ownership and employment status carried relatively little if any weight. While Segal's findings confirm for a multivariate analysis the relationships to be expected judging from findings by investigators using bivariate cross-tabulations, no indication is given of the causal relationships pertaining among the independent variables. A further methodological criticism of the findings may be raised that, as the tree procedure performs analyses on successively smaller subsamples, some final partisan categories reflect more sampling error than true variation.

Goldberg (1966) attempted to determine the influence of social characteristics on voting using the Simon-Blalock method of partial correlation coefficients. After fitting four models to data, he selected one in which respondents' party preference intervenes between sociological attributes, father's attributes and party choice on the one hand and partisan attitudes and voting on the other. While Goldberg primarily sought to explain relations among the latter variables, that portion of his model depicting causes of party identification is relevant to the current research. In his model respondent's party identification (RPI) is directly affected by his social characteristics (RSC) and his father's party identification (FPI); while the father's social characteristics (FSC) affect RPI only indirectly through

* I thank the Inter-university Consortium for Political Research for making the data used herein available. Much gratitude is extended to Professors Otis Dudley Duncan, Robert W. Hodge and David R. Segal for their advice and comments on portions of the present research. Needless to say, none of the above parties bears responsibility for the errors contained in the final product.

their effects on RSC and FPI. In addition, FPI has a direct effect on RSC which Goldberg tentatively interprets as "upwardly aspirational parents adopting party identification normally associated with higher status groups and transmitting their status aspirations to their offspring with sufficient impact to have them realized to some extent."

Although Goldberg's model provides a starting point for the present analyses, a number of crucial differences must be noted. He restricted his estimation of parameters of the model to the 1956 Survey Research Center survey subsample of respondents on whom complete data were available and only those with a Republican or Democratic party identification. More crucial, his measures of social characteristics, RSC and FSC, are indices computed from five variables (religion, race, occupation, size of community, region) in which each respondent is assigned the expected value of being a Republican. These procedures have the undesirable effect of (a) exaggerating the degree of association between variables in the total population and (b) obscuring the individual effects of the component variables comprising the index. On the latter point for regression analysis Wright (1960:103) cautions, "If one part of a composite variable (such as a total or average) is more significant in one relation and another part in another, the treatment of the variable as if it were a unit may lead to grossly erroneous results."

THE DATA

The present analysis attempts to develop a causal model of the influence of sociological and political socialization variables on

party identifications of the adult male population of the United States. A schematic representation of the causal ordering of variables to be considered for the model is shown in Figure 1. While some uncertainty obtains, the causal ordering is intended to portray a temporal sequence typical in the life cycle of individuals or cohorts (see Duncan, 1969:84). The following analyses will assess the existence and magnitude of causal paths to variables taken as dependent from variables taken as temporally antecedent. No causal ordering among the measures of social origin is assumed, and relations among these "predetermined" variables are represented in path diagrams by curved two-headed arrows.

Data for analysis was obtained from the 1968 Survey Research Center presidential election survey. A Negro Supplement to increase the number of black respondents was combined with the regular national cross-section sample, requiring a double-weighting of white respondents to maintain representativeness. Size of the male portion of the sample selected for analysis was 1355. Measures of variables were as follows:

Party identification (Y) is a three-point scale of responses to the standard SRC questions in which Democrats and leaning independents are low and Republicans and leaning independents high, with strict independents in the middle.¹ For political socialization in the background variables, both father's (P) and mother's party choice (Q) are measured in similar three-point scales.

¹ Separate analyses using a Republican-non-Republican dichotomous measure of party preference did not yield differences in significant variables or the relative magnitudes of parameter estimates from those using tripartite coding.

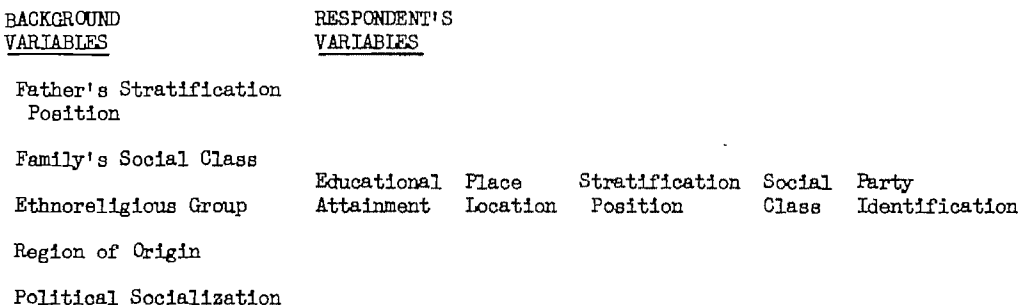


FIGURE 1. SCHEMATIC REPRESENTATION OF CAUSAL ASSUMPTIONS IN MODEL OF SOCIAL AND POLITICAL EFFECTS ON RESPONDENT'S PARTY IDENTIFICATION.

Social class of respondent (M) and family while growing up (N) are both dichotomies of middle-class and working-class responses to the question of subjective class membership. The respondent's stratification position is measured by Duncan's Socio-Economic Score for occupation (S) and a dichotomous variable for self-employment (K); the father's stratification measures are his occupational SES (X) and a dichotomy for farm-non-farm residence (Z). Current place location (R) is a dichotomy for urban residence. Education (U) is an eight-point scale corresponding to grouped years of schooling (see Blau and Duncan, 1967:165). Region of origin (B) is a Southern-non-Southern-born

dichotomy. Ethnoreligious group membership (E_i) is a set of dummy variables for five non-economic groups: blacks, Jews, Catholics, high Protestants (Episcopal, Congregational, Presbyterian, Methodist, Quaker and Unitarian) and other Protestants. Although the ethnoreligion variable is a measure of the respondent's current identification, Figure 1 shows it as a predetermined variable in Figure 1, indicating that whatever changes have occurred are not a problem in the present analysis.

Table 1 presents intercorrelations among the variables, the empirical basis for estimating the models' path coefficients. A few comments on the magnitude of the correla-

Table 1. Simple Correlations among Variables (decimals omitted)

Variable ^a	Variable ^a															Mean	S.D.	
	M	S	R	U	X	P	Q	N	Z	B	K	E ₁	E ₂	E ₃	E ₄			E ₅
Y	19	20	-07	18	15	47	49	19	-04	-23	06	19	08	-07	-07	-24	1.82	0.93
M		49	17	44	33	05	08	50	-26	-18	08	14	-13	08	11	-11	0.41	0.49
S			20	62	45	04	06	32	-36	-18	01	18	-13	08	13	-19	40.79	24.64
R				20	16	-09	-08	09	-35	-15	-08	05	-26	23	04	06	0.66	0.47
U					45	09	09	32	-37	-22	-03	19	-09	04	05	-15	4.71	1.83
X						14	14	47	-47	-16	-03	16	-11	03	13	-09	28.13	22.64
P							80	15	-01	-24	03	17	05	-12	-04	-11	1.74	0.93
Q								18	-02	-27	01	20	04	-11	-09	-11	1.74	0.93
N									-24	-07	12	15	-07	01	06	-10	0.29	0.48
Z										29	11	-07	23	-23	-11	06	0.35	0.48
B											01	-09	17	-29	-11	29	0.32	0.48
K												-06	10	-04	05	-08	0.19	0.40
E ₁													-42	-19	-06	-11	0.12	0.32
E ₂														-60	-18	-34	0.57	0.50
E ₃															-08	-15	0.21	0.41
E ₄																-04	0.02	0.15
E ₅																	0.08	0.27

^aY: Respondent's Party
M: Respondent's Social Class
S: Respondent's Occupational SES
R: Respondent's Urban Residence
U: Respondent's Education
X: Father's Occupational SES
P: Father's Party Identification
O: Mother's Party Identification

N: Family's Social Class
Z: Father Farmer
B: Respondent Southern-born
K: Respondent Self-employed
E₁: Respondent High Protestant
E₂: Respondent Other Protestant
E₃: Respondent Catholic
E₄: Respondent Jewish
E₅: Respondent Black

tion coefficients are in order before we proceed. Relationships among variables in the occupational achievement process have been most extensively investigated by Duncan and his colleagues (see Blau and Duncan, 1967; Duncan, *et al.*, 1968), and their work provides a check on the observed correlations in the 1968 SRC sample. The correlations reported for a 1962 Census Bureau survey were $r_{SX} = .405$, $r_{SU} = .623$ and $r_{XU} = .444$, which are closely matched in the 1968 sample. Correlations among political party identifications of children and parents are found in data gathered by Jennings in which high school seniors and their parents were directly interviewed. The two 1968 correlations based on adult males' recall ($r_{XP} = .47$; $r_{XQ} = .49$) are lower than the correlation of students' and combined parental partisanship (.59) reported by Jennings and Niemi (1968). Conversely, the 1968 parental partisanship correlation ($r_{PQ} = .80$) is higher than that calculated (.64) from data given in a table by Langton and Jennings (1969). The differences in correlations may reflect measurement differences (direct vs. recall), but the age differences and the absence of some 25 percent of the potential adolescent respondents through school dropout render results not strictly comparable. Goldberg (1969) reported a further analysis of the Jennings data in which he found 78 percent of the students correctly identified their fathers in broad categories of Democrat, Independent and Republican, with the remaining ascriptions about evenly split between incorrect partisanship and independent assignment. His statement, "Insofar as there is any systematic bias in the errors, it seems to operate in a manner not seriously debilitating to the present analysis" would seem applicable here.

ESTIMATING THE BASIC MODEL

There are five successive dependent variables in the schematic model in Figure 1 (occupational SES and self-employment of respondent are not temporally ordered). In the absence of a well-developed theory specifying the functional form which is to occur among the variables, a different tack must be taken in estimating the model's parameters. The model is translated into a

system of recursive equations in which all relations are asymmetrical, linear and additive. The coefficients of the equations are estimated from the known values of the predetermined and temporally antecedent variables by ordinary least squares methods. If we measure all variables in units of standard deviations, the structural coefficients are path coefficients (see Wright, 1960; Duncan, 1966) which in each equation represent the proportion of a standard deviation the dependent variable changes for a change of one standard deviation in the independent variable. In the following analysis, two sets of equations were calculated: the first in which all possible paths from independent variables were assessed, and a final set including only variables whose coefficients were at least 0.070 in the first set, a procedure which implies that the omitted variables made a negligible contribution to variation in the dependent variable. (All except one of the path coefficients in the final equations have a magnitude of one-tenth of a standard deviation or greater). Table 2 shows the regression analyses for estimating the path coefficients.

The second set is depicted in the usual path diagram in Figure 2, with the coefficients of the dummy variable set for ethnoreligion collectively represented by a dashed line. To avoid singularity in the matrix-estimation procedure, the coefficient of one variable in a set of dummies (in this case the blacks) must be set to zero in each equation (Johnston, 1963:222); thus the other ethnoreligious group coefficients represent deviations from the mean of the omitted group.

Only five of the dozen sociological and political socialization variables have substantial direct effect on the party identification of the male respondents. The insignificant variables are primarily the father's sociological characteristics and the respondents' ecological variables (region of birth and urban residence). The absence of major origin stratification influences on sons' party preferences is in line with Goldberg's results (1966), but the insignificant contribution of the ecological variables was obscured in Goldberg's use of a composite index variable for RSC. Another major variable lacking a direct path to party preference is respon-

Table 2. Multiple Regression Analyses to Obtain Path Coefficients for Basic Model

Dependent Variable ^a	Independent Variable ^a															R ²
	M	S	R	U	X	P	O	N	Z	B	K	E ₁	E ₂	E ₃	E ₄	
Partial Coefficients in Standard Form, b*																
Y _b	.080	.115	-.047	.020	-.012	.205	.252	.026	.028	-.062	.022	.218	.278	.176	.011	.333
Y _b	.093	.110				.213	.269					.243	.321	.210	.027	.328
M _b		.271	.044	.145	-.051	-.044	.017	.368	-.010	-.066	.046	-.002	-.044	-.011	.028	.393
M _b		.278		.153				.357								.380
S _b			.051	.481	.144	-.040	-.010	.066	-.052	.020		.160	.154	.173	.124	.448
S _b				.504	.189							.148	.129	.176	.123	.438
U _b					.270	-.002	-.035	.131	-.189	-.117		.178	.123	.053	.008	.283
U _b					.267			.125	-.192	-.105		.175	.123	.060	.014	.282
Standard Errors for b*																
Y _b	.029	.032	.025	.031	.030	.038	.039	.029	.029	.027	.023	.036	.045	.042	.027	
Y _b	.026	.027				.038	.038					.035	.043	.039	.026	
M _b		.029	.024	.029	.029	.037	.037	.027	.026	.026	.022	.034	.043	.041	.026	
M _b		.029		.029			.027									
S _b			.023	.025	.027	.035	.035	.024	.025	.025		.032	.041	.038	.024	
S _b				.024	.024							.032	.039	.036	.024	
U _b					.030	.040	.040	.027	.028	.028		.037	.046	.044	.028	
U _b					.030			.027	.028	.026		.037	.046	.043	.027	

^aY: Respondent's Party
M: Respondent's Social Class
S: Respondent's Occupational SES
R: Respondent's Urban Residence
U: Respondent's Education
X: Father's Occupational SES
P: Father's Party Identification
Q: Mother's Party Identification

N: Family's Social Class
Z: Father Farmer
B: Respondent Southern-born
E: Respondent Self-employed
E₁: Respondent High Protestant
E₂: Respondent Other Protestant
E₃: Respondent Catholic
E₄: Respondent Jewish

^b The b*coefficients in these sets were taken as estimates of the path coefficients.

dents' education (U). This is somewhat surprising in view of long-standing use of education by political sociologists as a measure of stratification position highly related to political orientation (see, for example, Campbell, *et al.*, 1960:207-8; Key, 1965: 245). The disappearance of education from the multivariate analysis of party preference is not difficult to understand, however, in view of its temporal proximity to the other background variables. Education in fact has an indirect effect on party identification through its substantial effects on the intervening variables occupational SES (S) and subjective class identification (M).²

² Indirect paths may be calculated by using the

Although the measures of respondents' social stratification position (S and M) both have significant direct and positive impacts on party preference, by far the greatest effect on Y is contributed by the two measures of parental party affiliation, P and Q. Of the 33 percent of total variance in Y accounted for by all five independent variables, the net increment due to P and Q alone is 20 percent, with an additional 6 percent shared between parental party and the other three independent variables because of their

basic path theorem for compound paths (Wright, 1960; Duncan, 1966). The indirect effect of education on party through occupation and social class ($p_{YU}p_{SU} + p_{YU}p_{MU} + p_{YU}p_{QU}$) is .08, almost half the zero-order correlation of U and Y.

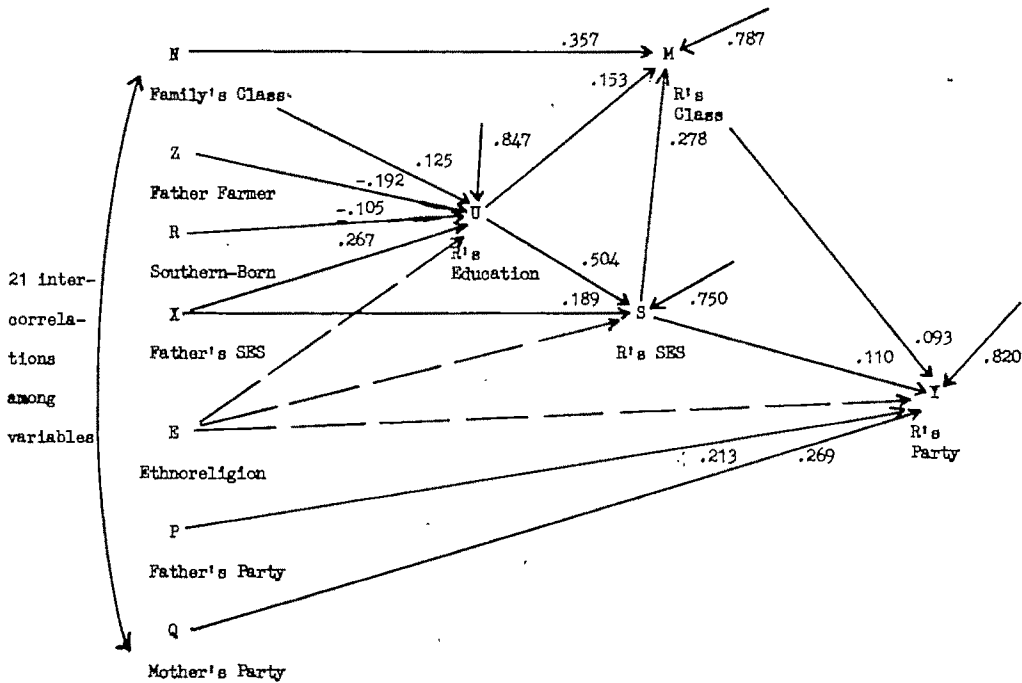


FIGURE 2. PATH DIAGRAM REPRESENTING THE DEPENDENCE OF RESPONDENT'S PARTY IDENTIFICATION ON SOCIOLOGICAL AND POLITICAL SOCIALIZATION VARIABLES.

intercorrelation. This finding should be convincing evidence in support of researchers on childhood political socialization who have consistently found early and durable formation of party preferences by the adolescent years (Hyman, 1959:46; Greenstein, 1965). The coefficients of the ethnoreligion dummy variables (E_i 's) shows that the five groups can be ordered on mean partisanship from most Republican (high Protestants) to most Democratic (blacks) *net* of differentials in the socio-economic distributions and political socialization experiences in the home. On the other hand, the five significant independent variables appear to account for most of the Southern-non-Southern born differences in partisanship. The zero-order correlation ($r_{YB} = -.23$) is reduced to a negligible partial coefficient in the regression equation ($b^*_{YB} = -.06$).

Background variables begin to have significant paths in the equations of variables which are taken as causally antecedent to respondents' party identification. Social class identification (M) receives direct paths from occupational SES (S) and education (U), which may be interpreted as an evaluation of current achievements in the subjec-

tive estimation of class position (Hodge and Treiman, 1968), and from familial social class (N), which may indicate a substantial intergenerational stability of subjective class; similar to the intergenerational transmission of party identification. The measure of the family's social class, "when you were growing up," may be ambiguous with regard to respondents' referents and subject to systematic bias in its reporting. In the next section some questions will be raised about the specification of this portion of the model. For the moment we may observe that the ecological variables (R and B) again fail to contribute significantly to the explained variance in the dependent variable, as do the other remaining background variables. Because of insignificant direct effects on party and indirect effects through S and M, the variable for urban residence, R, was eliminated as a dependent variable in further analyses. The finding that community size had a negligible impact on party affiliation contradicts the assertions of Segal and Goldberg. Whatever the urban-rural cleavages in party choice, the multivariate analysis suggests they are adequately accounted for by the other social and political variables.

Also lacking in the present findings are paths from parental party identification to occupational SES or subjective social class. Goldberg found such a significant but weak path from FPI to RSC. Rather than accepting his interpretation that such effects are surrogate measures for parental ambition for offsprings' success, the absence of such paths in the present model confirms certain basic premises about the American polity. Outside of possibly a few local machines, American political parties have not been noted as major institutional means to socio-economic achievement. In other societies, such as Soviet Russia, membership in political parties does result in differential access to such rewards. In the United States such links are neither theoretically predicted nor empirically observable.

The remainder of the path diagram draws heavily on models of the achievement process developed by Duncan (Blau and Duncan, 1967; Duncan, *et al.*, 1968). In the various versions involving many different variables and data sets, education (U) is viewed as intervening between origin occupation (X) and destination occupation (S). In the present analysis, current occupation is also directly affected by ethnoreligious group membership, a fact which reflects persistent racial and religious group differences in achievement net of social origins and educational attainment (see Duncan and Featherman, 1973). Similarly, educational attainment is affected by ethnoreligion and other social origin variables. Differences between the present representation and the various Duncan models, often based on data for non-Negro men with non-farm backgrounds, may also reflect the lack of a measure of father's education for which N, Z, and B may be to some extent substitutions. Such minor variations in the socio-economic achievement portion of the model should not obscure the major point about influences on respondents' political preferences: social background stratification and ecological variables affect party choice only indirectly, through the achievement process and correlation with ethnoreligious and parental party identification.

The stochastic disturbance terms in each equation of Table 2 have large path coefficients (calculated as $\sqrt{1-R^2}$), not atypical

of such models estimated from sample surveys of individual respondents. Such "residual paths" represent the effects of all unmeasured variables; but the adequacy of the model should not rest on the magnitude of the explained variance but rather the accuracy of the relationships specified.

AN EXTENSION OF THE BASIC MODEL

In the model depicted in Figure 1, respondents' social class identification is shown to intervene between occupational SES and party preference. This relationship was specified on the assumption that, since M is a current measure of subjective location in the stratification system, it would reflect an *evaluation* of prior and present socio-economic achievements, particularly occupation and education. However, there are strong reservations about this treatment. As calculated in Table 2, the direct effect of S on Y is much greater than its indirect effect through M, whose own direct impact on party, p_{YM} , is less than p_{YS} . These estimates of the relationships cast doubt on the accuracy of portraying M as a variable intervening between occupation and party preference. Duncan has suggested that rather than an evaluative variable, "class identification" in part is a projective question that taps the respondent's desires or inclinations as well as (if not instead of) his estimate of his objective standing in society" (Duncan, *et al.*, 1968:169).

Insight into how S and M affect Y can be gained by introducing a measure of initial party preference (I). Respondents were asked if they had ever considered themselves to be partisans of the party other than their current preference. Some 20 percent indicated that they had defected from their initial party. By re-assigning scores on the three-point scale for these respondents, a correlation between initial party and current party was found ($r_{XI} = .59$). Similarly, $r_{IS} = .09$; $r_{IM} = .16$; $r_{IP} = .50$; $r_{IQ} = .50$. A set of regression analyses were performed in which S, M, P and Q were taken as predetermined variables (the ethnoreligious variables E_i were omitted to simplify the extended model) and I was specified as intervening between them and current party preference, Y. The analyses are found in Table 3 and the results diagrammed in Figure 3.

Table 3. Regression Analyses For Extension of the Basic Model in Table 2

Dependent Variable	Independent Variable ^a					R ²
	I	S	M	P	O	
Partial Coefficients in Standard Form, b*						
Y	.431	.134	.040	.100	.181	.425
Y ^b	.436	.153		.098	.185	.423
I		.005	.118	.284	.267	.292
I ^b			.118	.284	.267	.292
Standard Errors for b*						
Y	.025	.024	.024	.036	.036	
Y ^b	.025	.021		.036	.036	
I		.027	.027	.039	.039	
I ^b			.023	.039	.039	

^a Y: Respondent's Party
 I: Initial Party
 S: Respondent's Occupational SES
 M: Respondent's Social Class
 P: Father's Party Identification
 Q: Mother's Party Identification

^b The b* coefficients in these sets were taken as estimates of path coefficients.

Initial party has by far the largest direct path to current party—over two-fifths of a standard deviation—indicating the great intragenerational stability of the party preference measure. Parental party choices, P and Q, have their strongest effects on the initial party, but still have net paths to Y, showing political socialization continues beyond the formative period. Note that, for the father at least, the indirect effect of P on Y through I exceeds the direct net path. The two stratification measures exert different effects on party preference: respondent's occupation (S) directly affects his current but not initial party preference, while subjective class (M) has the opposite pattern. Although the exact timing of I with respect to the other events in the socio-economic life cycle is ambiguous, the absence of a significant p_{IS} suggests that initial party formation occurs prior to labor force entry (concurrent in by most literature on political socialization); while the strong p_{YS} indicates that subsequent achievement of occupational status translates into a Re-

publican preference net of early socialization. The substantive meaning of the subjective class identification effect is more ambiguous. One possibility is that M, while measured in the present, is less an evaluation of current occupational success and more an aspirational variable exercising its strongest effect on party choice when first formed. The literature is silent on the extent to which subjective class identification is stable over time, as the party preference measure is believed to be. In all likelihood M is a fallible indicator of earlier motivations, mixing both aspirational and evaluative components from current and past experiences. One possible resolution to this dilemma in conceptualizing the measure of subjective class is to treat M as an indicator of a unobserved variable (see Hauser and Goldberger, 1971) having indirect effect on current party preference through its direct effects on early preference (I) and subsequent occupational achievement (S). Until such an analysis is performed on data that includes additional indicators of aspiration, the model developed in the right-hand portion of Figure 2 will have to represent an abbreviated form of a more complex causal scheme. The ambiguity over specifying the causal path from S to M in the basic model does not invalidate the finding of significant net paths from each of these and three other variables to Y, although the magnitude of the indirect effects may well be altered.

DISCUSSION

The foregoing exercise has resulted in the selection of a model representing causal influences of sociological and political socialization variables on the political party preferences of American men. In many ways the findings seem merely to confirm various scattered evidence in the literature on the importance of this or that variable for political cleavages in the society. But the present study also considers the multivariate causal relationships of social characteristics to party preference. In addition to ruling out the importance, direct or indirect, of ecological variables like region of birth and place of residence, the analyses found the effects of social origin stratification position to be indirect only, either through effects on the

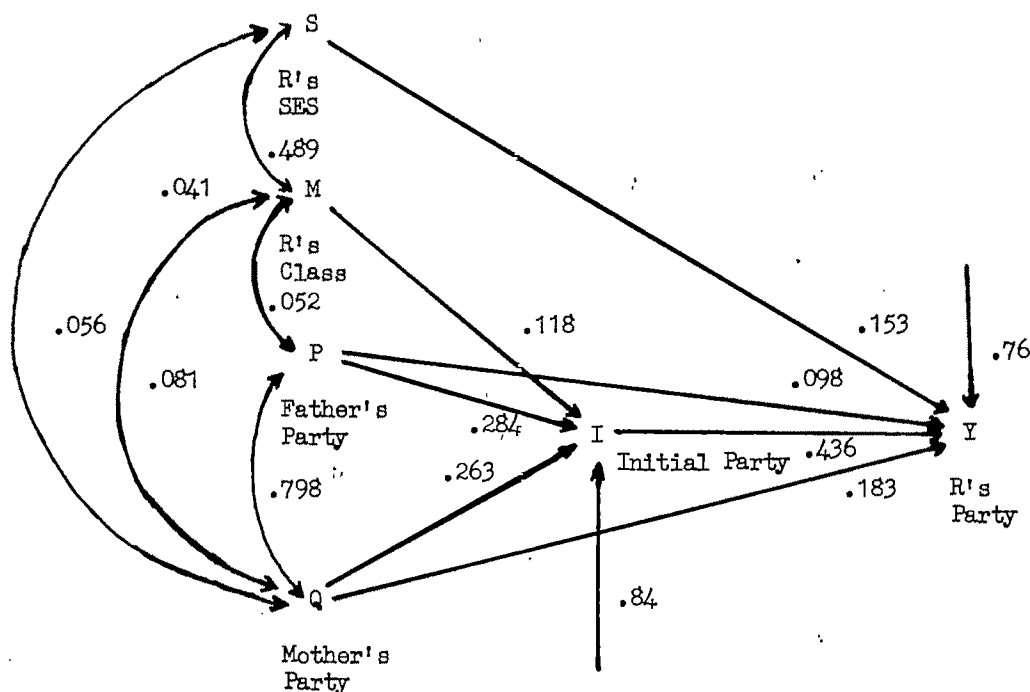


FIGURE 3. PATH COEFFICIENTS IN THE EXTENSION OF THE BASIC MODEL IN FIGURE 2.

respondent's achievement process or by intercorrelation with the parents' party choices. This complete accounting of the effects of origin SES on political orientation via intervening variables calls into question the importance of intergenerational "mobility effects" on political behavior (cf. Thompson, 1971; Lopreato and Chafetz, 1970; Knoke, 1973).

More important than tying together many threads in the sociology of party support, the present research provides explicit parameter estimates. The use of path analysis algorithms permits an assessment of the relative importance of antecedent variables in producing variation in the party preference measure net of other independent variables, for example, the greater variation induced by political socialization compared to respondent's occupational, class and ethno-religious affiliations. The parameter estimates yielded by data from a national sample in 1968 should be useful for comparison to other data sets taken at different times in discerning changes in the relative influence of causal components of party behavior.

The custom in closing is to try to meet the potential objections of critics. A frequent

criticism of causal modelling of this type is that crucial variables have been omitted, which, if inserted at the critic's specifications, would substantially alter the estimates of the model. It is difficult to anticipate all possible candidates for the role of the "forgotten X," but one plausible set would be additional political measures intervening between origin and/or destination achievements and the party preference variable. Extant measures of political orientations, such as perception of candidates or campaign issues, have been eliminated from such a role in the present model because their contemporary and ephemeral nature makes them more likely to be consequences than causes of party identification. But, an astute student of politics might interject, one should have included measures of more durable political orientations, such as liberalism-conservatism or belief in the "success ethic." Pleading the unavailability for secondary analysis of such variables in the present data is too easy. Certainly, measures of ideology known to be relatively uncontaminated by the political issues of the day should be added to the basic model as possible causes of party preference. However, advocates of such views must contend with

a mass of findings in the literature on political socialization which show that partisan identifications are socialized primarily on affectual and symbolic, rather than cognitive and instrumental levels (see Hyman, 1959; Greenstein, 1965). The effect of subsequent ideological perceptions may be relatively small in exerting shifts in party orientation, as the magnitude of inter- and in-ragenerational party choice correlation coefficients suggests. Nevertheless, should pertinent variables become available, they should be included in an elaboration of the basic model. In addition to temporally stable ideological measures, one might include political orientations of spouse, peers and other primary groups and structured interactions in occupational and ethnoreligious associations.

In line with Goldberg's comments (1966) on his model for voting behavior, the present model does not provide for changes in party identification in response to explicit political events. The stance in this article has been that so-called "partisan attitudes" must be considered primarily results rather than causes of party preference. Future researchers may wish to study models which allow for reciprocal influence of identification and attitudes—either through lagged variables as Goldberg suggests or by simultaneous equation methods (see Duncan, *et al.*, 1968). A more thorough investigation of the reaction of individuals to political events will have to employ cohort analyses, perhaps following panels of youths from formative socialization through labor force entry and noting reactions to significant political events (e.g., realigning elections). The present model is but a first approximation and testimony as much to what we do not know as to what we do.

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DIFFERENTIAL MORTALITY AND THE FORMATION OF POLITICAL ELITES: THE CASE OF THE U. S. HOUSE OF REPRESENTATIVES *

JAMES C. McCANN
University of Washington

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This paper documents a substantial differential in the death rates of northern and southern Democrats in the U. S. House of Representatives. The mortality of southern Democrats is found to be quite low in comparison to that of other American professional and white collar workers, while the mortality of northern Democrats is found to be excessive relative to that experienced by middle class Americans. Multiple decrement life table procedures are adapted to evaluate the impact of the mortality differential on the distribution of seniority in the House. The results show that low death rates are an important factor in the fashioning of the numerical advantage that Southerners hold among senior House Democrats.

INTRODUCTION

THE role of differential mortality in fashioning the composition of political elites seems never to have been explored. If it could be assumed that death were a very small risk for aspirants to, or incumbents in, positions of political power, or if it could be assumed that the risk of death did not covary with characteristics of sociological interest, there would be little point in calling attention to the matter. However, certain important national elites, notably the United States congressional leadership and

the Soviet Communist Party leadership, are heavily represented in older men, men who face serious risks of death by virtue of their age. Their continuation in positions of leadership is not only conditional on their political fortunes, but on their physical survival as well. When the leadership is aged, mortality not only conditions tenure in elite positions, but plays a role in the selection of elite members. Elite positions in party structures and legislative bodies are often attained only after years of service. If a position of leadership is to be attained, physical as well as political misfortune must be successfully averted.

In this paper we will show that differential mortality has figured importantly in the formation of the Democratic leadership in the U. S. House of Representatives in recent years. We will show that Democratic representatives entering the House from southern states in the 1930's have lived longer than

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their northern colleagues. This is partly because Southerners, on the average, enter Congress at a somewhat earlier age than Northerners, but it is primarily accounted for in that southern congressmen face lower age-specific mortality risks than other congressmen. Finally, we well demonstrate that the consequences of this regional differential in mortality are not politically trivial. The disproportionate power which Southerners wield in the House derives from their successful exploitation of the seniority system. If the southern Democrats faced the same mortality risks as Northerners, their representation among senior House Democrats would be considerably smaller and their ability to exploit the seniority system would be substantially reduced.

The contribution of differential mortality to southern power in the House seems to have escaped the notice of students of politics. Wolfinger and Heifetz (1965:342) discovered that a larger proportion of northern than southern Democrats die while in Congress.¹ They assert that the regional difference in age at entry accounts for the difference in mortality (pp. 345-7). Wolfinger and Heifetz accord the differential little regard in any case, for they conclude that "lack of party competition [in southern congressional districts], aided perhaps by southern favoritism in the appointments of subcommittee chairmen—and in the wake of the northern Republican sweep in 1946—seems to be an adequate explanation for the disproportionate number of Southerners in influential positions in the House" (Wolfinger and Heifetz, 1965:349). In fact, this explanation is not adequate.

DATA

The data used in this study are the vital and electoral histories of all persons starting first terms in the House of Representatives in the years 1931 to 1939.² The major vari-

ables in this analysis are date of birth, date of entry into Congress and, when appropriate, year of exit from Congress, reason for leaving, and year of death. These were taken from the *Biographical Directory of the American Congress* (1961). Information on these men is complete through January 1, 1961. The life tables and the decrement tables describing length of service in the Congress are based on the experience of persons from the start of their first full term in Congress to the beginning of 1961. The study population consists of 609 individuals, including 206 Republicans, 300 Democrats from northern and western states, and 103 Democrats from the states of the Old Confederacy. Only three of the 206 Republicans represented districts in these southern states.

DIFFERENTIAL MORTALITY IN THE HOUSE OF REPRESENTATIVES

Average life expectancies at selected ages for northern and southern Democrats and for Republicans entering the House of Representatives in the 1930's are shown in Table 1.³ Average life expectancy at age x , written e_x , is the mean number of remaining years of life for persons surviving to their x^{th} birthday. For example, the value of e_{50} for Republicans is 21.6 years. This means that Republicans who were alive at their fiftieth birthday lived, on the average, 21.6

beginning of the preceding Congress. Thus, not all "first term" congressmen have exactly the same amount of seniority. Those defined as starting a first term all have less than one full term of seniority; those defined as starting a second term all have less than two, but at least one, full term of seniority; and so forth. Those who enter after the convening of a Congress and are not reelected to the succeeding one do not come under observation in this study. Those who enter after the convening of a Congress and are reelected are counted as starting a first term with the convening of the Congress to which they were reelected.

³ These life tables do not naturally "close" because observation of the lives of these congressmen in the years 1931 to 1961 does not yield an age at which the proportion dying is 1.0. The life tables were made to close by assuming that the life expectancy at age eighty-five for each of the three classes of congressmen was 4.3 years, the value for U.S. white males in 1960. In fact the mortality of southern Democrats in the ages past eighty-five is lower than that of northern Democrats and of Republicans, and so the procedure understates the actual mortality differential.

¹ According to the authors, 37% of those northern Democrats who were in safe or relatively uncompetitive seats and 22% of those southern Democrats who were in safe seats and who left the House in the years 1947 through 1963 left because of death. (Table 7, p. 342).

² An individual who is seated at the opening of a Congress is defined as starting a first term with that Congress if he had not been seated at the

additional years or to age 71.6. Corresponding values for white American males in 1940 are presented for comparison in Table 1.

The values for northern Democrats, for Republicans, and for U.S. white males are generally in rather close agreement. The values for southern Democrats are much higher than the others. The differential in life expectancy for House Democrats from the two regions is substantial. This differential is 6.9 years at age forty. The differential in life expectancy in the U.S. population is 5.5 years for males and females and 3.2 years for the white and non-white races at age forty. The differential for Democrats from the two regions is 3.4 years at age seventy. The corresponding differential in the population at age seventy, is 2.4 years for the sexes and 0.5 years for the races.

The Standardized Mortality Ratio (S.M.R.) is typically used to index occupational and social class differentials in mortality. This measure is the ratio of observed deaths in a class to the deaths expected in the class given its age structure and some standard schedule of mortality rates. The ratio is multiplied by 100 according to convention. Thus, an S.M.R. of 100 indicates a mortality level in the study population equivalent to that in the standard: a value greater than 100 indicates higher mortality in the study population than in the standard, and a value less than 100 indicates lower mortality in the study population than in the standard. Given the age specific exposure of northern Democrats from the time they en-

tered Congress until 1961 and taking the 1940 mortality rates for white males as a standard, 174.62 deaths would be expected. In fact, 187 deaths were observed over the period. The Standardized Mortality Ratio for northern Democrats is $100(187/174.62) = 107.1$. The value for Republicans is 98.9. As with the life expectancies, the S.M.R. for southern Democrats, 57.8, deviates sharply from those of the other groups. The differential in mortality between southern Democrats and other representatives exceeds that between the social classes. The S.M.R. for males ages twenty-five and older in the United States in 1960 is 113 for those earning \$2,000 or less annually and eighty-nine for those earning \$10,000 or more. It is 101 for those with less than five years of education and eighty-nine for those with thirteen or more years of schooling (Spiegelman, 1968:96).

Given these comparative differentials it should come as no surprise to learn that the low level of mortality observed in southern congressmen is not accounted for by regional differentials in the mortality of the total population. White adult males in the South Atlantic and East South Central states experience virtually the same level of mortality as white American males generally (U.S. Public Health Service, 1956).

Nor is the differential adequately accounted for by random error. The standard errors of the S.M.R.'s are 10.0, 9.5, and 7.9 for Republicans, northern Democrats, and southern Democrats, respectively. The difference in the ratios is 41.1 points between Republicans and southern Democrats and 49.3 points between northern Democrats and southern Democrats.

Finally, these results are not due to an underregistration of southern deaths. The *Biographical Directory* reported 187 or 62.3% of the 300 northern Democrats but only thirty-seven or 35.9% of the 103 southern Democrats as deceased as of January 1, 1961. Each of the sixty-six Southerners reported alive was traced to 1968 through other sources. For each of them a date of death after 1961 or an activity in the years 1961 to 1968 was established. Congressional historians helped ascertain the status of eighteen of the more obscure southern congressmen. It was established that all of the

Table 1. Life Expectancies of Persons Entering the House 1931-1939 and of U.S. White Males, 1940

	Rep- licans	Northern Democrats	Southern Democrats	White Males ^a
e40	30.3	29.1	36.0	30.0
e50	21.6	21.6	26.0	22.0
e60	15.8	15.8	19.0	15.0
e70	10.6	10.7	14.1	9.4
e80	6.0	6.1	7.3	5.3

^aSource: *Vital Statistics of the U.S. Life Tables*, 1968, Table 5-5, p. 11.

sixty-six Southerners reported alive as of January 1, 1961 were alive at that date.

The mortality ratios for northern Democrats and Republicans are near 100, the standard value for white males in 1940, while the ratio for southern Democrats is well below 100. These results imply that the differential is a consequence of exceptionally low mortality among southern Democrats. However, the impression that the mortality of southern Democrats is deviant weakens when professional men living in the same period are taken as the standard. Guralnik (1963) has prepared standardized mortality ratios for men in various occupational categories in 1950. These ratios are, for obvious reasons, limited to experience in the ages twenty to sixty-four. The standard population is all males (not specific for race) in the year 1950. Values for selected non-manual occupations are shown in Table 2. For comparison, mortality ratios for the three categories of congressmen, restricted to ages under sixty-five and standardized on the experience of all males, are shown. There are two S.M.R.'s in the table for each congressional group. In one the death rates of

U.S. males in 1940 is used as the standard and in the other the 1950 rates are employed. The lives of the congressmen are observed over the thirty-year span, 1931 to 1960; and the mortality rates in the population were changing in this period. Therefore, no single annual schedule is strictly appropriate as a standard for comparing the mortality of congressmen to the mortality of the population. The 1940 and 1950 schedules were selected because a large proportion of our observations of the lives of this cohort of congressmen fall between these years.

The results in Table 2 indicate that in the ages under sixty-five mortality of southern congressmen is low relative to that of other white males in most professional and other non-manual employments. It is lower than the mortality of lawyers and judges, the typical occupation of men entering the House. However, it is not extreme by white collar standards. Teachers, for example, have lower mortality. The mortality of other congressmen, and particularly that of northern Democrats, is excessive by white collar standards. The mortality ratios of northern Democrats are exceeded in only one of the

Table 2. Standardized Mortality Ratios for Selected Nonmanual Occupations, U.S. Males, 1950,^a and for Republicans, Northern Democrats, and Southern Democrats Entering the House 1931 to 1939 for the Ages 25 to 64

Occupation	SMR	Occupation	SMR
Musicians and music teachers	162	Clergymen (white)	83
Pharmacists	126	Designers and draftsmen	75
Real estate agents and brokers	126	Mail carriers	68
Authors, editors, and reporters	102	Officials and inspectors, state and local administration	68
Salesmen and salesclerks (retail)	99	Teachers (n.e.c.)	61
Accountants and auditors	94	Social, welfare, and recreation workers	55
Physicians and surgeons	91	Social scientists	45
Lawyers and judges	90		
Salesmen and salesclerks (mfg.)	89		

		SMR	S.E.
Northern Democrats	(1940) ^b	132.1	13.7
Northern Democrats	(1950)	145.6	15.1
Republicans	(1940)	107.6	16.2
Republicans	(1950)	124.9	18.8
Southern Democrats	(1940)	68.4	16.1
Southern Democrats	(1950)	81.4	19.2

^aSource: Lillian Guralnick, *Mortality by Occupation and Industry*, Table 2, p. 82.

^bYear in parenthesis refers to the annual schedule of mortality rates used in the standardization.

forty non-manual occupations for which Guralnick prepared the index—musicians and music teachers.

The results for congressmen in these comparisons are based on a small number of deaths—92, 44, and 18—for northern Democrats, Republicans and southern Democrats, respectively—but the standard errors are sufficiently small to support the inference that northern Democratic representatives have excessive mortality relative to other men in professional and non-manual occupations, at least up to age sixty-five.

This sharp pattern of differential mortality does not appear among congressmen entering the House before 1930. Standardized mortality ratios were computed specific to each of the party-region categories for the cohorts entering the House 1911 to 1919 and 1921 to 1929 as well as 1931 to 1939. Within each of the two earlier cohorts the S.M.R.'s for Republicans, northern Democrats, and southern Democrats were similar. A preliminary analysis of Democrats in the 1941 to 1949 cohort shows a continuation of the regional differential in mortality.

It is, therefore, reasonable to inquire as to what characteristics and experiences distinguished southern Democrats from other representatives entering after 1930 which did not distinguish these classes in earlier cohorts. There are two factors which are related to mortality that should be mentioned in this regard, though unfortunately, it is not possible to evaluate either of them with the materials at hand. First, there is suggestive evidence that the pace of political campaigning picked up in the New Deal era. The level of political interest, as measured by voter turnout, increased in this period (Lane, 1959:22). Districts which had formerly been "safe" were sharply contested as the political loyalties of the electorate shifted. However, the South remained a one-party region and was not affected by these changes. One conjecture, then, is that the life of the politician became more taxing in the North for men of both parties in the New Deal era and after, while remaining a less demanding business in the South. A second possibility is that differential use of alcohol and tobacco characterized northern and southern congressmen in this cohort. The sale of alcohol was legalized in 1933

in the wake of increased use in the 1920's and cigarette consumption increased sharply after World War I (Preston, 1970:15). It is possible that the 1931-39 cohort of congressmen was the first to be affected by these habits and that they were differentially adopted by northern politicians. This suggestion is obviously at variance with the tobacco and bourbon image of southern politicians. On the other hand, it might be that the conservative southern electorate is relatively less tolerant than the northern electorate of officials who smoke and drink.

The origins of the differential in the mortality of northern and southern congressmen are obscure. One important consequence of the differential is, on the other hand, very clear. The relatively low mortality of southern Democrats has contributed significantly to their strong representation in the senior Democratic House membership.

THE POLITICAL CONSEQUENCES OF DIFFERENTIAL MORTALITY

During the 1930's 403 Democrats began freshman terms in the House of Representatives. Of these, three hundred were from northern states and 103 represented districts in the eleven states of the Old Confederacy. Only sixty-one of these persons succeeded to at least a seventh uninterrupted term, including thirty-three southern and twenty-eight northern members. This southern advantage of five members at the seventh term rises to eight members in the following term and to nine members in the eleventh term. The tenacity of the southern Democrats is not without consequence. Committee chairmanships in the House are generally assigned to members on the basis of seniority. In fact, in every Democratic congress convening from 1949 to 1961 the seniority system was followed without exception. This means that committee chairmanships were invariably assigned to the committee member with the most seniority (Polsby *et al.*, 1969:796, 803-4). These chairmen guide the work of the House and may block, amend, delay, or otherwise frustrate legislation which they view unfavorably. Senior house members also have a strong voice in the allocation of committee assignments to junior members and they may influence the dis-

tribution of patronage (Polsby *et al.*, 1979).

The fact that Southerners, despite an initial three-to-one disadvantage, come to outnumber Northerners among the senior House Democrats is largely accounted for by the higher rates of re-election for Democrats in the South. Until very recently the South was a one-party region and congressional elections were almost invariably won by the democratic nominee. Often they were uncontested. Facing little attrition at the polls, Southerners tend to hold their seats. Northern Democrats, on the other hand, usually face highly competitive opposition in any bid for re-election and are decimated by frequent unfavorable outcomes. Only one of the southern Democrats entering in the 1930's was defeated by a Republican, while 125 of the Northerners were driven from office by opposition party candidates.

Largely because of differential electoral risks, the combined attrition of Northerners due to retirement, failure to attain renomination, and defeat in a general election—all factors except death—is much greater than that for Southerners. Of the three hundred Northerners starting a first term eighty-two, or 30.7 percent, failed to start a second term for one of these reasons and 23.4 percent of those who succeeded to a second term failed to attain a third on one of these accounts. These three hundred northern Democrats served, in whole or in part, a total of 971 terms in the twenty-two years following their entry into the House. In 253 of these 971 terms, or 26.0 percent of the cases, the incumbent concluded his service in Congress because he retired, was not renominated, or was defeated in an election. The overall attrition rate from causes other than death, then, was .260 "failures per person per term." The failure rate for Southerners is much less severe. Factors other than death removed only 13.6 percent of the initial cohort before the start of a second term, while 17.0 percent of those succeeding to a second term failed on one of these accounts to attain a third. The overall attrition rate—death excepted—is .152 for southern Democrats.

Because of differential mortality, northern Democratic congressmen entering the House in the 1930's also faced a greater risk of dying in Congress than their southern colleagues. When the number of deaths in office

is divided by the total number of terms served, the results are .039 for the Northerners and .016 for Southerners. These rates read as .039 and .016 "deaths per person per term." This difference in death rates cannot be attributed entirely to age. The Standardized Mortality Ratio for Northerners *in office* is 107.1, while the value for Southerners is 57.8.

While the death rate for Northerners in office is nearly two and one-half times larger than the rate for Southerners, the risk of death is much smaller than the risk of attrition from other causes in both regions. Only 13.1 percent of the northern Democrats and 9.4 percent of the southern Democrats who left office before the start of an eleventh term left because of death. These rather small proportions can give the impression that death, despite its uneven distribution, is a factor of little significance in the formation of the southern seniority advantage. However, such rates and proportions understate the effects of mortality.

DIFFERENTIAL MORTALITY AND SENIORITY: MODEL I

A meaningful evaluation of the contribution of differential mortality to the southern advantage in seniority may be obtained by comparing the observed regional seniority distributions to those which would be expected if the mortality of northern and southern congressmen were at the same level. The approach adopted here is to adjust the mortality of southern Democratic congressmen to fit the pattern experienced by their northern colleagues. The essential task in such an evaluation is to construct the adjusted seniority distribution. The observed seniority distribution is a consequence of the operation of mortality and other factors, including retirement and electoral defeat, on the initial cohort over time. In order to generate the distribution which would obtain when mortality is altered and the intensity of other factors is held constant, it is necessary to factor or separate the mortality component from the other components of decrement.

The factoring of these components is facilitated by the assumption that the risk of leaving office because of death and the risk

of leaving office for other reasons operate independently. From this assumption it follows that the probability, $P_i(\bar{D} \cap \bar{R})$, that an individual in the House at the start of the i^{th} term will "both not die and not leave office for other reasons" and, hence, will succeed to the $i+1^{\text{th}}$ term is the product of the two probabilities given in the expression,

$$P_i(\bar{D} \cap \bar{R}) = P_i(\bar{D}|\bar{R}) \cdot P_i(\bar{R}|\bar{D}). \quad (1)$$

The factor $P_i(\bar{D}|\bar{R})$ in (1) is interpreted as the probability that an individual starting an i^{th} term will succeed to an $i+1^{\text{th}}$ term given that death alone operates as a risk, and $P_i(\bar{R}|\bar{D})$ is interpreted as the probability that an individual starting an i^{th} term will succeed to an $i+1^{\text{th}}$ term given that only risks other than death operate to limit tenure. For a detailed treatment of the factoring of "survival" probabilities see Chiang (1968:242-65).

If the term-specific survival probabilities, $P_i(\bar{D} \cap \bar{R})$, are assumed to be independent of one another, the probability, l_k that an individual starting a first term will succeed to a k^{th} term is the product of the probabilities of surviving each interval up to the k^{th} . Thus,

$$l_k = \prod_{i=1}^{k-1} P_i(\bar{D} \cap \bar{R}). \quad (2)$$

Expression of $P_i(\bar{D} \cap \bar{R})$ in terms of its factors gives

$$l_k = \prod_{i=1}^{k-1} P_i(\bar{D}|\bar{R}) \cdot P_i(\bar{R}|\bar{D}), \text{ or} \quad (3)$$

$$l_k = \left[\prod_{i=1}^{k-1} P_i(\bar{D}|\bar{R}) \right] \cdot \left[\prod_{i=1}^{k-1} P_i(\bar{R}|\bar{D}) \right], \quad (4)$$

and in alternative notation,

$$l_k = l_{k,d} \cdot l_{k,r} \quad (5)$$

Equations (4) and (5) show that the survivorship function, l_k , may be factored in the same manner as the term-specific survival probabilities. The probability that an individual entering Congress will start a k^{th} term is, then, the product of $l_{k,d}$, the probability he will not die in office given death is the only risk limiting tenure, and of $l_{k,r}$, the probability he will not be removed from

office by some event other than death, given that only risks other than death limit tenure.

If the two risks operate independently, the probability $l^*_{k,r}$, that a southern Democrat will succeed to the k^{th} term, given the prevailing southern risks of leaving office for reasons other than death and the northern risks of leaving because of death, is

$$l^*_{k,r} = l^{n_{k,d}}_{k,d} \cdot l^{s_{k,r}}_{k,r}, \quad (6)$$

where the superscripts n and s refer to the survival of Northerners and Southerners, respectively. Finally, the expected number of Southerners surviving to the k^{th} term when mortality is adjusted to northern levels, $S^*_{k,r}$, is simply

$$S^*_{k,r} = S_1 l^*_{k,r} = S_1 \cdot l^{n_{k,d}}_{k,d} \cdot l^{s_{k,r}}_{k,r}, \quad (7)$$

where S_1 is the number of Southerners starting first terms in the House in the 1930's.

Values of $l^{n_{k,d}}_{k,d}$ and $l^{s_{k,r}}_{k,r}$ were estimated from the experience of congressmen within terms of office. In particular, the mortality estimates are based on deaths in office only. Details on the estimation procedures are given in the appendix.

Table 3 shows the observed numbers of northern and southern congressmen in office at the start of each term from the first through the eleventh and the numbers of Southerners expected if the South were subject to the term-specific risks of death prevailing in the North but were governed by southern risks of leaving office for other reasons.

The results indicate an advantage for the

Table 3. Observed Numbers in House at each Term, by Region, and Expected Numbers with Southern Mortality Adjusted

Term	North Observed	South Observed	South Adjusted	Percentage Reduction
1	300	103	103	--
2	214	88	87.8	0.2
3	155	72	69.6	3.3
4	109	61	55.7	8.7
5	76	45	43.5	3.3
6	45	38	34.4	9.5
7	28	33	28.8	12.7
8	19	27	21.0	22.2
9	14	22	16.9	23.2
10	11	19	11.5	39.5
11	9	18	10.5	41.7

South at every term due to differential mortality. In the seventh term, for example, 28.8 Southerners would be expected in office if they were governed by the northern schedule of mortality risks. In fact, thirty-three remain in office under the more benign southern regime. The effects of differential mortality are modest in the first six terms but sharpen later. The righthand column of Table 3 shows the percent reduction in the observed number of southern officeholders at each term due to substitution of the northern mortality schedules. These percentages rise from 0.2 to 9.5 in the first six terms. The reduction is 22.2 percent at the start of the eighth term and an impressive 41.7 percent with the beginning of the eleventh term.

A comparison of the two arrays of observed values shows five more Southerners than Northerners in office by the start of the seventh term; with the eighth term the southern advantage is nearly three to two; and at the beginning of the eleventh term it is two to one. When southern mortality is adjusted, the South maintains but a thin edge over the North in these higher seniority categories. Southern congressional influence, which depends so strongly on the exploitation of the seniority system, would surely have been less impressive had the Southerners been subject to the mortality rates which the Northerners faced.

MODEL II: AGE DEPENDENCE AND TERM DEPENDENCE

The preceding model is limited by the assumption that the risk of death is dependent only on seniority or length of service in Congress. This is not an entirely satisfactory assumption since mortality risks depend on health which, in turn, is strongly conditioned by age. Age differences among congressmen are reflected in length of service only to the degree that congressmen are similar with respect to age at entry into the House. In fact, there is considerable variation in age at entry including some systematic differences by region. In particular, southern Democrats have a somewhat more youthful age at entry distribution than northern Democrats and this difference could account for some of the difference in the mortality risks measured in

Model I. Even if the age at entry distributions were the same in both regions, differences in age composition could arise later if the relationship between age at entry and the risks of retirement and defeat differed in the two regions. If, for example, Southerners who entered the House at older ages were differentially selected out by retirement and defeat but no such selection by age at entry occurred in the North, the term-specific mortality rates of southerners would be reduced in the later terms due to the *formation* of a relatively youthful age distribution.

In order to control for the effects of age composition and age selection, age at entry was added to the model and the two survivorship terms, $l_{k,d}$ and $l_{k,r}$, were made specific for age at entry. An exact expression for the number of Southerners surviving to the k^{th} term is, under these specifications,

$$S_k = S_1 \int A^s(x) \cdot l_{k,d}^s(x) \cdot l_{k,r}^s(x) dx, \quad (8)$$

where, S_1 is the number of Southerners starting a first term,

$A^s(x)$ is the proportion of the Southerners who entered the House at age x ,

$l_{k,d}^s(x)$ is the probability a Southerner entering the House at age x will survive to term k , given death alone operates as a risk of leaving office, and

$l_{k,r}^s(x)$ is the probability a Southerner entering the House at age x will survive to term k , given that only risks other than death are operating.

Expression (8) allows survivorship in the face of mortality, $l_{k,d}^s(x)$, to depend on both age at entry and length of service in the House; and these, of course, add to current age. $A^s(x)$ weights the mortality component for the initial age distribution and $l_{k,r}^s(x)$ modifies these weights for attrition due to retirement and defeat.

To obtain the expected number of Southerners surviving to the k^{th} term if the southern Democrats had the same mortality risks as northern Democrats within age at entry classes when the age at entry distribution and the age and term-specific risks of retirement and defeat are held constant at southern levels, the factor $l_{k,d}^s(x)$ is substituted for $l_{k,d}^n(x)$ in expression (8). The mortality adjusted distribution with age at entry controlled is, therefore,

$$S^M_k = S_1 \int A^n(x) \cdot l^n_{k,d}(x) \cdot l^s_{k,r}(x) dx. \quad (9)$$

The effects of variation in the age at entry distribution are measured by substituting the northern weights, $A^n(x)$, for the southern weights in expression (8). The age at entry-adjusted seniority distribution

$$S^A_k = S_1 \int A^n(x) \cdot l^n_{k,d}(x) \cdot l^s_{k,r}(x) dx, \quad (10)$$

gives the number of southern Democrats expected in Congress at the k^{th} term if the Southerners had the age at entry distribution of Northerners and the risks of dying in office and of leaving Congress for other reasons were held constant at southern levels within age at entry classes.

An adjusted distribution incorporating the joint effects of variation in mortality and age at entry is obtained by substituting both $A^n(x)$ and $l^n_{k,d}(x)$ for the corresponding functions in (8). The result,

$$S^*_k = S_1 A^n(x) \cdot l^n_{k,d}(x) \cdot l^s_{k,r}(x) dx, \quad (11)$$

is analogous to the adjusted distribution of Model I. It gives the expected number of Southerners in office at the k^{th} term if the Southerners had the northern age at entry distribution and the northern age at entry-specific risks of dying in office, but the risks of leaving office for other reasons were held constant at southern levels within age at entry classes.

The evaluation of S^M_k , S^A_k , and S^*_k is accomplished by summation of products of the

age at entry proportions and the survivorship factors over four broad age at entry classes: 25 to 34, 35 to 44, 45 to 54, and 55 and older. The values of these adjusted distributions are shown in columns three, four, and five of Table 4. The percentages by which each of the adjusted distributions reduce the observed southern distribution are shown in columns six, seven, and eight of the table.

The results indicate that the low rates of mortality among southern congressmen contributed quite substantially to southern numbers in the higher order terms and that this holds net of differences in the regional age at entry distributions. The percentage reduction in southern numbers obtained by adjusting mortality to northern levels but holding age at entry constant is remarkably similar to the total reduction obtained in Model I where the effects of age at entry were included with the mortality effects. The reductions shown in column six are only slightly smaller than those given by Model I for the second through the ninth term and values even larger than those generated by Model I are obtained in the tenth and eleventh terms.

The southern age at entry distribution also contributed favorably to their seniority. The results in column seven show that in all terms but the seventh and eighth the substitution of the northern age at entry proportions reduces southern numbers. This is to

Table 4. Observed and Expected Seniority Distributions with Mortality Risks and Other Risks Dependent on Age at Entry

Term	Observed Numbers		Expected Numbers of Southerners			Percentage Reduction Due to Adjustment		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	North	South	Mortality Adjusted	Age at Entry Adjusted	Both Adjusted	Mortality Adjusted	Age at Entry Adjusted	Both Adjusted
1	300	103	103	103	103	--	--	--
2	214	88	87.9	87.5	87.4	0.1	0.6	0.7
3	155	72	70.4	70.0	68.0	2.7	2.7	5.6
4	109	61	56.4	60.0	54.7	7.6	1.6	10.4
5	76	45	43.6	43.8	42.8	3.0	2.6	4.8
6	45	38	35.1	37.9	34.8	7.7	0.3	8.4
7	28	33	29.4	33.1	29.3	11.0	-0.4	11.2
8	19	27	21.0	27.0	21.0	22.0	0.0	22.3
9	14	22	16.9	20.6	15.6	23.1	6.5	29.3
10	11	19	10.9	17.4	9.7	42.7	8.6	49.0
11	9	18	10.2	16.1	8.7	43.4	10.4	51.4

be expected since the northern age at entry distribution is somewhat older than the southern distribution. It is to be noted, however, that the effects of age at entry are considerably smaller than the direct effects of mortality. At every term from the sixth through the eleventh the reduction obtained by adjusting mortality exceeds that obtained by adjusting age at entry by a ratio greater than four-to-one.

The percent reduction achieved by substituting both the northern age at entry proportions and the northern mortality risks for the corresponding southern functions is shown in column eight of Table IV. It is interesting to note that here the joint effects generally produce larger reductions in southern numbers than those given by Model I which also combines age at entry and mortality effects. This is because the southern age classes least affected by attrition through retirement and defeat are also the age classes most sharply favored in life chances. Southerners who entered the House between the ages of thirty-five and fifty-four suffered smaller losses through retirement and defeat than those who entered at younger and older ages, and the thirty-five to fifty-four age class also exhibits the sharpest regional mortality differential. This selection effect could not be detected by Model I which does not explicitly incorporate age at entry as a variable. The results indicate that if Southerners entering the House had the same age distribution as entering Northerners and that if Southerners experienced the mortality of Northerners within age at entry classes, the numbers of Southerners remaining in Congress past the seventh term would be considerably smaller than they in fact were. Reductions of between 22.3% and 51.4% in southern numbers are obtained in the eighth through eleventh terms with this adjustment. The adjustment reduces the strong observed southern margin after the seventh term to a thin edge in the seventh through the ninth terms and reverses it in the tenth and eleventh terms.

A PERIOD INTERPRETATION

The preceding analysis dealt with a cohort of individuals who entered Congress at different points over a span of eight years.

The length of term distribution referred to no specific Congress because it aggregated experiences in several Congresses. Thus, the "first term" referred to above was composed of events in all first terms in the Seventy-second through the Seventy-sixth Congress, or all first terms starting with the convening of a Congress in the years 1931 to 1939. The "second term" referred to all second terms starting in the years 1933 to 1941, and so forth. It is fair to ask how the effects measured here affected particular years in time. For the year 1939, the individuals in the cohort include all members of Congress beginning a first through a fifth term; for the year 1941, they include all persons starting a second through a sixth term; and, for the year 1959, they include all persons starting an eleventh through a fifteenth term.

Table 5 shows the numbers of northern and southern Democrats in the cohort who stayed on to each new Congress. It also shows the numbers of Southerners who would have remained if the Southerners had experienced the same annual mortality rates as Northerners. In this case the rates of mortality and of decrement due to other causes depend on years in time rather than terms in Congress. The results in Table 5 are consistent with those shown in Tables 3 and 4. The South gained substantially from its low mortality and youthful age distribution in every Congress from the Eightieth forward.

SUMMARY

Southern power in the U. S. House of Representatives derives from the numerical dominance of Southerners in the senior membership of the House Democratic caucus. This numerical advantage is a result of low turnover: southern Democrats who enter the House generally hold their seats longer than Democrats entering from the North. The difference in turnover rates is sufficiently great to fashion a substantial majority of Southerners at higher order terms out of an initial numerical disadvantage of three to one. The basic factor in southern tenacity is low attrition rates at the polls. However, the relatively low mortality rates of southern Democrats is

Table 5. Numbers of Northern and Southern Democrats of Those Entering the House 1931 to 1939 Who Started each New Congress from the 77th to the 87th and Expected Numbers of Southern Democrats Starting when Mortality is Adjusted

Congress	Year Convened	Terms Represented	Numbers Starting Congress			
			North Observed	South Observed	South Adjusted	Percentage Reduction
77th	1941	2nd to 6th	109	57	57.3	--
78th	1943	3rd to 7th	53	48	46.8	2.5
79th	1945	4th to 8th	38	42	37.9	9.8
80th	1947	5th to 9th	24	32	27.2	15.0
81st	1949	6th to 10th	18	25	19.2	27.2
82nd	1951	7th to 11th	15	22	16.0	36.4
83rd	1953	8th to 12th	10	19	13.8	27.4
84th	1955	9th to 13th	9	17	13.0	23.5
85th	1957	10th to 14th	7	17	11.6	31.8
86th	1959	11th to 15th	6	14	8.2	41.4
87th	1961	12th to 16th	6	10	5.8	42.0

an important supplement to their electoral advantage. The edge which southern Democrats hold at the higher order terms would be substantially smaller if the Southerners faced the same risks of dying in office as their northern colleagues. Southern Democrats entering the House are somewhat younger than the Democrats entering from the North and this contributes to their relatively low death rates. However, the major factor in the low death rates of Southerners is their low age-specific mortality and not their youthful age composition. Southern Democrats in Congress are evidently healthier than northern Democrats.

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APPENDIX

Estimation of $P_i(\bar{D}|\bar{R})$ and $P_i(\bar{R}|\bar{D})$

The term $P_i(\bar{D}|\bar{R})$ is defined as the probability that a person in office at the start of the i^{th} term will not be removed from office by death prior to the start of the $i+1^{\text{th}}$ term, given that death is the only risk to tenure operating in the interval $(i, i+1)$. $P_i(\bar{R}|\bar{D})$ is the probability he will not be removed from office by some other event, given that death does not operate as a risk. The procedures used for estimating $P_i(\bar{D}|\bar{R})$ and $P_i(\bar{R}|\bar{D})$ depend on an idealization of the interval $(i, i+1)$. The interval is divided into three segments: one covering the period from the start of the i^{th} term to a point just before primary elections for the $i+1^{\text{th}}$ term; a second covering the election period; and a third which begins immediately after the general election and ends with the convening of the $i+1^{\text{th}}$ term. All events except death which prevent succession to a $i+1^{\text{th}}$ term are assumed to occur in the second segment. These events include failure to file for election (a kind of retirement), defeat in a primary election, and defeat in a general election, all of which may be realistically assigned to the second segment. However, resignations which occur before or after the election period are also assigned to the second segment. Deaths occurring before the primary election are assigned to the first segment and deaths after the election period are assigned to the third segment. Those few deaths which

occurred in the interval between the primary and general election were assigned one-half each to the first and third segments. Estimates derived from this model differ little from those generated by more detailed models of the process.

Under this model the overall probability that an individual starting an i^{th} term will succeed to an $i+1^{\text{th}}$ term, $P_i(\bar{D} \cap \bar{R})$, is the product of three probabilities, namely:

- (a) the probability of surviving the first segment,
- (b) the probability of surviving the second segment given that the first segment is survived, and
- (c) the probability of surviving the third segment, given that the first and the second segments are survived.

Each of these terms may be estimated directly from the corresponding observed proportions in the data.

The factor $P_i(\bar{D}|\bar{R})$ is estimated by setting the probability governing the second segment, or the probability of averting events other than death at 1.0. It is, therefore, taken as the product of (a), the proportion surviving the first segment, and (c), the proportion surviving the third segment of those who survive the first and second segments. The factor $P_i(\bar{R}|\bar{D})$ is estimated by setting the probabilities governing the first and third intervals at 1.0. The estimate, then, is simply (b), the proportion surviving the second interval of those who survive the first.

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EFFECTS OF VERTICAL MOBILITY AND STATUS INCONSISTENCY: A BODY OF NEGATIVE EVIDENCE *

ELTON F. JACKSON
Indiana University

RICHARD F. CURTIS
University of Arizona

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This study attempts to determine whether dimensions of social rank combine additively in influencing individual attitudes and behaviors, or whether statistical interactions appear which would support hypotheses of vertical mobility or status inconsistency effects. Samples of male heads of household were interviewed in three Indiana and three Arizona communities, roughly matched in size. Two forms of mobility (career and intergenerational) were employed and six forms of inconsistency (including achieved vs. ascribed forms). Forty-three dependent variables were used, including most of the variables previously suggested as consequences of mobility or inconsistency. Most of the relationships appeared to be additive. The interactions which did appear were not clustered with respect to any particular independent or dependent variables, usually varied in form from city to city, and did not resemble patterns expected on the basis of mobility or inconsistency theory. The findings suggest that multidimensional additive models adequately represent the effects of social stratification on the individual.

IN the past twenty-five years, many sociologists have maintained that vertical mobility and/or status inconsistency have important, often adverse, influences on the individual, in addition to any effects of social rank. However, evidence from past research has not been sufficient to establish that such effects exist. This is partly because most past studies have used only one or a few forms of mobility or inconsistency, related them at most to a small number of dependent variables, and, sometimes, used limited samples. This paper analyzes the effects of two forms of mobility and six forms of inconsistency on a large number of dependent variables, using samples from six U.S. communities.

Although mobility and inconsistency are conceptually distinct experiences (and most past research has dealt with either one or the other), for several reasons it is appropriate to consider them both in the same overall analysis. Both kinds of experiences have been thought to affect the individual through similar intervening mechanisms; for

example, disruption of interpersonal relations and conflicting or ambiguous expectations for behavior. Also, mobility and inconsistency have been thought to produce similar kinds of responses, such as prejudice and withdrawal from social participation (see, for example, Blau, 1956; and Lenski, 1956). Finally, both experiences are necessarily defined by the possession of two (or more) different rank positions (held either sequentially or simultaneously). Hence, both are constructed from prior concepts, either from early and later ranks such as current occupation and first occupation, or from separate ranks such as income and education. This situation creates a fundamental theoretical and analytical problem.

Theoretically, a redundancy is created: the same ideas have two sets of names, a condition which does not increase their power to explain. In mobility theory, for example, degree of prejudice or some other response could be explained by socialization in an origin stratum along with current experiences in a destination stratum (two ranks on a given dimension) or *equally well* by the experience of moving from the first stratum to the second (mobility). But in fact, these are the same: one cannot move without experiencing one stratum and then the other, and one cannot experience one stratum and then the other without moving. The empirical

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events referred to by the term "mobility" are the same as the events referred to by "stratum of origin and stratum of destination."

The essential difference between "mobility" theories and "multidimensional rank" theories—when they differ—is that they posit different mechanisms to account for the effects of stratification on individual behavior. For example, a "mobility" theory might specify hindered interaction in primary groups as the intervening mechanism; while a "multidimensional" theory might be based on processes of socialization and resocialization (for an illustration of such specifications, see Jackman, 1972). Without such specifications, the apparently competing theories cannot be conceptually distinguished. The contrast between mobility or inconsistency theory on the one hand and multidimensional rank theory on the other only arises when they specify different intervening mechanisms.

In data analysis, this theoretical redundancy takes the shape of an "identification problem": the empirical data are equally consistent with several explanatory models, and so cannot be used to choose among them. For example, if downwardly mobile people are more intolerant than stable people, it may be difficult or impossible to attribute this difference to the process of moving rather than to the combined effects of father's and current occupation acting additively on the individual (see Blalock, 1966; Duncan, 1966). The problem is essentially definitional: mobility (or inconsistency) is defined in terms of prior rank variables; hence, one cannot ascertain its effect while holding constant its constituent rank variables. In many cases, then, the data can be equally well explained by simple additive models of rank effects, models of mobility or inconsistency effects, or models combining additive and nonadditive effects (see Hodge and Siegel, 1970; and, for an illustration, Hodge, 1970).

The best way to handle the identification problem, as Blalock (1966) observed, is to introduce additional exogenous or intervening variables into the analysis. If these added variables have the proper pattern of causal links to the original variables, it will be possible to distinguish definitely between

the hitherto equally-well-fitting models. Such additional variables, however, have proved difficult to specify theoretically and measure empirically.

A more common approach, and the one we shall use here, is to compare the explanatory power of a simple additive model of rank effects with that of more complicated models (including mobility and inconsistency models). If the more complicated models do not explain significantly more variance, we provisionally assume that the additive model accurately represents how social status affects the dependent variable in question. Essentially, in this approach, we are accepting the more parsimonious of available, equally adequate models.

DATA AND METHODS

The data for the analysis come from random samples of male heads of households in six American communities, interviewed in 1966 and 1967. The communities were selected in pairs, one each from Indiana and Arizona, roughly matched in size. Indianapolis and Phoenix had about 700,000 residents at the time of the survey; Columbus and Yuma about 25,000; and Linton and Safford about 5,000. Sample sizes are just under seven hundred in Phoenix and between three and four hundred in the other five cities.¹ Analyzing our problem in several rather different communities allows us to estimate whether mobility and/or inconsistency effects are more or less general, or whether they appear only in certain social settings. It also allows us to see which effects do not replicate across cities in any fashion and hence should possibly be labelled chance fluctuations.

The basic rank dimensions for this analysis are: occupation, father's occupation (while the respondent was growing up), first occupation, education, racial-ethnic rank, and personal income. Each rank variable was trichotomized in the analysis.²

¹ More details on the sampling and data collection are given in Artz, *et al.* (1971).

² The occupation variables were scored according to Duncan's Socioeconomic Index of Occupations (Duncan, 1961). Education was scored in years completed, and income in dollars. Racial-ethnic rank was scored by assigning Bogardus (1959) social-distance scores to the race or na-

The forty-three dependent variables include most of the variables which have been suggested as consequences of mobility and/or inconsistency (for example, political liberalism, withdrawal from social participation, prejudice). Several other variables were added on a more or less exploratory basis, even though in some cases the rationale for including them was less than compelling. Table 2 lists the dependent variables, grouped in categories.

In the analysis that follows, these dependent variables are treated as interval scales, though few (such as number of associational memberships) can actually be considered to be more than ordinal scales. (An addendum giving details on the measurement of each variable is available from the first author). In addition, seventeen (those starred) of the forty-three dependent variables are measured only as dichotomous scales. This introduces some inaccuracies in the significance tests for these variables (Goldberger, 1964:248-51). Our findings must therefore be regarded as somewhat tentative.

In the analysis, each test relates a dependent variable to a pair of trichotomized rank variables. This procedure yields a three-by-

three table, formed by cross-classifying the two rank variables, with a mean value for the dependent variable in each of the nine cells. Intergenerational and career mobility are defined by the off-diagonal cells in the tables in which respondent's occupation is cross-classified by father's occupation and by first occupation, respectively. Six forms of status inconsistency are defined by the off-diagonal cells in tables formed by cross-classifying the six possible pairs formed from respondent's occupation, income, education and racial-ethnic rank.³ Each dependent variable was thus run against eight pairs of rank variables (two forms of mobility and six forms of inconsistency) within each of six cities, yielding 2,604 tests.⁴

We want to determine, for each such three-by-three table, whether the two rank variables affect the dependent variable additively, or whether a more complex model (incorporating a mobility effect, for example) is required. That is, does each table display statistically significant interaction.

Our search for interaction effects uses a method suggested and demonstrated by Duncan (1966:90-5) and applied in several studies of mobility and inconsistency effects (e.g., Jackson and Burke, 1965; Hodge and Treiman, 1966; Treiman, 1966). We will first see how much variance in each dependent variable can be explained by a dummy-variable additive (non-linear) model of the form

tional origin of each respondent (scores for the Pacific West region were given to Arizona residents; scores for the North Middle region were given to respondents in Indiana).

The variables were trichotomized according to two somewhat contradictory guidelines: First, the "high" and "low" categories could not be too narrow, lest we end up with unacceptably low N's in the corner (High-Low and Low-High) cells. Second, they had to be narrow enough so that any respondents high on one rank and low on another would indeed be sharply mobile or inconsistent.

The three intervals selected for the occupational variables were: 0-29, 30-59, and 60+ on the Duncan scale. The low interval includes laborers, farmers (this is mainly relevant for father's occupation, since few of our respondents were farmers), most operatives and some craftsmen. The high interval includes most professionals, managers, and officials. Educational categories were: less than high school graduate, high school graduate, and more than high school graduate. Income categories were: 0-\$5500, \$5501-9500, and more than \$9500. Racial-ethnic categories were: 1.51+, 1.50-1.20, and 1.20 or less. The "Low" (most social distance) category includes men of south and east European ancestry, Jews, Indians, Mexican-Americans and Negroes. The "High" category includes men of Canadian, English and (white) "American" ancestry.

³ Rank dimensions were considered two at a time because a simultaneous cross-classification of three or four rank variables would have resulted in too few cases in some cells. It is possible, however, that findings in our analysis which appear to be mobility or inconsistency effects might actually be due to the influence of a third, omitted rank variable. Thus our procedure is somewhat biased against the null hypothesis and in favor of finding apparent mobility and inconsistency effects.

⁴ In analyzing some of the dependent variables, subsamples had to be omitted owing to the nature of the variable. The omitted variables and subsamples (given by row in Table 2) are:

- A2, A3 and B5: Men who did not belong to a formal association.
- B2: Men with no relatives in the community area.
- B6: Men who had not become friends with association co-members.
- B7: Men without co-workers.
- E2: Negroes.

$$\hat{D} = a + \sum_{i=1}^2 b_{1i}R_{1i} + \sum_{j=1}^2 b_{2j}R_{2j}$$

where D = the dependent variable score, $R_{11} = 1$ if the person is high on the first rank variable and 0 otherwise, $R_{12} = 1$ if the person holds medium rank on the first rank variable and 0 otherwise, $R_{21} = 1$ if the person holds high rank on the second rank variable and 0 otherwise, and $R_{22} = 1$ if the person holds medium rank on the second rank variable and 0 otherwise. We then compare this model's explanation with that afforded by the nine subcells formed by cross-classifying the two rank trichotomies; that is, we test for the statistical significance of interaction.

If interaction is not significant, that is, if a more complicated, non-additive model does not significantly improve our explanation of the dependent variable, we will conclude, not that the hypothesis of a mobility and/or inconsistency effect is disconfirmed or rejected, but that the data suggest that such an hypothesis is unnecessary.

What if statistical interaction is present? Blalock (1967a, 1967b), while granting that statistical interaction may help uncover inconsistency or mobility effects, has argued cogently that the presence of interaction is not sufficient proof of such effects. That is, statistical interaction may appear for many reasons besides the existence of mobility or inconsistency effects. (Blalock also shows that it may be difficult or impossible to determine the *form* of such effects, even if their existence is established).

For this reason, we shall regard the number of statistically significant interactions as a "ceiling" estimate of the extent to which mobility and inconsistency effects may be at work. Such an estimate is useful in and of itself. We shall also attempt to go beyond this estimate and tentatively identify which instances of interaction could plausibly be due to mobility or inconsistency effects. We will first examine the ways in which the observed means for mobiles and inconsistent depart from values predicted by the additive model, to see whether the departures resemble those that a model of inconsistency or mobility effects would reasonably suggest. For example, if statistical interaction

appears when respondent's first and current occupations are related to an index of intolerance, a mobility effect would be suggested if one of the main departures indicated that downwardly mobile men were more intolerant than predicted by the additive model. Similarly, we would expect observed levels of psychosomatic symptoms to be higher than predicted for men inconsistent on, for example, occupation and racial-ethnic rank.

We shall also check to see if such patterns recur across cities. We cannot necessarily expect effects to hold for all six cities, since some kinds of effects may only occur in certain community contexts (large cities, for example), but we should expect genuine effects to display some regularity. In sum, if interaction effects appear which are not interpretable as mobility or inconsistency effects, or if the latter sorts of effects appear in a scattered fashion, now in one city, now another, we shall again conclude that there is no reason to posit the existence of mobility or inconsistency effects.⁵

Therefore, our general theoretical goal, namely the attempt to separate mobility or inconsistency effects from multidimensional rank effects in order to attack the issue of how social stratification (broadly conceived) affects individual behavior, has been narrowed to a very specific question for analysis: are simple additive multidimensional models of rank effects generally adequate, or does the existence of statistically significant, interpretable and replicable interaction effects indicate the need for status inconsistency or vertical mobility explanations?

To illustrate our analysis we show, in Table 1, six tests which relate occupation-income inconsistency to social class identi-

⁵ However, we will *not* require that any observed mobility or inconsistency effect explain a large additional amount of the variance. Such a criterion might lead us to dismiss real and important effects. For example, suppose that sharp mobility and/or inconsistency had quite powerful effects on individuals, over and above the basic main effects of the rank dimensions involved. The amount of explained variance added by such mobility or inconsistency effects might well be quite small, since relatively few people are sharply mobile or inconsistent. Hence our basic criterion will be statistical significance, not a large gain in explaining total variance over the whole sample.

Table 1. (High) Social Class Identification by Occupation and Income, in Six Cities

Occupation	High (N)	Medium (N)	Low (N)		High (N)	Medium (N)	Low(N)		
Indianapolis Income					Phoenix Income				
High	4.65 (51)	3.97 (36)	4.00 (18)		4.48 (106)	3.90 (87)	4.06 (31)		
Medium	3.56 (18)	3.19 (42)	2.71 (14)		4.04 (47)	3.42 (107)	3.19 (75)		
Low	2.71 (7)	2.57 (54)	2.43 (56)		2.33 (9)	2.70 (63)	2.70 (116)		
Source	% Variance	F	df	p	% Variance	F	df	p	
Between subcell	34.4%	18.8	8	<.05	22.6%	23.1	8	<.05	
Additive model	33.9	37.3	4	<.05	21.7	44.1	4	<.05	
Interaction	0.5	0.5	4	NS	0.9	1.9	4	NS	
Columbus Income					Yuma Income				
High	4.60 (55)	4.16 (43)	3.85 (13)		4.74 (34)	3.88 (33)	3.43 (14)		
Medium	4.41 (22)	3.40 (42)	3.23 (22)		3.61 (36)	3.29 (63)	3.16 (32)		
Low	3.43 (14)	2.78 (51)	2.54 (37)		3.69 (16)	2.56 (52)	2.51 (72)		
Source	% Variance	F	df	p	% Variance	F	df	p	
Between subcell	30.6%	16.0	8	<.05	23.6%	13.2	8	<.05	
Additive model	30.0	31.5	4	<.05	22.4	25.0	4	<.05	
Interaction	0.6	0.6	4	NS	1.2	1.3	4	NS	
Linton Income					Safford Income				
High	4.81 (16)	3.80 (30)	3.36 (11)		4.37 (27)	4.07 (27)	3.40 (10)		
Medium	3.32 (19)	2.96 (27)	3.26 (34)		3.81 (26)	3.71 (34)	3.00 (37)		
Low	3.42 (12)	2.72 (32)	2.64 (83)		4.82 (11)	2.89 (35)	2.52 (79)		
Source	% Variance	F	df	p	% Variance	F	df	p	
Between subcell	18.2%	7.1	8	<.05	26.8%	12.7	8	<.05	
Additive model	16.4	12.7	4	<.05	23.0	21.0	4	<.05	
Interaction	1.8	1.4	4	NS	3.8	3.6	4	<.05	

fication, within each of six communities. In each table, the joint effects of the two rank variables can be seen by inspecting the mean scores on class identification within the nine cells. In the analysis of variance reported below each table, three tests are made. The between-subcells test establishes whether any effects of any sort occurred or whether the differences between the nine cell means could have been due to chance (the .05 level is used throughout). The next test is for the significance of the additive model of rank effects. The third test (for interaction)

indicates whether the excess of variance accounted for by the nine subcell means over the additive model is greater than a random model would suggest. In this case, only one interaction test, in the city of Safford, was significant. The observed means and their deviations from the predicted values (not shown) suggest that in Safford men of high income and low occupational rank hold higher class identifications than would be expected on the basis of the additive effects of occupation and income. However, this finding did not appear in any of the other

five communities, so we can have little confidence that this effect actually exists.

RESULTS

Of the 2,064 tests, 1,075, or 52%, exhibited statistically significant differences between the subcell means. It is within this subset of tables, where there appears to be some sort of combined or independent effect of one or both rank variables, that we must ask whether the additive model of rank effects is adequate. Of the 1,075 tests of the significance of interaction performed on these tables, 123, or 11.4%, were statistically significant.⁶

Therefore, in those situations where rank did appear to have some effect, eight out of nine times a non-additive model did not contribute beyond chance levels to the variance explained. Moreover, the percentage of *true* interaction effects is probably smaller than 11.4%, since in a large number of tests some statistically significant results will occur solely by chance. And since the number of mobility and inconsistency effects is probably smaller than the number of true interaction effects, one could conclude at this point that mobility or inconsistency explanations are, at best, seldom required.

Still, it may well be that only certain kinds of mobility or inconsistency affect only certain specific dependent variables. It is, after all, theoretically implausible that all or most of a large variety of variables should be related to all or most forms of mobility or inconsistency. Such specific and restricted effects would account for the small total number of significant interactions, but would imply that those that appear should be concentrated in a few subsets of the 1,075 examined tables. To investigate this possibility, we show, in Table 2, for each of the

$8 \times 43 = 344$ combinations of rank-pairs and dependent variables, the proportion of the six community samples with significant between-subcell differences in which significant interactions also appeared. For example, the results in Table 1 are translated into the proportion 1/6 in column 8 and row I2 of Table 2, meaning that when social class identification was run against occupation and income, the between-subcell effect was significant in all six cities, but in only one city was the interaction term significant. High ratios, then, should suggest the existence of specific mobility or inconsistency effects.

Looking first at the column totals in Table 2, we see that the significant interactions are not disproportionately frequent for any one type of mobility or inconsistency—percentages of significant interactions range from 9.2 to 15.3%. A similar finding appears with respect to the dependent variables. In only eight of the forty-three rows does the total percentage equal or exceed 25%, and these typically are dependent variables in which the between-subcells differences were not significant in many instances. For example, the two largest row percentages, both 60% (in rows B2 and J3), are both based on only five instances of a significant between-subcells effect out of a possible forty-eight (six cities $\times$ eight types of mobility or inconsistency). The highest of these eight row bases is thirteen. Such results, showing that interaction effects are only common in instances in which rank effects of any sort are exceedingly sporadic, can lend little support to a theory of mobility or inconsistency effects. Finally, a grouping by city totals (not shown in Table 2) yields similar findings: the six percentages of significant interactions range from 5.8% to 14.7%, indicating no particular tendency for these effects to concentrate in a given community.

These results, however, are still not conclusive. Even if interaction effects seldom appear in the group of analyses involving a given dependent variable, or a given type of inconsistency or mobility, it still may well be that particular types of mobility or inconsistency only affect particular dependent variables. If so, certain *cells* of Table 2 should contain high proportions of sig-

⁶ The figure 11.4% is not to be interpreted simply. In a number of completely independent tests 5% will be significant at the .05 level, if no true effects whatsoever exist. However, the per cent of the 1,075 tests which we would expect to show statistical significance in the total absence of any true interaction effects cannot be figured so readily, since these tests are not independent of each other, except across cities. The tests within a given city share a common population, and for many subsets the dependent variable or the independent variables are repeated.

Table 2. Frequency of Statistically Significant Interaction Terms in Relating Pairs of Stratification Variables to Dependent Variables, in Six Cities.

Dependent Variables	Number of Significant (p < .05) Interaction Terms/ Number of Significant Between-Subcell-Mean Terms (in Six Cities)									Total (and Percent) Significant
	Pairs of Stratification Variables									
	FO xO	1stO xO	R-E xEd	R-E xO	R-E xInc	Ed xO	Edx Inc	Ox Inc		
A Formal Social Participation										
1. Number of Associational Memberships	1/6	1/6	0/6	0/6	0/6	0/6	1/6	0/6	3/48 (6.3)	
2. Infrequency of Attending Most Imp't Ass'n	0/2	0/1	0/2	0/1	0/1	1/3	0/3	2/2	3/15 (20.0)	
*3. Held Office in Most Important Association	0/5	3/4	0/1	0/4	0/2	0/5	0/2	0/6	0/29 (0.0)	
*4. Church Membership	0/3	3/3	0/4	1/4	1/2	0/5	2/3	0/3	4/27 (14.8)	
5. Political Activity Index	0/6	0/5	1/5	0/6	0/5	1/6	1/6	1/6	4/45 (8.9)	
B Informal Social Participation										
*1. Relatives in the Community Area	2/4	0/2	1/3	0/3	0/2	0/2	0/2	0/3	3/21 (14.3)	
2. Infrequency of Visiting with Relatives	1/3	1/1	0/0	0/0	0/0	0/0	1/1	0/0	1/5 (60.0)	
3. Infrequency of Visiting with Friends	0/3	2/3	0/2	0/3	0/0	1/4	0/2	0/1	3/18 (16.7)	
4. Rather Visit Relatives than Friends	1/4	0/5	0/5	0/3	0/2	0/6	2/5	0/6	3/36 (8.3)	
*5. Became Friends with Co-members of Association	0/1	2/2	1/1	1/2	0/0	1/1	0/1	0/1	5/9 (55.6)	
6. Infrequency of Visiting with Co-members	0/0	0/0	0/1	0/2	1/2	0/0	0/0	1/2	2/7 (28.6)	
7. Doesn't Get Along with Work Associates	0/1	C/2	0/0	0/2	0/1	1/2	0/0	0/1	1/9 (11.1)	
*8. Have Someone to Turn to in Need	0/0	C/1	1/2	1/3	1/5	0/0	0/2	0/3	3/16 (18.8)	
C Political Liberalism										
*1. Changes Are Needed	0/2	0/2	0/4	1/4	1/4	0/4	0/3	0/4	2/27 (7.4)	
2. Domestic Liberalism Index	1/6	0/6	1/6	0/6	0/5	0/6	1/6	2/6	5/47 (10.6)	
3. Considers Himself a Conservative	1/2	0/0	0/3	0/1	0/2	0/2	0/2	0/1	1/13 (7.7)	
4. Democratic Party Preference	0/5	0/5	1/5	0/5	0/5	0/5	0/5	0/4	1/39 (2.6)	
D Satisfaction and Symptoms of Stress										
1. Satisfaction with Job	0/3	0/1	0/2	0/2	0/2	1/4	2/3	1/3	4/20 (20.0)	
2. Satisfaction with Income	1/1	1/1	0/1	0/2	0/6	0/0	2/5	1/5	5/21 (23.8)	
3. Satisfaction with Success	0/1	1/2	0/0	0/0	0/1	0/1	1/3	0/2	2/10 (20.0)	
*4. Satisfaction with Enjoyment in Life	0/0	0/2	0/1	0/2	0/1	0/1	0/0	0/1	0/8 (0.0)	
5. Psychophysiological Symptoms Index	1/2	0/1	0/2	0/2	2/5	0/1	1/4	1/5	5/22 (22.7)	
E Intolerance										
*1. Negroes are Undesirable Neighbors	0/0	0/0	1/3	1/3	0/3	1/2	1/1	1/1	5/13 (38.5)	
2. Disapproval of Negroes as Neighbors	0/0	0/0	1/3	2/3	1/2	1/1	0/1	0/1	5/11 (45.5)	
3. Index of Punitiveness Against Deviants	0/5	0/6	1/6	0/5	1/6	1/6	1/6	1/6	5/46 (10.9)	
4. Index of Adherence to Conventional Norms	1/6	0/4	1/5	3/5	1/5	0/6	0/5	0/6	6/42 (14.3)	
5. Dogmatism Index	0/6	0/6	0/6	1/5	0/4	1/6	1/6	1/6	4/45 (8.9)	
F Anomia										
1. Anomia Index	0/6	0/6	0/6	0/6	0/6	0/6	1/6	1/6	2/48 (4.2)	
2. Political Potency Index	0/3	0/5	0/4	0/4	0/4	1/4	1/5	1/4	3/33 (9.1)	
G Aspirations for Son										
1. Disappointed if Son were Machine Operative	0/4	0/6	0/6	0/4	1/5	1/6	0/6	0/5	2/42 (4.8)	
*2. Wants Son to Have a Better Job Than R Does	0/5	0/5	1/4	1/5	0/3	0/5	0/3	0/5	2/35 (5.7)	
*3. Wants Son to Be More Successful Than R Is	0/0	1/3	0/2	3/0	0/2	1/2	1/3	0/1	3/13 (23.1)	

nificant effects, even if the total proportions are low.

Scanning the cells tells us that *little or no evidence appears for a variety of mobility or inconsistency effects previously hypothe-*

sized in the literature. For example, domestic political liberalism (row C 2) does not seem to be related to any of the forms of status inconsistency. Although the subcell means almost always significantly differ for

Table 2 (cont.). Frequency of Statistically Significant Interaction Terms in Relating Pairs of Stratification Variables to Dependent Variables, in Six Cities.

Dependent Variables	Number of Significant (p < .05) Interaction Terms/ Number of Significant Between-Subcell-Mean Terms (in Six Cities)								
	Pairs of Stratification Variables								Total (and Percent) Significant
	FO xO	1stO xO	R-E xE	R-E xO	R-E xInc	Ed xO	Edx Inc	Ox Inc	
H Leisure Activities									
*1. Engages in Active Sports	0/2	1/2	0/2	0/1	0/2	0/2	0/3	0/3	1/17 (5.9)
*2. Engages in Passive Activities	0/0	0/0	0/0	1/1	0/1	0/0	0/1	0/2	1/5 (20.0)
I Self-Perceptions									
1. Powerless in the Community	0/3	1/5	0/4	0/3	0/5	0/6	1/6	0/5	2/37 (5.4)
2. (High) Social Class Identification	0/6	0/6	0/6	0/6	1/6	0/6	0/6	1/6	2/48 (4.2)
J Salience of Rank									
*1. Only Join an Ass'n Backed by Imp't People	0/4	0/5	0/6	0/3	0/3	0/5	0/6	0/3	0/35 (0.0)
2. Family Should Sacrifice to Get a Man Ahead	0/1	0/2	0/1	0/1	1/2	0/3	0/2	0/2	1/14 (7.1)
*3. Important to Live in the Best Area	0/0	0/0	1/2	0/0	1/2	0/0	0/0	1/1	3/5 (60.0)
K Perceptions of Responsibility and Legitimacy									
*1. Perception of Being Treated Unfairly	0/0	0/1	0/1	1/2	1/2	1/1	0/2	1/1	4/10 (40.0)
*2. Perception of Blocked Opportunity	0/0	0/0	0/0	0/0	0/1	1/1	0/1	0/1	1/4 (25.0)
3. Responsible for Own Success or Failure	1/3	1/3	0/3	0/4	1/5	1/5	0/5	2/4	6/32 (18.8)
*4. Drive and Initiative are a Main Reason for Success in America	0/6	1/6	1/6	0/6	0/6	0/6	1/6	0/6	3/48 (6.3)
Total	11/120	13/126	13/132	14/130	15/134	16/143	22/144	19/146	123/1075
Percent Significant	9.2	10.3	9.8	10.8	11.2	11.2	15.3	13.0	(11.4)
Indexes: Summed Responses									
	0/5	0/6	1/6	0/6	0/5	2/6	0/6	1/6	4/46 (8.7)
*Alternative Responses									
	0/6	1/5	0/6	1/6	0/5	1/6	0/6	0/6	3/46 (6.5)

*Dichotomous scale

this variable; the additive model of rank effects is rarely rejected. A significant interaction effect appears, at most, two of six possible times and usually not at all. The results for the effects of inconsistency on psychosomatic symptoms of stress (D 5) are equally sparse. A third example concerns associational memberships (A 1). When relating this variable to the various forms of inconsistency and mobility, significant interactions appear even less often—the proportions are all 1/6 or 0/6. Finally, when relating punitiveness or dogmatism (E 3, 5)—which we take as rough indicators of intolerance—to mobility, the ratios of significant interaction effects are all 0/5 or 0/6.

What results do we find if we systematically search Table 2 for relationships, whether or not previously hypothesized? Among those instances in which the between-subcells effect was significant in at least three of the six cities, only seven ratios appear over 1/2: two ratios of 2/4, four of 2/3, and one of 3/5. Therefore, for only one combina-

tion of any of the eight forms of inconsistency or mobility with any of the forty-three dependent variables did a significant interaction occur in even three of six cities.⁷

Even when significant interaction terms are observed, they need not indicate a mobility or inconsistency effect. As discussed above, interaction can be due to many sorts of departures from additivity; only certain of the possible patterns of departures would suggest a mobility or inconsistency effect. To gauge the frequency of such patterns, we examined the differences between the ob-

⁷ In several of the tests described above, the number of cases in the corner (sharply mobile or inconsistent) cells is rather small. In order to check the possibility that more meaningful results might emerge if only those tables with somewhat larger N's were examined, we constructed a version of Table 2 for only those tests in which *both* corner cells had an N of ten or more. Although some of the details changed, the results, if anything, displayed fewer mobility or inconsistency effects. Significant interactions occurred in only 11.1% of the tables with significant between-subcell effects, and there were no discernable patterns or concentrations in the data.

served subcell means and those predicted from the additive model in all instances where the interaction term was significant.⁸ Usually the deviations were not especially concentrated in the mobile or inconsistent cells, but were scattered in other ways throughout the table.

Moreover, when departures from additivity did appear in a form suggesting mobility or inconsistency effects, the form did not replicate from city to city. As an illustration, Table 2 indicates that in relating the "Perception of being responsible for own success or failure" variable (K 3) to occupation and income, there was a significant rank effect in four of the six cities; and in two of these four (Linton and Yuma) the interaction term was significant. However, the results from the two cities are contradictory. In Linton both types of sharp income-occupation inconsistencies show *less* tendency to accept responsibility; in Yuma

both types are *more* likely to judge themselves accountable for their success or failure. If either pattern had shown up in one or two additional communities, we might suspect genuine inconsistency effects, shifting as community contexts change. But this did not happen, suggesting that only chance factors are at work.

In only one instance did an interpretable pattern of deviations recur even approximately from city to city. This occurred in the analysis of the Index of adherence to conventional norms (E 4). This variable was built from items measuring whether the respondent feels that occupants of certain positions (e.g., college professor, minister, elementary school teacher) are doing wrong if they engage in certain kinds of marginally deviant behavior (e.g., swearing, divorce, visiting a bar). It was one of the variables included for exploratory purposes. When we related it to racial-ethnic rank and occupation, a significant between-subcells effect occurred in five cities, and interaction was significant in three. The data for these three cities appear in Table 3.

The deviations of observed means from those predicted by the additive model assume the same theoretically plausible form in Yuma and Safford: People who have

Table 3. Index of Adherence to Conventional Norms by Racial-Ethnic Rank and Occupation, in Three Cities

Racial-Ethnic Rank	Linton Occupation			Yuma Occupation			Safford Occupation		
	High	Medium	Low	High	Medium	Low	High	Medium	Low
Number of Cases									
High	17	27	45	20	30	17	23	35	27
Medium	45	56	80	47	81	60	24	29	42
Low	3	4	4	12	17	61	6	13	41
Observed Means									
High	3.71	4.85	3.73	3.30	3.43	2.71	4.43	3.94	4.22
Medium	3.76	4.29	4.28	3.36	3.31	3.75	2.54	3.97	4.52
Low	4.00	5.50	2.75	3.17	3.29	4.46	2.33	4.38	4.78
Observed Minus Predicted Means									
High	+.02	+.40	-.25	+.22	+.33	-.84	+.92	-.23	-.48
Medium	-.03	-.26	+.20	+.06	-.02	-.02	-.59	+.18	+.21
Low	+.29	+1.03	-1.25	-.58	-.48	+.25	-1.17	+.23	+.10

⁸ Since the interaction terms for these tables were significant, the regression equation stating the additive main effects is somewhat biased. Since the amount explained by interaction, however, was typically very small (1 to 3%), the estimates would not be so far off that the deviations cannot profitably be used as a guide to the specific locations of the most important departures from additivity.

sharply inconsistent racial-ethnic and occupational ranks are less likely to condemn unconventional behavior than are consistent people. In Linton, the findings are not so clear, but the extremely small number of persons of low racial-ethnic rank in that city make interpretation difficult in any case. No replication from city to city can be seen in the effects of moderate inconsistency. For a final bit of evidence, in the three cities where interaction was not significant (including Indianapolis, where even the between-subcells effect was not significant), the deviations of observed from predicted means for the sharply inconsistent cells showed the same pattern as in Yuma and Safford, namely a tendency toward less adherence to conventional norms. In this one instance, then, we have a pattern of effects which is somewhat plausible and which replicates from city to city. Considering that this particular relationship was not hypothesized in advance, that this dependent variable did not seem to be related in our analysis to any other forms of status inconsistency or mobility, and that some such result is possible solely on a chance basis in 2,064 tests, this finding is probably best regarded as a trace result to be checked in future research.

One analysis remains to be examined. A plausible hypothesis is that a mobile or inconsistent person typically reacts to his situation by adopting simultaneously a *number* of attitudinal or behavioral responses. A second hypothesis is that mobile or inconsistent persons respond in different ways, depending on several contingencies, including the particular pattern of their inconsistency or mobility. In this second view, there are a number of alternative or functionally equivalent responses to mobility or inconsistency, any of which might suit a given person's situation. For example, Jackson (1962) suggested that certain patterns of inconsistency encourage the person to favor social change, while other patterns lead to the development of physical symptoms of stress.

Either of these hypotheses might explain the generally negative findings above. If the first hypothesis were true, mobiles or inconsistencies would not differ much from the gen-

eral population on any single dependent variable, but would differ in the number of responses exhibited at the same time by a given person. Under the second hypothesis, a single dependent variable would again fail to distinguish mobiles or inconsistencies; but they should differ from the rest of the population in possessing at least one of several possible responses.

To test these possibilities, we constructed two combined indices of responses, one to reflect each of the above hypotheses. For both indices, we combined the scores on four variables commonly thought to be related to mobility and/or inconsistency: domestic liberalism, psychophysiological symptoms, infrequency of visiting with friends, and punitiveness (taken as an indicator of intolerance). The first index, the "summed responses" index, was formed by giving each individual a score of zero on any component variable on which he was lower than one standard deviation below the grand mean, 2 if he was higher than one standard deviation above the mean, and 1 otherwise. A person's final score was then formed by summing his scores on the four components, so a man with several of these traits ended up with a high score. The second index, an "alternative responses" index, was scored by giving each person a 1 if he was more than one standard deviation above the grand mean on *any* of the four components; otherwise he was scored zero. These measures are somewhat rough-and-ready in construction, but they suffice for an approximate test of the above two hypotheses.

Neither of the above hypotheses is supported. The two indices were run against the eight forms of mobility and inconsistency within each of the six cities in the same manner as each of the forty-three individual dependent variables. The results are given in the last two rows of Table 2. Interaction effects appear for the summed responses index in only four of forty-six instances where the between-subcell sum of squares was significant; for the alternative responses index the overall ratio was three out of forty-six. Nor did the patterns of deviations for the few significant interactions appear to reflect mobility or inconsistency effects.

DISCUSSION

Our findings provide little support for theories of mobility and inconsistency effects as opposed to those of additive multidimensional rank effects. How do these negative results fit with past findings?

Since extensive citation and review of the past literature in these areas are available elsewhere,⁹ we will here simply draw some general conclusions about the evidence provided by past studies. (We restrict ourselves to survey studies of individual effects which employ U.S. samples).

The initial impression given by past literature is that findings are mixed and contradictory, some confirming the effects of mobility or inconsistency, others not. Several differences between the studies, however, help to explain and reconcile these differences in findings. First, in many past studies the identification problem in general, or the specific problem of controlling for the independent effects of the rank dimensions *per se*, have been ignored or handled inadequately (for example, see Jackson, 1962). The results of such studies can, unfortunately, be given little weight.

The studies which have tried to control for the constituent rank dimensions have, in general, used two different methods. One is the "regression" approach which we have used here; the other is one suggested by Lenski (1964), involving comparing percentage sums due to consistent and inconsistent (or stable and mobile) combinations of ranks. This "comparison-of-sums" method is probably less desirable, since it yields no estimate of the form of the best-fitting additive model and hence no estimate of the source of any observed statistical interaction. Further, many of the studies using this method have dichotomized their rank dimensions, permitting only gross measures of mobility and inconsistency. This suggests that past results using the comparison-of-sums method be regarded with somewhat more caution than results using the regression method of analysis. Since practically all past studies using the comparison-of-

sums method obtained at least some mobility or inconsistency effects, while most regression studies have not, this "methodological weighting" of the evidence inclines us to argue against such effects.

Another obvious reason for the past mixed results is that many different dependent variables have been used. There is certainly no reason to expect that mobility or inconsistency should affect every aspect of human attitudes and behavior; so some researchers may have obtained results, while others did not, because they focussed on the correct dependent variables. Studies of liberal political attitudes, identifications or behavior appear to be the prime example. Almost all of these studies (e.g., Lenski, 1967; Segal and Knoke, 1968) have found inconsistency or mobility effects, while most studies of other responses have not. Perhaps mobiles or inconsistencies differ from the rest of the population in political orientation while resembling them in most other ways.

Unfortunately, this difference between studies in choice of dependent variable is confounded with the difference in analysis method. Most studies on political liberalism have used the less sophisticated comparison-of-sums method, so the abundance of political effects in the literature may be due to true effects or to the method employed. Since in the present study we obtained almost no effects on the political variables we included, our results would favor the latter interpretation.¹⁰

¹⁰ Another difference worth noting is that most of the studies obtaining effects were done with samples including females, while the studies (like ours) which restricted their samples to males almost always concluded that additive models were adequate. A possible explanation for this finding is that, as Laumann and Segal (1971) suggest, mobility or inconsistency only affect persons highly concerned with or conscious of status and rank. If, in addition, women have markedly higher levels of status concern than men, this would suggest that studies of combined male-and-female samples would show mobility or inconsistency effects while studies of males alone would not. However, Laumann and Segal's data suggest that status inconsistency does not have effects on men high in status concern, a finding which tends to cast some doubt on this explanation. We are further skeptical because this sex difference in samples is also somewhat con-

⁹ See Broom and Jones (1970), Laumann and Segal (1971), Vorwaller (1970), and Kessin (1971).

Two past studies using the regression method did conclude that clearcut effects exist. They found that at least certain types of sharply inconsistent persons tend to develop psychosomatic symptoms (Jackson and Burke, 1965) and to support the Democratic Party (Segal and Knoke, 1971). But both results fail to replicate in the present study.

Combining our findings with those of past studies, then, we conclude that mobility and inconsistency effects almost always disappear when the main effects of rank dimensions are rigorously controlled. Multidimensional additive models seem adequate to represent rank effects in the U.S. today.

The most obvious theoretical interpretation of these results is that mobility and inconsistency *per se* do not affect individuals (stressfully or otherwise). This in turn leads one to question some of the assumptions from which such hypotheses were derived, for example, that different rank positions carry clearly different expectations, or that being subject to conflicting expectations is stressful for the individual (for one investigation bearing on this problem, see Segal, *et al.*, 1970).

Another possibility is that mobility and inconsistency effects do occur, but only under certain conditions or for certain subgroups. If these conditions are absent in the U.S., and/or if these subgroups are too small to affect the findings from a random sample, an additive model would adequately fit most bodies of U.S. data. For example, mobility or inconsistency effects perhaps only occur in a rigid, salient, and stable class structure (see Broom and Jones, 1970). In the U.S., rates of mobility and inconsistency are so high as to make them the modal experience. Some kinds of rank discrepancy, such as upward mobility, are also supported by the value structure. As a result, perhaps only persons who are extraordinarily mobile or inconsistent experience stress.¹¹ Such effects

founded with the difference in analysis methods. All of the studies done with the comparison-of-sums method were (so far as we can tell) done with combined male-and-female samples.

¹¹ Some evidence suggests this. Blau and Duncan (1967:388) and Kessin (1971), while concluding that additive models are generally adequate, did locate some effects of extreme mobility. Vorwallers (1970) data also contain traces of such effects.

would be too rare to detect in a study of the present kind.

Another alternative hypothesis (see Treiman, 1966) is that mobility and/or inconsistency, perhaps even in their moderate forms, do affect individuals, but that such persons quickly learn various techniques of resolution and adjustment to get around their difficulties. Any population at a given time would then consist mainly of persons who had never experienced such problems and persons who had already adjusted to them. Hence, only those few who had recently become mobile or inconsistent would show any effect.

Finally, we should remind the reader that our negative results, and the generally negative findings of the better-analyzed past studies, should be interpreted as disconfirming theories of mobility and inconsistency effects only to the extent that such theories imply non-additive patterns of rank effects. Data, like ours, showing additive relationships between rank dimensions and various dependent variables are consistent both with certain (sometimes rather implausible) models of mobility and inconsistency effects and with multidimensional rank models. Our conclusion is simply that in general in the U.S. today such mobility or inconsistency models need not be posited: somewhat simpler, more parsimonious models stating additive effects of rank dimensions appear to adequately represent the effects of stratification on the individual.

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A CONFIGURATIONAL APPROACH TO CONTINGENT CONSISTENCY IN THE ATTITUDE-BEHAVIOR RELATIONSHIP*

ALAN C. ACOCK

University of Southern California

MELVIN L. DEFLEUR

Washington State University

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Using 202 subjects, this study develops and tests a configurational approach to the contingent consistency concept of the attitude-behavior relationship. Its theoretical position is that neither attitude, nor social situational variables adequately predict behavior when treated separately. When combined in a linear model based on interaction between variables, behavioral predictions are improved considerably.

Attitude toward the legalization of marijuana was measured along with perception of the position of peers and family. At a later date subjects were observed in an experimental situation where they were asked to vote to legalize marijuana. A multiple regression solution showed that the independent effects of the three variables did not clearly predict behavior, their additive effects did somewhat better, and the interactions (configurations) led to the clearest predictions.

These results supported a theoretical orientation similar to field theory. It should be noted that only one attitude object was used and only two situational variables were considered along with attitude. Nonetheless, the results strongly suggest that a configurational approach promises to unravel the attitude-behavior inconsistency problem generated in recent years. Therefore, the approach deserves further development and testing as a specific model of the contingent consistency relationship between attitudes and action.

THE relationship between attitude and behavior has long been a central issue in social psychology. The earliest systematic use of the concept was in the work of Thomas and Znaniecki (1918) who viewed attitudes as the subject "par excellence" of social psychology. Following their lead the use of attitudes as a construct in social psychology became so widespread that by the mid-1930's Allport reported that the concept was the "primary building stone in the edifice of social psychology" (1935). According to a text of the period, "Perhaps no single concept . . . occupies a more nearly central position" (Murphy, *et al.*, 1937:889).

During these early decades, some writers

were very critical of the use of the concept, particularly with respect to its relationship to behavior. LaPierre (1938) for example, saw "attitude" as filling the vacuum created by the demise of "instinct." Noting that attitude had the advantage of a social origin, he still held the general view that attitudes were "cheap," but "irrelevant" (1934).

The controversy over the concept continues unabated. Some still feel that understanding attitudes is essential to understanding behavior. Others feel the concept is all but scientifically useless. Current research trends may help resolve these earlier conflicts. A profitable avenue to pursue may be to formulate models which elaborate the "contingent consistency" concept of the attitude-behavior relationship (Warner and DeFleur, 1969). The most significant feature of this orientation is that attitudes are no longer used as the sole basis for predicting behavior. Research has switched to examining configurations of attitude, or attitudes combined with other variables on which variations in overt behavior are contingent (DeFleur and Westie, 1958; Rokeach, 1968; Warner and DeFleur, 1969; Albrecht, 1970; Ajzen and Fishbein, 1970).

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The Configurational Approach: Growth and Dimensions

The need for viewing attitude configurations and other variables acting concurrently in behavioral prediction is a consequence of the dismal failure of attitude to predict behavior. For example, Wicker, after reviewing a great many studies, concluded that on average only about 10% of the variance in overt behavior on a given issue can be attributed solely to differences in attitude toward that issue (Wicker, 1969).

For many years, writers have suggested the need for combining attitude with other variables in some way in order to predict overt behavior. Sherif's early review of research in the area expressed this general idea (1949:37). Recognizing the importance of attitudes he noted that "cross pressures" (the influence of non-attitudinal social variables) also effected behavioral decisions related to attitude issues. Sherif argued that behavior depends not only on the individual's attitude, but also on his perception of the response of others' to his behavior.

DeFleur and Westie (1958), in an experimental study of the attitudes of white subjects toward blacks as related to overt racial discrimination, reached a similar conclusion. Nearly a third of their subjects' overt actions were inconsistent with their measured attitudes. In a follow-up interview with the inconsistent subjects, the problem was tentatively unraveled. Fully 75 percent of the inconsistent subjects explained their behavior by pointing to reference groups or persons whom they perceived as holding an attitude different from theirs. The subjects felt constrained by these perceptions. Several years later, Linn (1965), in a similar study, found that 59 percent of overt acts could not be predicted from attitude alone. He too challenged the inconsistent subjects with similar results. An excellent example was the student who said "those [measured attitudes] were my desires, but I couldn't do them. I had to think of my parents and of my home town" (1965:362). These studies clearly indicate that attitude is only one of several variables that must be considered in attempts to predict overt behavior. The fact that so many researchers have disregarded such other variables may explain Wicker's

findings that attitudes alone have dismally failed to predict behavior.

Viewed from this perspective, which is perhaps a modern statement of Durkheim's concept of "social facts," attitude emerges as merely one factor shaping behavior in real situations. Other factors are the numerous situational variables—that is, the psychological, social, and cultural influences in the situation of action—that play a part in guiding overt behavior decisions. There is no reason to expect any single variable to predict actual behavior; there is every reason to expect combinations of variables to provide such predictions.

The attitude-behavior process, in other words, is a multivariate relationship. Weissberg has expressed the idea in the following terms:

An attitude, no matter how conceived, is simply one of the terms in the *complex regression equation* we use to predict behavior; we cannot expect it to do much. . . . The embarrassing thing is that we have not systematically investigated these other sources of influence on overt behavior and not that we are unable to predict overt behavior solely from the predisposition (Weissberg, 1965:424, italics added).

In summary, these comments and studies strongly suggest that the key to understanding behavior *via* attitudes lies in discovering interrelated configurations of attitude and social or cultural variables that accurately predict action. It is quite likely that the independent effect on the behavior under study of each variable in the configuration may be small. Thus, the key is neither a person's attitude, *nor* such social influences as his perception of the opinions of certain reference groups, when these variables are allowed to operate separately, but the joint operation of such variables. The joint operation may be expressed by adding the independent effects and the interactions (if any) of the variables operating simultaneously.

The idea of looking at configurations (or interactions of variables) is certainly not new. Many social scientists have provided compelling arguments for viewing the consequences of variables acting in combination with each other. For example, Yinger argued that the basic unit for understanding a wide range of events is a field of variables:

The field is a new unit. The point is not simply that one sees better with two eyes used simultaneously than with one—indeed, one may not see much better but he sees differently; a new dimension is added. In mathematical terms, the combined result of two or more influences are a product, not a sum, of their interaction (Yinger, 1965:7).

But what combinations of what variables are likely to be effective? The question is, of course, empirical as well as theoretical. Building on prior research, a configurational model embodying the principles suggested by Yinger and others is presented in simple graphic terms in Figure 1. This schema implies that both attitude and perceived social influences do effect behavior directly. However, when attitude is combined with social influence variables in configurations involving interaction effects between such variables, (theoretically) the predictive power is greater than when these variables operate separately. Although not shown graphically, the configuration concept implies that combining one variable with attitude would increase prediction; including a second would increase it more. This increase would continue until all social influences relevant to a given situation were combined with attitude. At this point one would, presumably, attain maximum predictive power. This general idea constitutes the configurational approach to attitude-behavior relationships. However, combining more and more situational influences with attitude does not imply that one simply *adds* the effects of such variables to the predictive capacity of attitude. The configura-

tional model is premised on the assumption that the interactions between variables (i.e., configurations) will substantially increase the ability of the variable complex to predict behavior over and above the variables operating separately. Thus, adding variables is not as important as combining variables statistically.

While the idea of configurations is intuitively easy to grasp, adequate theoretical conceptualization and testing against data demand a statistical treatment that explicitly examines such interaction effects. Unlike sociological research that has discounted interaction, the configurational framework views interaction as the key to effective prediction.

This study presents a statistical analysis of a simplified version of this configurational model. The attitude topic is the "legalization of marijuana." In addition to attitude toward this issue only two situational variables will be considered. Both involve primary groups in the role of reference groups. These variables are "perceived peer position" and "perceived family position" toward the issue. These variables and their combinations are used to predict overt behavior—that is, whether, when given an opportunity, subjects voted for or against legalizing marijuana in an experimental session several weeks after the attitude and reference group perceptions were measured.

The present analysis admittedly only partly tests the configuration model. Only one general type of social influence is considered, namely, perceived reference group

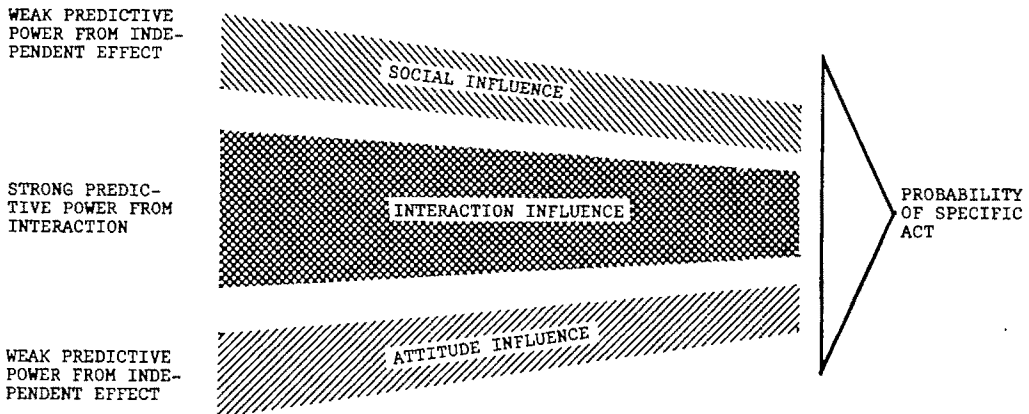


FIGURE 1. A GRAPHIC REPRESENTATION OF THE CONFIGURATION APPROACH TO THE ATTITUDE BEHAVIOR RELATIONSHIP

position; numerous other social influences have been left out. This should not imply that other socio-cultural or psychological variables are unimportant in predicting overt behavior. Hopefully, subsequent research will extend this form of analysis to other attitudes, issues, and situational variables. However, the present purpose is not to catalogue all possible predictive factors but to explore the efficacy of the configurational approach—in an admittedly specific context—compared to the relative predictive capacity of attitude alone or other variables alone. As will be shown, even the present relatively simple analysis provides considerable support for the predictive efficacy of the configurational model.

Overview of Research Procedures

The procedures for gathering the data used in this analysis have been described in detail elsewhere (Albrecht, 1970). However, they can be summarized briefly. The subjects were drawn from courses in introductory sociology at a large Pacific Northwestern university. An analysis of their characteristics showed them to be fairly typical undergraduates (Kamal, 1970). The issue, legalizing marijuana, was particularly relevant to these students. An earlier study at this university indicated that nearly half had been present when marijuana was used; and over 20 percent had tried it themselves (L. DeFleur and Garrett, 1970). These and other observations suggest that both attitude and behavior toward legalizing this drug were salient issues to the sample from which the present data were obtained.

A total of 436 students were given a "public opinion survey" which contained the items on attitude as well as perceived family and peer position toward legalization. The items were presented in the context of a large and rather complex questionnaire, so that subjects would not relate them to the later measurement of overt behavior. Follow-up interviews some weeks later indicated that this goal was achieved.

Of these 436 subjects, 81 percent agreed to participate (for payment) in an unnamed experiment later in the semester. From those who agreed to participate, 204 were carefully selected using several controls, so that

seventy-two were in the first quartile and seventy-two were in the fourth quartile on the attitude variable, i.e., they most (or least) favored legalizing marijuana. In addition, thirty were in the second and thirty were in the third quartiles. This relative deemphasis of the middle positions was found to have introduced no biases in the analysis. It was intended to provide a large number of subjects (in the first and fourth quartiles) with *clear-cut* attitudes. It also permitted testing hypotheses on attitude salience. Due to missing background data on two cases the final sample was reduced to 202 cases.

Attitude toward legalizing marijuana was assessed using a pretested sixteen item Likert scale. The responses to this scale were close to a normal distribution. For the present analysis, subjects were divided at the median so that 101 were classified as favorable and 101 unfavorable toward legalization. (Another approach using only subjects from the first and fourth quartiles was also tested. The latter model gave slightly stronger predictions. However, there were too few cases to get stable estimates of the independent effects for some configurations.)

Perceived family positions toward legalization were classified on a five point scale from "very favorable," to "very unfavorable." This variable posed certain problems in operationalization as the distribution had an extreme positive skew. It was skewed thus because most subjects saw their parents as definitely opposed to legalizing marijuana. For this reason it was necessary to combine "very favorable," "somewhat favorable," and "neutral or don't know" into one category titled "not opposed." This classification contained only 16 percent of the 202 subjects. The remaining subjects (84 percent) were classified as perceiving their families as being "opposed." This strong tendency for families to be perceived as opposed to legalizing marijuana limited the present analysis statistically. In any case, for ease of exposition, the "not opposed" category will sometimes be called "positive" or "favorable," although the problem will not thereby be resolved.

Perceived peer position was measured on the same scale as perceived family position.

However, in this case it was possible to trichotomize the scale, thus separating the neutral and don't know group. The three levels were "favorable" (35 percent either perceived their peers as somewhat or very favorable), "neutral" (21 percent perceived their peers as neutral or don't know), and "unfavorable" (44 percent perceived their peers as somewhat or very unfavorable). In the present analysis, the "neutral" category will not be evaluated in depth as the model does not make a clear prediction for this position.

The overt behavior toward the attitude issue was measured in a experimental session several weeks after the "public opinion survey" was administered. Voting was chosen as a form of behavior having a special significance in American society. It is simple to use in a laboratory setting, and it provided for various conditions (secret versus public, etc.). It could be argued that other forms of action would be preferable. However, for exploratory work, voting was both simple and meaningful. Thus, the actual behavioral measurement was a written ballot on which the subjects overtly voted (in a ballot box) for or against the legalization of marijuana. The subjects were divided into groups for the experimental period. Half of them were told their vote would be "released" to their home town press. However, this group was found not to differ significantly in behavior from the other half who voted in private. Since they did not differ significantly, both groups could be combined and treated as manifesting the same overt behavior. The similarity of public and private voting behavior may attest to its utility as a laboratory measure of overt action. Several additional variables were manipulated in the experimental sessions that potentially could have influenced the results of the present paper. However, the data indicate that these variables either had no significant effect, or were randomly applied to the subjects.¹

¹ A discussion of the effects of these variables appears in Frideres (1970), Albrecht (1970), Acoc (1971), and Gray and Warner (1971). The other variables dealt with discussion sessions that preceded the vote. Although some of these had significant effects (see Frideres, 1970; Kamal, 1970), the conditions were randomly assigned to subjects. This procedure has a conservative effect on the re-

Statistical Design

A statistical design to evaluate the configurational approach must have several features. It must allow one to estimate the unique effects of each variable as an aspect of the larger process. Further, it must enable one to analyze the model taken as a whole. Most important, it must afford analysis of various configurations of the independent variables. These configurations, or interactions, must be treated as the joint occurrence of two or more independent variables. For example, it must be possible to evaluate the importance of a favorable attitude combined with perceived peer support over and above what would be expected if these two variables operated separately. The "general linear model" meets these requirements.

Using the linear model poses few problems when all variables are interval level or above. There is, however, a potential problem when the dependent variable is treated as a dummy variable.² Such is clearly the case in the present study where the dependent variable is the actual vote which either does or does not occur.

The way out of this problem has been suggested in the statistical literature (Netter and Maynes, 1970; Sonquist, 1970; Gray and Warner, 1971). It involves a change in discourse from the individual to the aggregate level. This change is logically consistent with the usual focus of sociology on groups or aggregates and has important research applications beyond the present study. Basically, one is no longer interested in predicting what any single individual will do in light of his personal idiosyncracies. Rather, one predicts what the *conditional probability* (or expectation) is for members of specified categories to act in prescribed ways.³ This type of dependent variable can

ported results since it randomly increases the error term. Future research without these variables might well lead to even stronger results than are presently being reported.

² The independent variables in the present study are ordinal. The linear model uses only nominal information in its application. However, the sign of the B coefficients will allow us to evaluate ordinal hypotheses (e.g., those who perceive their peers as favorable are more likely to vote positively).

³ We should emphasize that we think of the configurations as categories of people who share a spe-

be termed Z . In this form, it can be written as either an expectation or a conditional probability (see Equation 1):

$$Z = E(Y|X_1) = P(Y = 1|X_1) \quad (1)$$

Where $E(Y|X_1)$ = the expectation of an individual's voting "yes" ($Y = 1$), given membership in a category defined by a configuration of independent variables X_1 . (By implication it should be clear that $1 - Z$ = the conditional probability of voting "no.") And where $P(Y = 1|X_1)$ = the conditional probability of an individual's voting "yes" ($Y = 1$), given membership in a category defined by a configuration of independent variables X_1 .

This approach may be clarified by considering what the various terms mean as they are used for the present type of data in the general linear model given in Equation 2 (below). In this case, the value of B_0 indicates the expectation or probability of voting to legalize marijuana ($Y = 1$) for the category of subjects who have negative attitudes and perceive both their family and peers as negative. B_0 could have represented some other category. This particular combination was selected because it provides a theoretical "base line." That is, members of this category should be expected to have the *lowest* probability of voting to legalize marijuana. The additional B values show the increment (or change) in the expectation of voting to legalize marijuana as we move to other categories. In other words, one of the B values indicates the increment that can be independently attributed to a favorable attitude (by itself). Thus when B_0 (the intercept) and the B value associated with favorable attitudes are added, the result is the probability of those with a favorable attitude, but with peers and family negative, voting to legalize marijuana. The B values for other variables and configurations have similar interpretations. Consider the category of sub-

jects who have a favorable attitude and perceive their peers as favorable. There are four relevant terms. This category, like every category, has a certain minimum probability given by B_0 . They also have one B value for a favorable attitude and one other for perceiving peers as positive. Finally, one must add still another B value for the independent effect of the *combination of attitude and peers positive*. The configurational orientation states that combinations such as the latter should have positive effects and should improve behavior predictions.

There are many limitations to statistical inference using the above procedures and in applying them to these data.⁴ The linear model places no theoretical limit on the values the dependent variable can assume. Hence, it is possible to get predictions (Z values) of less than zero or greater than unity. There is the additional problem of multi-collinearity, particularly irksome in examining interactions.⁵ These problems led the present researchers to limit the use of statistical inference. To minimize potential difficulties, the present analysis focused on descriptive measures. Still, the results are strong enough to suggest that the configurational approach is much more successful than earlier theoretical orientations for interpreting attitude-behavior relationships.

At this point it is desirable to state the

⁴ The problems specifically relevant to the present findings have been reported by Acock (1971). Netter and Maynes (1970) who summarize some of these and other problems and also refer to a large body of literature in the area concerning proper and improper use. Although we feel that these procedures greatly aid research analysis, the reader is cautioned that this approach must be individually applied. Since this study was completed, Goodman (1972) has presented an alternative procedure. However, the procedures we have used are adequate.

⁵ We are not dismissing the problem of multicollinearity. The results reported however, describe the present data. Before inferential statistics can be applied to specific estimates, it will be necessary either to replicate the present design with a large sample, or, experimentally manipulate the variables to keep them orthogonal. The reader is strongly cautioned not to generalize these results beyond the present sample by drawing statistical inferences. The model fits the present data well, and in so doing suggests that similar approaches might be tried on other data. For the coefficients of explained variation which relate the independent variable: see the appendix.

cified set of attributes, for example, a favorable attitude, a favorable perception of their peers' attitude, and an unfavorable perception of their family's attitude. The probability of a certain behavior is expected to vary from one category to another, to be conditioned by membership in the category. Hence, the unit of analysis is the category, since in this form of analysis only the category and not the individuals can possess a probability.

specific application of the general model that will be used for the present problem. Thereafter, the next section will present the actual data analysis.

Briefly, the model may be written as follows: $Z = B_0 + B_1X_1 + B_2X_2 + B_3X_3 + B_4X_4 + B_5X_1X_2 + B_6X_1X_3 + B_7X_1X_4 + B_8X_2X_3 + B_9X_2X_4 + B_{10}X_1X_2X_3 + B_{11}X_1X_2X_4$ (2) Where Z is the expected value of Y or the conditional probability of voting to legalize marijuana;

$X_1 = 1$, if a subject has a favorable attitude

$= 0$, otherwise;

$X_2 = 1$, if a subject does not perceive his family as opposed

$= 0$, otherwise;

$X_3 = 1$, if a subject perceived his peers as favorable

$= 0$, otherwise;

$X_4 = 1$, if a subject perceives his peers as neutral

$= 0$, otherwise.

We will now summarize the statistical procedure and relate it specifically to equation 2. The procedures for writing such expressions are presented in elementary texts on linear models (Cohen, 1968; Mendenhall, 1968) and will not be discussed at this point. The value of " Z " is the expectation (or conditional probability) that a person will vote to legalize marijuana for any category or combination of categories. This should vary depending on whether he is in one or another category, as was explained earlier. If he happens to be in the category with a favorable attitude, he receives a 1 for X_1 ; if not, he receives a 0. This has the effect of making B_1 relevant only for those subjects who have favorable attitudes. Similarly, the other terms logically exhaust the possible levels of attitude, family, and peers, as well as their various combinations. For example, B_{10} is relevant only if $X_1 = X_2 = X_3 = 1$. To be involved in this particular configuration means that the subject would have a favorable attitude, perceive his family as favorable, and perceive his peers as favorable. What is the probability that such a person would vote to legalize marijuana? The answer is given by B_0 (the intercept which is relevant to everyone) + B_1 (the increment due to attitude) + B_2 (the increment due to family) + B_3

(the increment due to peers positive) + B_8 (the increment due to the interaction between attitude and family positive) + B_6 (the increment due to the interaction between attitude and peers positive) + B_8 (the increment due to the interaction between family and peers positive) + B_{10} (the increment due to the interaction between all three variables being favorable together). In the foregoing, of course, the terms B_4 , B_7 and B_9 are not relevant since they involve configurations which are not possible for those involved in the configurations mentioned. For example, there is no B_4 because B_3 specifies that the subject perceives his peers as favorable. Hence, he cannot at the same time perceive them as neutral—which B_4 requires.

The Zero Order Relationships

Before assessing the above model, it is important to discuss the zero order relationship between each independent variable and the behavior under study. Each of the three independent variables had a positive relationship to behavior when treated separately. Attitude had a much stronger relation than has been the norm for the literature. Using the logic of the linear model, it was determined that attitude could explain 27.6% of the variation in behavior (Alker, 1965, discusses the statistical procedures used in these bivariate relationships). This result had a probability of less than .001 using chi-square. The rather unanticipated predictive ability of this attitude is probably due to the strong salience of the topic to the sampled population.

The results of perceived family position were positive, but clearly less so than those above. Only 1.3% of the variation could be explained ($.05 < p < .10$). This weak relationship may be partly an artifact of the operational procedures (as was mentioned the "neutral" and "don't know" subjects were included with those who perceived their families as "favorable"), or it may relate to the relatively low influence parents have concerning this issue. The "generation gap" concerning "pot" has been widely discussed.

The perception of one's peers was strongly related to behavior, explaining 31.1 percent of the variation ($p < .001$) when the neutral group was excluded from the analysis. The 31.1 percent figure is misleading. It is dif-

difficult to compare this to the 27.6 percent explained by attitude alone because the attitude variable was divided at the median. Due to the nature of the measurement of perceived peer position, it was not possible to divide the responses at the median and make them directly comparable to the attitude measurement procedure. Including the neutral subjects might have attenuated the explained variation, perhaps to the level of the attitude variable.

Overall Results of the Configuration Model

Since each variable had a positive relationship with behavior, let us now consider the overall results of the linear model. In reading this section the reader is reminded that the norm for the literature is to find about 10% of the variation explained. The overall model presented in Equation 2 was able to explain 35.4% ($R = .596$, $p < .001$) for the entire 202 subjects. As was mentioned earlier, another model was tested using only those 144 subjects with strong attitudes (first and fourth quartile on attitude scale); and the results were even stronger, with 42.1% of the variation explained ($R = .649$, $p < .001$). These results appear to support strongly the configurational approach within the limits of the present data. Furthermore, combining the other variables with attitude adds 7.9% to the explained variations. When the effects of these variables are standardized, they add 11.0% (see Gray, Warner, and Richardson for a discussion of this procedure).

Three conclusions seem justified from this initial analysis. First, attitude did much better than has often been reported. Second, combining attitude with additional variables clearly increased the overall prediction. Finally, the configurational model is strongly supported by its overall power to predict behavior.

Tests of Specific Configurations

While these results are all consistent with the configurational approach, the major test of the model requires a breakdown of the effects of each term. Therefore, the remainder of the paper will focus on the regression coefficients associated with the various configurations. The correlation coefficients are

less critical with dummy variables because: (1) we are mainly interested in predicting the probability of behavior category by category (the regression coefficients do so directly); and (2) the correlations include configurations where no clear prediction is expected (for example, perceived neutrality of peers); and hence the results may be what we expect without the correlations being large.

As the model indicates the independent effects of each variable should be positive, but they need not be strong. The above discussion of zero order effects does not treat this problem directly. The theoretical model demands that the independent effects be isolated, and to do so requires controlling for all the other terms. These considerations lead to three hypotheses:

Hypothesis 1. The independent effect of attitude on behavior is positive.

Hypothesis 2. The independent effect of perceived family position is positive.

Hypothesis 3. The independent effect of perceived peer position is positive.

The second point made by the theoretical model is that combining additional variables leads to improved predictions. When one variable is combined with attitude, the prediction should be enhanced substantially. Moreover, the configurations should have a positive independent effect over that obtained by simply adding the effects of the separate variables. These considerations lead to two hypotheses:

Hypothesis 4. The independent effect of the configurations is positive.

Hypothesis 5. The configurations are better at predicting behavior than the individual variables.

Hypothesis four predicts that the configurations will have their own independent effect that cannot be explained by simply adding the independent effects of the component variables. This is the fundamental notion of field theory. Hypothesis five goes further to stipulate that the configurations of attitude with other variables should lead to clear predictions of behavior. The support of hypothesis five does not depend on whether the prediction derives from the additive or the interactive effects of the variables. Considered together, these two hypotheses stipulate that the configurations should both add

to the predictive power and should achieve a high level of prediction.

In order to facilitate discussion it might be useful to summarize the overall results of the solution to the linear model. Table 1 shows each variable, each configuration, the coefficient from equation 2 associated with it, the increment associated with it, and the resulting overall prediction in the number of cases used for each estimate. For reasons discussed above, significance levels are not reported for the individual estimates.

The hypotheses may be evaluated by carefully considering the results reported in Table 1. It will be recalled that the intercept value of $B_0 = .20391$ is associated with no explicit hypothesis. The value indicates the expected probability of voting to legalize marijuana, if a subject has an unfavorable attitude, perceives his family as unfavorable, and per-

ceives his peers as unfavorable. This value serves as a base line for evaluating the other terms. It indicates the low probability of positive behavior under the above conditions.

There is some support for Hypothesis 1: attitude does have an independent effect on behavior with other variable effects removed. The value $B_1 = .22466$ (the regression effect increase) shows that a favorable attitude in itself (where other variables are negative) more than doubles the probability (over that of B_0) that a person will vote positively. However, the conditional probability for attitude operating alone brings the expectation to only $Z = .429$ (i.e., $.20391 + .22466$). These results show (1) attitude is positively associated with behavior as Hypothesis 1 specifies, but (2) by itself, attitude does not yield unequivocal predictions of behavior.

Table 1. Application of Linear Model to Configurational Orientation

Multiple Regression Solution					
Independent Variable or Configuration	Coefficients from Model	Regression Effect* (Change in Probability)	Probability of Voting:		Frequency Base
			"Yes"	"No"	
Base line (all variables negative)	B_0	.20391	.204	.796	101
Attitude+**	B_1	.22466	.429	.571	101
Family+	B_2	-.23886	.000	1.000	31
Peers+	B_3	.17108	.375	.625	71
Peers n	B_4	.06998	.274	.726	43
Attitude+ $\cap$ Family+	B_5	.14361	.333	.667	25
Attitude+ $\cap$ Peers+	B_6	.22257	.822	.178	62
Attitude+ $\cap$ Peers n	B_7	.26616	.765	.235	22
Family+ $\cap$ Peers+	B_8	-.13614	.000	1.000	18
Family+ $\cap$ Peers n	B_9	.01175	.023	.977	9
Attitude+ $\cap$ Family+ $\cap$ Peers+	B_{10}	.35101	.942	.058	17
Attitude+ $\cap$ Family+ $\cap$ Peers n	B_{11}	-.25772	.300	.700	5

Note: $R = .596$; $p .001$; + = positive; n = neutral; $\cap$ = interaction, i.e., simultaneous occurrence.

*Five places were included from the computer solution to avoid rounding errors in the additions; attention should be given only to the first three digits.

**Where only one variable or a two-variable configuration is indicated, those factors not indicated are negative.

There is no support for Hypothesis 2. Indeed, the independent effect of a favorably perceived family position (while other factors remain negative) is in the opposite direction of that predicted. The value of $B_2 = -.23886$ leads to a prediction that $-.035$ (i.e., $.20391 - .23886$) of subjects who perceive their family as not opposed will vote to legalize marijuana (with other variables controlled). A person cannot have less than a zero probability of voting affirmatively. The slightly negative value is due to the fact that the least squares solution imposes no lower limit. This means that although the $-.035$ is theoretically impossible, it is still the best least squares solution and is sufficiently close to a possible value of zero that the discrepancy may be ignored. Therefore, zero has been entered into Table 1 in the B_2 row. The unusual results in this row may be due to the family's limited influence or they may be an artifact of the operational procedure for the family variable. It would not seem wise at this point either to accept or reject Hypothesis 2; but further study with better measurement of this variable is needed.

There is support for Hypothesis 3. The regression effect value, $B_3 = .17108$ leads to the predicted probability of an affirmative vote of $Z = .375$ (i.e., $.20391 + .17108$). This value is far from sufficient for predicting behavior. However, perceived peer position does appear to have a small independent effect.

Two preliminary conclusions from the above analysis seem warranted. First, two of the three hypotheses are supported (the one on attitude and the one on peers). The one failure may in part be explained by a "generation gap" or by the method of operationalization. The second conclusion is much more critical to the theoretical point of view presented in Figure 1. It seems clear that the independent variables operating separately are unable to predict behavior reliably. The variable for attitude and peers contributes slightly to one's ability to predict who will vote affirmatively. The family variable contributes nothing to a "yes" vote, at least in this particular context. Any improvement in the ability to predict positive behavior must come from the configurations. This is precisely what the theoretical orien-

tation specified. That is attitude and social influence should each have weak positive relations with behavior, but configurations should yield stronger predictions. The analysis of the configurations will further test the conceptualization.

The first configuration combines attitude and perceived family support. Hypothesis 4 is supported. That is, $B_6 = .14361$ means that there is a positive increment over what would be expected if the variables operated separately. However, Hypothesis 5 is not supported in this instance. The prediction for members of this configuration, $Z = .333$ (i.e., $.20391 + .22466 - .23886 + .14361$), is not particularly noteworthy. Again this configuration contains the problematic family variable.

The second configuration, combining attitude and peer support, strongly supports both Hypotheses 4 and 5. The B_6 value of $+.22257$ indicates the regression increment for the attitude-peers positive configurations. When this increment is combined with the independent effects of the two variables, the combined prediction is $Z = .822$ (i.e., $.20391 + .22466 + .17108 + .22257$). Such a level of prediction is not customary in attitude-behavior research. This fact has two meanings. First, the configuration offers information not available in the independent operation of the component variables ($B_6 = .22257$) supporting Hypothesis 4. Second, the joint occurrence of the two variables leads to a reasonably clear prediction of behavior supporting Hypothesis 5. Stated more simply, fully 82 percent of the sixty-two subjects included in this configuration voted to legalize marijuana.

With respect to the combination of attitude with *both* family and peer support, the theoretical model specifies that the prediction should be even clearer. Although the configuration contained only seventeen cases, the results strongly support both Hypotheses 4 and 5. First, the configuration has the largest positive regression increment, $B_{10} = .35101$, of any term. When the appropriate terms are combined, the prediction is that members of the configuration have a probability of voting to legalize marijuana of $Z = .942$ (i.e., $.20391 + .22466 - .23886 + .17108 + .14361 + .22257 + .13614 + .35101$). This means that over 94 percent

(sixteen out of seventeen) of the subjects with all variables positive voted to legalize marijuana! In view of the level of association and prediction usually found in the literature this result seems potentially important. It suggests that the configurational approach can greatly expand our understanding of the attitude-behavior relationship in terms of predicting overt action.

Social Influences as Cross Pressures

There is no direct hypothesis relevant to the family-peer configuration in itself. The model does not explicitly state that the various social influences interact with one another. The social influences are viewed as interacting with attitude within various configurations. However, what of the case of "cross pressures"? Can the influence of reference groups alone overcome the contrary influence of attitude? One hypothesis would be that if subjects are caught in such cross pressures, with attitude pushing them in one direction and social influences in another, attitude would tend to prevail in shaping their behavior. Data from the B_8 configuration would seem to support this for those who had negative attitudes and positive reference group influences. That is, the value of $B_8 = -.13614$. This probability "increment" due to seeing one's family and peers as favorable when one's own attitude is negative leads to a $Z = .00001$ (i.e., $.20391 - .23886 + .17108 + .13614$). In other words, the probability of voting "yes" for these subjects was zero. All eighteen of those in this configuration voted in accord with their negative attitudes. In other configurations with attitudes negative (but with some support from one or the other reference group), the majority also voted in accord with their attitudes, e.g., B_2 , B_3 and B_9 (with peers neutral in the latter case). However, in the reverse situation, with attitude positive and the cross pressures negative (as in B_1), the hypothesis of the primacy of attitude could not be confirmed!

Further study of the impact on behavioral decisions of cross pressures induced by mixed attitude and reference influences is clearly needed. The above suggests that negative

attitudes seem to have primacy in the cross pressure situation, while positive attitudes do not. In any case, it seems clear that social influences play a limited role in their own right. Perhaps the best hypothesis at present is that attitude is a *pivotal* variable and that reference group influences interact with it in various patterns to shape behavioral outcomes but have limited influence in cross pressure situations. Such an interpretation suggests that those who argue that we should ignore attitude in behavioral prediction and concentrate on social influences may be on the wrong track.

Discussion

A great many studies of attitude-action relationships have noted: (1) the limited ability of attitude by itself to predict behavior; (2) the role of various situational influences in modifying the attitude-behavior relationship; and (3) the need for a systematic statement on how variations in overt behavior depend on variations in attitude combined with other independent variables.

This paper has presented a somewhat simplified and tentative version of a configurational approach to such contingent consistencies based on a specific model. It suggests that this approach can meet the need indicated in (3) above, while taking (1) and (2) into account. In general, this specific application of the general linear model supported the configurational approach. While the present analysis by no means completely demonstrates the value of this specific model, the combination of only two situational influences, perceived family position and perceived peer position, with attitude yielded clear predictions. Thus, the promise of the configurational approach seems high.

An important hypothesis that is suggested by the present analysis is that situational influences, alone or in their own configuration, do *not* adequately predict overt behavior. Only when coupled with attitude do these variables have solid predictive capacity. It was hypothesized, somewhat speculatively, that in the complex of psychological, social and cultural variables involved in attitude-related decisions to act, attitude(s) may play a central or pivotal role. Thus,

attitude(s) may provide a base-line factor for decision-making about action toward the relevant issue or object. Against this base-line the individual raises other considerations, such as the views held by his reference groups, considering in particular, possible sanctions for acting one way or another; *then* he makes his action decision. This "pivotal hypothesis" provides a clear direction for further research.

Overall, the present research should be regarded as exploratory and its conclusions tentative. It is a laboratory study of only one attitude and a relatively simple act. It also focuses on a student population—as does so much research on this problem. While it is based on an old idea (field theory), it proposes a previously untried model—a systematic way for studying the relationship between attitudes, situational variables and overt behavior. If further research on other attitudes and situational variables shows this approach to be more generally applicable, it may have a number of practical implications in situations where variations in overt action are contingent upon variations in attitudes and situational variables combined in similar configurations.

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Coefficients of Explained Variation (r^2) Between Independent Variables.*

Variable	1	2	3	4	5	6	7	8	9	10	11
1. Attitude+	1.00	.07	.30	.00+	.14	.45	.12	.08	.00+	.09	.03
2. Family+		1.00	.04	.00+	.77	.05	.00+	.53	.26	.50	.14
3. Peers+			1.00	.14 ^A	.07	.81	.07 ^A	.18	.01 ^A	.17	.01
4. Peers ₊				1.00	.00+ ^A	.12 ^A	.45	.03 ^A	.13	.03 ^A	.10
5. Att.+ $\cap$ Fam.+					1.00	.09	.01	.59	.13	.64	.18
6. Att.+ $\cap$ Peers+						1.00	.05 ^A	.18	.01 ^A	.21	.01
7. Att.+ $\cap$ Peers ₊							1.00	.01 ^A	.10	.01 ^A	.21
8. Fam.+ $\cap$ Peers+								1.00	.00+	.92	.25 ^A
9. Fam.+ $\cap$ Peers ₊									1.00	.00	.53
10. Att.+ $\cap$ Fam.+ $\cap$ Peers+										1.00	.00
11. Att.+ $\cap$ Fam.+ $\cap$ Peers ₊											1.00

* Each coefficient is based on a two by two table, and the r^2 values are equivalent to ϕ^2 values.

^A The values are squared coefficients and hence the signs are missing. The coefficients noted with an A reflect negative correlations.

TECHNOLOGY AND SOCIAL BEHAVIOR OF WORKERS IN FOUR COUNTRIES: A SOCIOTECHNICAL PERSPECTIVE *

WILLIAM H. FORM

University of Illinois (Champaign-Urbana)

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Social behavior of industrial workers is explained by the technical constraints of the work operations and the technological complexity of the industry, rather than newness of industrial employment and other social factors. Four automobile plants differing in technological complexity were studied in four countries. Four work operations selected in each plant varied in the degree to which they constrained interaction opportunities. The amount and quality of worker interaction was a joint function of the mobility opportunity provided by work operations and the departments' population density. Both mobility opportunity and density were related to the plants' technological complexity. Social interaction at work was moderately related to contact with workmates outside the plant but not to social anomie. These findings cast doubt on the long held belief that anomie results from an oppressive technological environment.

EUROPEAN social theorists have long emphasized the alienating effects of industry on the work lives and community behavior of workers (Marx, 1956; Durkheim, 1947; Ellul, 1967). Studies of industrialized societies have also stressed that new industrial workers experience serious problems of accommodation. Researchers have reasoned that the disruptive effects of industrial discipline are most keenly experienced by those socialized in the intimate, personalistic systems of traditional societies (Moore, 1951:114; Hoselitz, 1962:60-3). In many developing societies, workers have hesitated to commit themselves to industry (Feldman and Moore, 1960; Kerr, 1960), remaining anchored in the social life of the family and the village (Myers, 1958; Lo-preato, 1967:142).

In contrast, workers in older industrial societies presumably have forged a distinc-

tive way of life which is adapted to the bureaucracy of the factory and the anonymity of the city. In the factory they created informal organizations (Roethlisberger and Dixon, 1947; Whyte, 1955; Jaques, 1952) and in the city they fashioned integrated social circles (Hoselitz, 1955; Hatt and Reiss, 1957). If this view is correct, workers in mature industrial societies, compared to those in newly industrializing societies, would develop more and deeper relations with workmates inside and outside the plant. With a firmer social network, they should show less anomie than new industrial workers (Smelser, 1963:44). Yet until comparative studies measure the amount and quality of interaction in and outside the plant, generalizations about the response of workers to industry remain speculative. The research reported here provides some systematic evidence concerning the social interaction of workers in a particular industry in four countries at different stages of industrialization.

Not all research argues that industrial workers are traumatized by their first exposure to factory life. On the contrary, workers often accommodate quickly to industrial discipline (Inkeles, 1966; Karsh and Cole, 1968; Lambert, 1963; Reynolds and Gregory, 1965); the factory fosters adaptation to the new environment (Inkeles, 1969) by exposing workers to discipline, forcing them to meet people outside their traditional circles, and inducting them into such formal associations as the labor union.

* I am grateful to Dr. Paolo Ammassari of the University of Rome for urging me to do this study and for providing many ideas, some of which I unfortunately ignored. He was also the field director of the research in Italy. I am also indebted to the other field directors: Richard Gale, for Argentina; Baldev R. Sharma, for India; and Steven Deutsch, for the United States. I am especially grateful to Martin Messner for his criticism of an earlier draft of the manuscript and Joan Huber, William Faunce, and Sigmund Nosow for their helpful suggestions. The following agencies supported the research: The Social Science Research Council, the Fulbright Commission, the National Science Foundation, the School of Labor and Industrial Relations at Michigan State University, and the Institute of Labor and Industrial Relations at the University of Illinois (Champaign-Urbana).

This research argues that the social effects of industry are similar whenever their technologies are similar. Technology and its organization heavily influence the quantity and quality of worker interaction inside and outside the factory. This study hypothesizes that workers exposed to similar technologies, whatever their previous socialization, local culture, or society's level of industrialization, will respond to that technology similarly.¹ The spatial distribution of machines, the routines required to operate them, and the behavior necessary to link work functions heavily influence social interaction at work (Blauner, 1964; Sayles, 1963; Meisner, 1969). To a lesser extent, patterns of work interaction in turn influence interaction in the community. Thus work behavior, rather than newness of industrial organization or the worker's exposure to it, may be crucial to community relationships. In order to test this idea, the social behavior of workers should be examined in factories with similar technologies located in societies which vary in industrialization. The design of this research approximates this condition.

Selecting the Plants and Samples

To reduce complexities of the research design, a single industry was studied in four nations differing in degree of industrialization (see Form, 1969). The automobile industry was selected because its technology is relatively uniform, modeled on United States practices. The United States represented a modern industrial society; Italy, an industrial society with an old but expanding industrial base; Argentina, a more recent industrializing society; and India, a society with a small and new mass production sector. Various indices reveal that these countries represent ordered levels of industrialization.²

¹ This research focuses on social interaction in the work situation. Other sociologists of a technological persuasion (Woodward, 1958; Burns and Stalker, 1961; Zwerman, 1970) have investigated how an industry's technology affects its social organization and management behavior.

² No completely satisfactory index for degree of industrialization is available. However, all indices consulted show that the United States is at the highest point of development, that Italy and Argentina have similar ranks and are highly developed compared to India. Thus Farace (1966),

All plants had a complete range of operations from design to production, were in operation for at least ten years, and had native born managements. The plants were: Oldsmobile (OLDS) in Lansing, Michigan; Fabbrica Italiana Automobile Torino (FIAT) in Turin, Italy; Industrias Kaiser Argentina (IKA) in Córdoba; and Premier Automobile Limited (PAL) in Bombay. Random samples of manual workers were selected from departments having similar technologies and representing the core manufacturing processes. Skilled workers were selected from design, experimental, and maintenance departments; semi-skilled workers from parts manufacturing departments, and unskilled workers from the final assembly lines. The samples accurately represented the skill composition of all manufacturing departments; but since the number of skilled workers in the OLDS and IKA samples was insufficient, additional numbers were subsequently drawn and used for special analyses.

Technological Characteristics of the Plants

The technological organization of the four plants differed somewhat, as reflected in the distribution of skills and the workers' operations (see Table 1). OLDS had the most complex technology and the most automatic equipment, made the most complex product, and had more personnel in quality control (semi-skilled workers in test, inspection and repair). Although FIAT resembled OLDS, the company manufactured more of its parts, and thus had more machine operators. IKA, the Argentine plant, produced several models of cars and trucks and was not as automated or rationalized as OLDS or FIAT. PAL, the Indian plant, was technologically most primitive; more of its operations were performed by skilled workers rather than by semi-skilled; and more test-inspection-repair (TIR) functions per-

using fifty-four variables to rank 109 countries along a development continuum reported ranks as: United States, 1; Italy, 23; Argentina, 27; and India, 96. The same order was reported for per capita consumption, the ratios being: United States, 8,103; Italy, 1,186; Argentina, 1,069; and India, 140. The ranks for per capita coal consumption for 138 countries are: United States, 1; Italy, 46; Argentina, 51; India, 101 (U.N. Statistical Yearbook, 1961:278-9).

Table 1. Skill Level and Work Operations of Representative Samples of Automobile Workers in Four Plants (in percent)

Plant	OLDS	FIAT	IKA	PAL
Skill Level				
Skilled	19	18	16	25
Semi-Skilled	53	51	35	39
Unskilled	28	30	48	36
Totals	100	99	99	100
Departmental Operations				
Craft	12	18	10	30
TIR*	34	16	26	8
Machining	27	33	31	31
Assembling	27	32	33	31
Totals	100	99	100	100
N	(249)	(306)	(275)	(262)

*Test, Inspection, and Repair.

formed by higher skilled workers. The experimental departments were most alike in skill, but their ecologies differed. OLDS had the largest and most widely dispersed machines; those in the other plants were smaller and closer together. The four parts-making departments were the most uniform in the size and placement of their machines. Technological differences were most apparent in the assembly lines. OLDS' lines were the longest, most rationalized, most mechanized, and most rapid. Except for adjustments necessary to assemble smaller vehicles, FIAT's lines were similar to OLDS's. IKA had several slow moving lines for different types of vehicles. Workers were rotated from one line to another. PAL's crowded assembly lines were not mechanized; automobile frames were placed on dollies and pushed from one work station to another as crews performed a series of operations with hand tools.

Technological Constraints and Interactional Opportunities

Since this research suggests that technology heavily circumscribes worker behavior, the technologies of the selected operations in the industry must be specified. Technology includes (1) the tools or machinery appropriate for certain operations; (2) the routines required to operate those machines; and (3) the design of work flow.

In mass production industries, departments are typically homogeneous in these three attributes. Compared to the total possible range, the range of technologies in automobile manufacturing is relatively small. Consequently, to the extent that technology affects the interaction of workers, the range should also be relatively small. However, two technologies are not widespread in the automobile industry: hand tool or craft technology, and automated continuous process technology. Although these two technologies differ greatly, they tend to produce solidary work groups (Blauner, 1964; Woodward, 1958). Technologies in some industries, such as textiles, disperse workers and limit their interaction. The automobile industry is in the middle range of technological constraints on social interaction. Meissner (1969:244-5) says that workers control the details of their work performance at lower levels of technology. In the middle range, a man-machine mixture prevails; and performance is extensively programmed to machine demands. Here worker control of production performance is most tenuous. However, the fit of worker performance to machine requirements is imperfect, necessitating supervisory intervention. In the middle level of technology, workers, supervisors, and managers all exercise some control; and this situation puts the greatest pressure on the workers and reduces their independence.

Several students have investigated the man-machine relationships in the automobile industry (see Table 2). Assembly workers are confined to a restricted station, their work requiring only superficial attention and almost no communication. Communication tends to be frequent but brief and shallow because of the noise.³ Operators, whose machines make identical pieces, also work in a restricted technological environment, sometimes as restrictive as the assembly line. Test-inspection-repair operations provide a wider range of interaction opportunities. Constraints on interaction are high in many inspection operations (e.g., routine quality control), but the relative quiet permits con-

³ Touraine (1955) warns that it is easy to exaggerate the uniformity of social interaction on assembly lines; great variations are possible within and between lines.

Table 2. Technical, Ecological, and Social Dimensions of Four Technologies in Automobile Manufacturing*

Variables	Assembling Operations	Machine Operations	Test, Inspection, and Repair	Skilled-Craft Operations
Control dimensions: Tools and machines	Hand tool, individually controlled	Automatic machines, low control	Hand tools and high control	Semi-automatic multiple purpose machines, high control
Work flow and amount of control	Live line, low control	Hand Transfer, low control	Line or hand transfer, some control	No work flow, high control
Pace of work and control	Machine determined, low control	Timed production, low control	Timed processing, some control	Untimed production, high control
Operation cycle	Invariable and short, no control	Invariable and medium, minimum control	Variable cycles possible, variable amount of control	Variable to long cycle, high control
Task differentiation	Low and relatively independent	Low and independent	Moderate and dependent	High and independent
Work attention required	Surface and brief	Surface and moderately long	Detailed and brief to long	Detailed and long
Technological inter-dependence	Absent	Absent	Some	Some
Technically permitted interaction	Frequent and brief	Infrequent and brief	Variable	Infrequent and Long
Technically permitted cooperation	Absent	Absent	Some	Considerable
Spatial constraints and type of spatial boundaries	Small space, confined, restricted floating	Small space, confined, fixed boundaries	Variations in space toward loose boundaries	Larger space, semi-confined, loose boundaries
Sources of influence	Extra-technical	Extra-technical	Semi-technical	Semi-technical
Communication: duration and type	Intermittent, incomplete, shouting	Intermittent, incomplete, shouting	Intermittent and variable, talking	Intermittent and complete, talking

*See Meissner (1969) and Goldthorpe, *et al.* (1968:43-68) for related schemes.

tinuous communication. Workers who test motors and make minor repairs have more opportunity to move about and talk. Finally, craft operations are the least restrictive. Workers can set their machines, leave their work stations, and communicate with others under relatively quiet conditions. Although craft work requires occasional consultation with supervisors and fellow workers, it does not demand intensive and continuous interaction. Such freedom and autonomy permit the formation of solidary work groups (Sayles, 1963; Lipset, *et al.*, 1956). In summary, data in Table 2 show that assembling, semi-automatic machining, test-inspection-repair,

and craft operations constitute a hierarchy of technological constraint on the interaction opportunities of workers.⁴

The industry's technological complexity must also be considered. The less complex its technology, the less specialized are the machines and the less differentiated the skills required to operate them. Thus the four operations studied in IKA and PAL

⁴This hierarchy is similar to Tudor's (1972) measure of occupational complexity which has three dimensions: variety of occupational tasks, complexity of interpersonal relationships, and complexity of data-manipulations. The present study focuses on variety of occupational tasks.

were technologically less specialized than in OLDS and FIAT. Therefore, according to the theory, the range in interaction for IKA and PAL workers should be smaller than for OLDS and FIAT workers. Moreover, in plants technologically less complex (IKA and PAL), workers should have higher interaction opportunities because their machines are smaller and occupy less space, reducing the spatial barriers to interaction. The more standard the technology, the more uniform should be worker interaction rates. Thus, since machining was the most standard of the four operations, machine operators in all four plants should exhibit less variation in interaction than workers in other departments. Finally, machines associated with craft work vary in complexity and in capacity to perform multiple functions. The more complex the technology, the greater the worker's freedom to move about. Thus OLDS and FIAT craftsmen should have higher interaction opportunities than those in IKA and PAL. In summary, increasing industrialization is associated with increasing technological specialization and complexity, a condition which results in greater differentiation in constraints on worker behavior.

HYPOTHESES

This research hypothesizes that the level of a plant's industrial complexity and the degree of technological constraint on the job affect the quantity and quality of worker interaction on and off the job. A related hypothesis holds that non-technological factors such as workers' personal evaluations of each other, differences in social backgrounds (e.g., rural-urban), job evaluations, and seniority will not significantly influence interaction.

1. The higher the workers' positions in the job control hierarchy, the more social contacts they will have on the job.

2. The more technologically complex the plant, the more will the interaction rates of workers in separate work operations vary.

3. The less the plant's technological complexity, the higher the population density of its operations, and the more uniform the workers' social interaction rates.

4. The greater the social interaction on

the job, the more intimate the relationships.

5. The greater the interaction at work, the greater the interaction of workers off work.

6. Anomie is not directly related to the plant's technological complexity but inversely related to the amount and quality of worker contact.

FINDINGS

The most reliable data on social interaction is gathered by direct observation. Since we could not obtain permission to do this, we asked workers about the space and interaction at their work stations: how far they could move freely, how many people worked in the space, how many could they talk to, how many did their work require them to talk to, and how many did they actually talk to. Various ratios were constructed from this information.

Eliminating Some Non-Technological Influences on Work Interaction

The length of time a worker has been on the job, how he evaluates his job, how he gets along with his fellow workers, and his background experiences could affect the amount and quality of his work contacts. All workers had at least one year of plant tenure; and the lowest mean tenure was five years, certainly enough time to establish meaningful social ties. Three-quarters or more in the four plants were satisfied with their daily contacts and the same proportion were satisfied with their physical working conditions. Half or more had at least two good friends in their work group and an even larger percentage had at least one. Data presented in another report (Form, 1971) revealed that these conditions did not affect interaction rates. Briefly, the amount and quality of social contacts on the job were not related to how workers evaluated their fellow workers, their job satisfaction, tenure, rural or urban background, department's physical conditions, caste (for India), and many other variables. Since these findings eliminated some possible influences on work interaction, the relevance of technological factors was then explored.

Table 3. Percentage of Workers Who Can Move About Freely at Their Work Stations

Techno- logical Constraints	Complexity of Technology			
	(High)		(Low)	
	OLDS	FIAT	IKA	PAL
Assembling (high)	16	12	2	77
Machining	32	10	5	78
TIR	53	31	34	91
Craft (low)	79	61	38	67
Totals	47	25	16	76
N*	(306)	(306)	(315)	(262)

*All data in the remaining tables of this paper are based on the analytic samples and report the same number of cases as this table.

Technological Constraint and Social Interaction

Type of operation (assembly, machining, test-inspection-repair, and craft) was used to indicate technological constraints on interaction. Work operations usually reflect man-machine arrangements better than skill levels.⁵ Thus, unskilled workers can perform a wide variety of activities such as sweeping, assembling or cleaning machines; but a single operation, such as assembling, represents a more restricted technological milieu. However, in automobile factories more complex skills are required as one moves from assembly work, machine operations, test-inspection-repair, to craft work.⁶

Tables 3-5 present basic data on the ecology of workspace. Clearly, the lower the technological constraint, the greater the worker's physical mobility (see Table 3). This is especially noticeable at OLDS and FIAT, the plants with the most rational work organizations and the most complex technologies. Data in Table 4 show that in-

⁵ The associations between type of operation and all other variables used in this analysis were higher than those between skill level and the pertinent variables. The lower the plant's technological complexity, the more discriminating was the type of operation.

⁶ Unskilled workers are found among both assemblers and machine operators, and semi-skilled workers among machine operators and those in test-inspection-repair. In three factories no interaction differences appeared within the skill levels for the different operations. Only in FIAT did unskilled assemblers interact more than other unskilled workers; while, semi-skilled machine operators interacted more than other semi-skilled workers.

Table 4. Percentage of Workers with More Than Twenty-four Workers in Their Workspace

Techno- logical Constraints	Complexity of Technology			
	(High)		(Low)	
	OLDS	FIAT	IKA	PAL
Assembling (high)	13	22	24	29
Machining	16	21	15	35
TIR	37	44	37	41
Craft (low)	45	82	33	28
Totals	28	38	25	32

creasing percentages of workers are found in workspaces associated with lower technological constraints. As expected, this trend is most striking in OLDS and FIAT, the plants with the most complex technologies. Data in Table 5 reveal that the lower the operation's technological constraint, the greater the tendency for workers to interact. Differences between assembly workers and machine operators are predictably small, but the differences between these workers and those in test-inspection-repair and the crafts are quite large. These results support observations drawn from Table 2, and the hypothesis that those performing complex operations are less constrained in mobility, hence interact more.

Technological Complexity, Density, and Social Interaction

The third hypothesis posits an inverse relationship between technological complexity and density. Since interaction may be affected by population density, two indices of density were devised. The first measured the physical concentration of workers and may be used to compare densities directly. The second was adjusted for each plant to assure sufficient numbers in the density categories for statistical analysis. The two mea-

Table 5. Percentage of Workers Who Talked to Eleven or More Workers

Techno- logical Constraints	Complexity of Technology			
	(High)		(Low)	
	OLDS	FIAT	IKA	PAL
Assembling (high)	28	18	8	31
Machining	28	19	1	26
TIR	41	49	19	50
Craft (low)	55	79	23	37
Totals	36	37	11	33

Table 6. Percentage of Workers by Operation in High Density* Areas

Techno-logical Constraints	Complexity of Technology			
	(High) OLDS	FIAT	(Low) IKA	PAL
Assembling (high)	55	59	91	94
Machining	43	51	79	93
TIR	57	75	78	96
Craft (low)	38	80	82	90
Totals	48	64	83	93

*Density was defined as number in workspace over workspace in yards. High density was defined as a ratio over 1.0.

sures were highly and significantly associated.⁷

The first index of density shows an inverse relationship to complexity of plant technology; the more complex the technology, the lower its density (see Table 6). Density was lowest at OLDS and highest at PAL. Moreover, the more complex the plant technology, the greater the differences in density of its operations. Thus, operations in OLDS and FIAT were relatively highly differentiated in density; but almost no differentiation was apparent in PAL. As anticipated, machining departments in all plants tended to be low in density because the machines occupied so much space; but assembly departments had higher densities. Test-inspection-repair operations tended to be high in density as were craft operations, except for OLDS, which had the most complex machines. Similar results were obtained with the standardized index of density. These data generally support the third hypothesis and conform to my impressions based on physical inspection of the plants.

Regardless of technological constraints, the higher the workspace density, the greater likelihood of worker contact. Table 7 presents data for high interaction workers according to their operations and workspace density. In all plants and job categories, workers in higher density areas experienced high interaction.

Interaction on the job is also affected by how many persons a worker must speak to to do his job. In all four factories, the lower

⁷ The uncorrected coefficients of contingency were: OLDS, .613; FIAT, .678; IKA, .505; PAL, .485.

Table 7. High Interaction at Work (Talks to Eleven or More Persons) by Density of Work Place According to Degree of Technological Constraint (in percent)

Techno-logical Constraints	Density*	Complexity of Technology			
		(High) OLDS	FIAT	(Low) IKA	PAL
Assembling (high)	Low	16	-	21	-
	Medium	26	18	32	56
	High	58	82	47	44
	Total	100	100	100**	100
Machining	Low	38	-	26	27
	Medium	14	12	49	34
	High	48	88	26	39
	Total	100	100	101	100
TIR	Low	-	-	15	-
	Medium	11	24	36	55
	High	89	76	49	46
	Total	100	100	100	100*
Craft (low)	Low	26	-	22	11
	Medium	19	2	39	39
	High	55	98	39	50
	Total	100*	100	100*	100
N		42	48	36	28

*The adjusted density index was used for this table.

**Chi-square in original table not statistically significant at the five percent level.

the operation's technological constraints, the higher the required communication (see Table 8). For example, in OLDS only one-tenth of the assemblers, compared to three-tenths of the craftsmen, had to converse with five or more persons to do their jobs. These

Table 8. Workers Who Must Talk to Five or More to Perform Their Jobs, According to Operations Hierarchy (in percent)

Techno-logical Constraints	Complexity of Technology		
	(High) OLDS	FIAT	(Low) PAL
Assembling (high)	11	6	20
Machining	12	12	16
TIR	21	34	27
Craft (low)	35	25	31
Totals	21	17	23

ratios were similar in all plants. Yet, opportunities to interact exist beyond those required by the work; and these were typically exploited. The majority of workers needed to talk to no one or only one person, but they reported talking to an average of six or more. A ratio was computed for the amount of possible interaction experienced by dividing the number of people workers did talk to by the number they could talk to. The percentage who took most advantage of the chance to interact increased from OLDS to PAL (Table 9) following the trend of increasing densities (Table 6). This suggests that, as the complexity of plant technology increases, workers are less able to interact because the larger machines occupy more space and disperse the workers, thus reducing communication. Primitive technologies promote density and interaction. As expected, in all plants except PAL where the differences were small, assembly workers took most advantage of the chance to interact (Table 9).

Quality of Interaction

Social scientists have observed that social interaction at work is often shallow, that workmates are not typically close friends, and that their central life interests are outside the plant (Dubin, 1956; Goldthorpe, *et al.*, 1968). Yet few scholars, with the exception of Walker and Guest (1952), have tried to measure the intimacy of such relationships. This study proposes that stronger bonds between workers develop where the chance for interaction is greater. Factors associated with high interaction should also be associated with intimacy. An index of the quality of social interaction was constructed from the following questions (underlined responses were given a

score of "1" and total scores were computed by adding scores on the six questions):

1. Apart from the job, how do you feel about daily contacts you have with your fellow workers? *Very satisfied or satisfied.*
2. If you had a very important and delicate personal problem (an undesirable disease, a touchy family problem, a very large debt you could not meet, etc.) and you wanted to confide in someone, are there, among your fellow workers, people in whom you could confide and be certain that they merited your trust and would keep your secret? *Yes.*
3. In general, how many really good friends would you say you have in your (work) group? *Two or more.*
4. Do you exchange visits with any of your neighbors? (If yes) Are any of these your fellow workers? *Yes.*
5. Do you get into discussions with others over economic and political issues? (If yes) With whom most often? *Mentions fellow workers.*
6. For whose views do you have the highest regard—family, close friends, union officials, fellow workers, foremen and supervisor, political party workers, clergy, other? *Mentions fellow workers.*

All questions were associated with the total index beyond the .001 probability level as measured by the chi-square test; the uncorrected coefficients of contingency fell between .296 and .586; and the median was .465. Therefore the index is fairly reliable and, I hope, a valid measure of the quality of interaction.

Table 10 shows that half or more workers

Table 9. Workers Who Took Greatest Advantage of the Opportunity for Social Contact on the Job (in percent)

Techno- logical Constraints	Complexity of Technology			
	(High)		(Low)	
	OLDS	FIAT	IKA	PAL
Assembling (high)	17	53	45	56
Machining	15	26	41	60
TIR	12	21	43	54
Craft (low)	4	12	41	54
Totals	12	34	43	56

Table 10. Quality of Interaction Among Fellow Workers (in percent)

Quality Index Scores	Complexity of Technology			
	(High)		(Low)	
	OLDS	FIAT	IKA	PAL
Low:	0-1	12	27	19
	2	28	25	30
	3	34	25	30
High:	4-6	26	23	20
				35
Totals		100	100	99
				100

in all countries scored three or higher on the index, evidencing the existence of strong social bonds. Indian and American workers had somewhat higher scores than Italians and Argentinians. Conditions other than the chances to interact provided by the technology of the work situation may account for intimate social bonds. Thus high seniority workers should have had most time to develop strong personal ties. However, only in OLDS was the relation of tenure and quality of interaction statistically significant. Perhaps those most content with their jobs are most disposed to develop intimate ties. Yet, no association was found between a six item index of work satisfaction, the index of quality of social interaction, and several measures of work interaction quantity.

The relationship of interaction quality to three indicators of interaction quantity was positive but weak (see Table 11). The association was strongest in OLDS where the technology was most complex and in PAL where it was least complex. With the exception of IKA, more workers having high

interaction also had more intimate social ties. Except for FIAT, those who took most advantage of chances to interact had higher scores on the index of interaction quality. Finally, the data also show a consistent but weak relationship between the work place density and quality of interaction. Obviously, other factors than technology affect the quality of social interaction, but the fourth hypothesis which posits a relationship between quantity and quality of interaction is weakly supported.

In-Plant and Out-Plant Interaction

The fifth hypothesis states that contacts with fellow workers outside the plant are a function of contacts in the plant. The question is whether bonds at work carry over into the community. To answer this, an index was constructed to measure the amount of worker social contact outside the plant. The following questions were asked:⁸

1. Do you travel to and from work with fellow workers?
2. Do you exchange visits in your neighborhood with fellow workers?
3. Do you discuss economic and political issues with your fellow workers more than with other persons?
4. Apart from relatives, did you spend time with fellow workers during your last vacation?
5. Apart from the union, are your work-mates members of organizations to which you belong?

One weakness of this index is that it includes two items found in the index of quality of social interaction. The data in Table 12 reveal little worker social contact off work; two-fifths to two-thirds had no outside contacts or only one. OLDS workers had the most such contacts, but only one-quarter had three or more. With the exception of IKA, the data in Table 13 show that those who talk to more mates at work tend to have more contacts off work. Also, those who work in high density areas tend to have more out-plant contacts. Under-

Table 11. Percentage Having Intimate Social Ties with Fellow Workers According to Three Indicators of Interaction at Work

Indicators of Interaction	Complexity of Technology			
	(High) OLDS	FIAT	IKA	(Low) PAL
Number Talk To:				
0-2	16	27	41	7
3-10	35	37	46	50
11+	48	36	13	43
Total	99	100*	100*	100
N	(79)	(70)	(63)	(92)
Possible Interaction Realized:				
Low	3	34	2	18
Medium	31	33	55	18
High	65	33	43	63
Total	100	100*	100*	99
N	(70)	(58)	(63)	(93)
Density:				
Low	31	19	27	18
Medium	20	12	37	37
High	49	70	37	45
Total	100*	101	101*	100*
N	(78)	(69)	(63)	(92)

*Chi-square not statistically significant at the 10 percent level.

⁸ These questions appear in abbreviated form. Positive responses were scored "one" and accumulated to form the index.

Table 12. Scores on Out-Plant Interaction with Fellow Workers (in percent)

Out-Plant Interaction Scores		Complexity of Technology			
		(High) OLDS	FIAT	IKA	(Low) PAL
Low:	0	19	31	14	19
	1	34	35	34	41
	2	24	24	34	29
High:	3-5	23	9	18	11
Totals		100	100	100	100

standably, the trend is weakest for IKA and PAL which were the least differentiated in density (see Table 6).

Plant Interaction and Anomie

Finally, I hypothesized that anomie is not related to the plant's or society's technological complexity and is inversely related to the amount and quality of worker contacts. Those who have more interactions and stronger ties on and off the job, should be more socially integrated and feel less anomie. Five-item Guttman scales of anomie (normlessness) were constructed whose reproducibility coefficients were above 0.85. The scale items referred to the prevalence of normlessness in the general society.

1. In the everyday problems of life, it is easy to know which is the right path to choose.
2. It is hard to rear children nowadays because what is right today is wrong tomorrow.
3. It seems that nobody agrees on what is right or wrong because everybody is following his own ideas.
4. It is easy to find agreement on what is morally right.
5. There are so many different organizations with different goals, that it is impossible to trust any of them.
6. The world of today is changing so fast that it is difficult to be sure that we are making the right decisions in the problems we face every day.

Only the first part of the hypothesis was supported: no relationship appeared between the plant's or society's technological complexity and anomie scores; the Indian workers had the highest median scores

(4.7) followed by the Italians (3.7), Americans (2.9), and Argentinians (2.0). The second part of the hypothesis was not supported because the anomie scores bore no statistically significant association to any of the eleven indicators of quantity or quality of interaction of the four plants. Although there were some weak associations, they showed no discernible pattern.

CONCLUSIONS

This paper hypothesized that the quantity and quality of worker interaction is conditioned by the complexity of the plant's technology and the degree of technological constraint imposed by work operations. The findings tended to support the predictions of this hypothesis. Although all possible influences on interaction were not considered, many social factors commonly thought to influence interaction did not do so (Form, 1971).

In general, the more workers controlled their machines and the greater the density of their workplaces, the greater their chance for social interaction, and the more they took advantage of that chance. Differences in interaction rates among workers who performed different operations were largest in plants with the most complex and highly developed technologies. The more special-

Table 13. Amount of High Social Interaction with Fellow Workers Outside of Plant by Amount of Interaction Within the Plant (in percent)

In-Plant Interaction	Complexity of Technology			
	(High) OLDS	FIAT	IKA	(Low) PAL
Number Talk To:				
0-2	19	25	48	11
3-10	40	29	36	49
11+	41	46	16	41
Total	100*	100	100*	100
N	(69)	(28)	(56)	(105)
Density Index:				
Low	32	29	29	21
Medium	19	7	37	34
High	49	64	34	45
Total	100	100	100*	100*
N	(68)	(28)	(56)	(105)

*Chi-square not statistically significant at the 10 percent level.

ized the technologies, the more distinctive was their impact on interaction. Although the evidence was not conclusive, quantity and quality of social interaction appeared to be related. Moreover, some carry-over was found of in-plant social ties to the external community. However, the impact of technology on interaction was strongest at work and decreased elsewhere. Finally, the amount of anomie bore no relationship to social interaction rates on or off the job. Why might this be so?

Typically a gap exists between the theory and measurement of anomie. Although anomie is thought to derive from the dehumanizing conditions of the industrial *work situation* (Blauner, 1964), the usual measures of anomie focus on workers' psychological responses to statements about the normative integration of the society. However Tudor (1972) found that degree of job complexity was not related to feelings of powerlessness among American men. More important, Seeman (1972:395-6) found in studying Parisian workers that their responses to their occupational community and to events in the broader society were independent. It is quite possible for workers to be highly integrated at work and feel powerless about events in society. This study showed that the quantity and quality of social interaction at work in all four plants was relatively high. Most workers were satisfied with their jobs and contacts with their workmates. The newness of industrialism apparently was not related to the quality or quantity of worker interaction. Perhaps even where the industry is new, the work plant may be the most integrated social system in the society.

Evidence mounts that, whatever the level of industrialization and whatever their past exposure to industry, workers' responses to the work situation are strongly conditioned by technology. Even where industry is young and workers are newly exposed to industrial discipline, factory social life is apt to be similar to that in mature industrial societies. This study's limited findings prompt one to reexamine two fashionable explanations of worker behavior; the human relations approach which emphasizes management's effort to humanize the environment, and the cultural approach which emphasizes conflicts

in values between the factory and the traditional community. This study suggests that the social world of the factory worker everywhere is conditioned primarily by the factory's technological environment. Historically, automobile technology has been singled out by both the left and the right as providing ideal conditions for alienation and anomie. This study of automobile workers in four countries shows no direct relation between social relations in the factory and feelings of anomie. Evidence mounts that we should modify or abandon technological explanations of alienation and anomie (Seeman 1972).

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THE DIVISION OF LABOR: A POST-DURKHEIMIAN ANALYTICAL VIEW *

THEODORE D. KEMPER

Queens College, City University of New York

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Though we quite properly value Durkheim's discussion of the division of labor, a close look shows that he limited his concept to specialization. I propose that the ineluctable condition of humans is interdependence and that this implies divided labor to complete the tasks that no one actor can do alone. Importantly, actors may or may not be specialized in their interdependent activity. It is possible to examine different forms of the division of labor, as well as properties of whole systems of divided labor. The method adopted here is a non-technical, discursive analysis based on graph theory, which deals with the abstract properties of arrangements of points and lines. This analysis reveals several properties of the division of labor relatively common in sociological usage as well as several that are relatively uncommon.

THE respect we accord Durkheim for *The Division of Labor in Society* is, of course, entirely warranted. Had he not argued so incisively there against the Utilitarians in general and Spencer in particular, the discipline of sociology might not have emerged as early and as forcefully. With compelling logic, Durkheim demolished the claims of the psychological atomists on the origins of society. He argued for the existential priority of society over the individual and, as far as can be judged from the history of ideas, won the debate. However, modern sociology is indebted to Durkheim not only for his victory over the atomists, but also for the concept with which he won. The division of labor, as an idea, was appropriated originally by the Utilitarians. Durkheim, in a great intellectual *tour de force*, expropriated it and demonstrated that it could serve sociological theory even better than it could Utilitarian self-interest.

Yet, at the same time that we venerate Durkheim's views on the division of labor, we ought not to embalm them as our last thoughts on the subject. Durkheim himself complained that "... the theory of the division of labor has made such little progress since Adam Smith" (Durkheim, 1933:46),

and his own work was intended as a corrective. Cannot the same be said for the state of systematic analytic interest in the division of labor since Durkheim?

The division of labor is a core concept in sociology. At a minimum it implies such additional concepts as interaction, goals, roles (i.e. differentiated functions), technology, integration, rules; yet, despite its conceptual fertility, it has received far less attention than it merits (cf. Gibbs and Martin, 1962). This statement may occasion some surprise, for much interest has been invested in the various spin-off concepts just mentioned, e.g. interaction, goals, roles, etc., and in occupational differentiation, which for many sociologists is virtually synonymous with the division of labor.

But I wish to suggest that despite this valuable work, sociologists have for long displayed relatively little interest in the division of labor as an analytic concept in its own right. Weber (1947:218-45), depending on the work of Bucher (1901), who was also cited by Durkheim (1933:46), undertook what might be viewed as a preliminary analytical treatment of the division of labor. But his work on the subject is little noted and appears to have had virtually no impact on the development of sociological theory. I would suggest that Durkheim's own treatment of the division of labor—and our respect for his accomplishment—obstructs full-fledged analytical attention to the concept.

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In the first part of this paper, I shall examine some of Durkheim's views on the division of labor, with special attention to their function within his own intellectual paradigm. I will propose that the division of labor had other than strictly theoretical interest for Durkheim and that this, in part, bears on our current lack of analytical attention to the concept. Furthermore, Durkheim himself relegates the division of labor to secondary status in sociological analysis. It will be seen, however, that his position is logically flawed. In succeeding sections I shall propose and apply an approach to the division of labor as a sociological construct that goes beyond Durkheim's important but limited treatment.

THE DIVISION OF LABOR AS METATHEORY

Saving France from Anarchy

Quite apart from Durkheim's theoretical contribution in his discussion of the division of labor, the concept also served him in two non-theoretical ways. I will use the term *metatheory* to refer to these non-substantive uses. First, as Nisbet (1952) pointed out, Durkheim responded conservatively to the disintegration of French society following the Revolution of 1789. Though personally liberal (see Neyer, 1964; Richter, 1964; and Coser, 1964), Durkheim believed that what suited the individual and what suited society were not always congruent. Society, as a reality *sui generis*, had its own requirements, he believed; and these were principally mechanisms of integration. Intellectually confronting the instability of French society during the century following the Revolution, Durkheim's major interest was to discover the grounds for social solidarity and the moderation of conflict. In the division of labor, Durkheim saw a solution to the problem of social integration in a society racked both by the overthrow of the traditional political order and the dislocations unleashed by industrial growth.

Just as Marx applied analytical concepts to immediate social questions, Durkheim, I suggest, used his principal concept to answer a social question. Indeed, his discussion of the "forced division of labor," in which he announces that "the task of the most advanced societies is, then, a work of justice" (Durkheim, 1933:387), as well as the preface

to the second edition of the work, go beyond merely the tone of a superior political pamphlet. The latter, especially, is a programmatic attempt to mobilize occupational specialties into solidary groupings of political significance, transcending and even replacing traditional territorial solidarity and obviating a much-feared class solidarity (Giddens, 1970) as a basis for political action.

Thus, the division of labor served Durkheim at least in part as a metatheory expressing his conviction about a particular historical and political need. To state this is not to condemn him. It is to advise that he had other than only analytical or substantive interest in his concept.

Saving Sociology from Atomism

Durkheim also used the concept metatheoretically to stake out a claim for an independent discipline. Both in *The Division of Labor in Society* and *Suicide*, Durkheim demonstrated his point by means of concepts in some way sacred to his opponents. In *Suicide* he brilliantly analyzed an ultimate act of individual behavior. It was his genius—and sociology's great gain—to show that even so individual an act as suicide varied with the degree of social integration and regulation. How better to confound those who saw no utility in sociological analysis! This was Durkheim's second successful application of this method. Earlier, in *The Division of Labor in Society*, he had shown how one of the Utilitarians' principal concepts not only did not derive from the pursuit of individual self-interest, but was not even "the fundamental fact of all social life" (Durkheim, 1933:277).

The Utilitarians held that society was founded on self-interest and the propensity of individuals to exchange the products of their labors (Smith, 1937). For Durkheim this was simply a wrong-headed reversal of the facts. "Collective life is not born from individual life, but it is, on the contrary, the second which is born from the first" (Durkheim, 1933:279). Spencer, too, is deemed in error for believing "that primitive units pass from a state of perfect independence to that of mutual dependence" (Durkheim, 1933:279). Durkheim's answer is conclusive here. "With autonomous individualities, as are imagined, nothing can emerge save what is individual, and, consequently,

cooperation itself, which is a social fact, submissive to social rules, cannot arise. Thus, the psychologist who starts by restricting himself to the ego cannot emerge to find the non-ego" (Durkheim, 1933:279).

Had Durkheim left the matter at this point, he would have retired from the field the irreproachable winner of the debate. But he had one last bit of tidying-up in his argument with the Utilitarians; and here, I believe, he committed a logical error which has impeded the development of sociological theory. In making it, Durkheim revealed his essentially subsidiary interest in the division of labor; for while specialization was the principal sociological concept for the Utilitarians, it was a good deal less than this for Durkheim.

He writes:

Far from preceding collective life, [differences between individuals] derive from it. They can be produced only in the midst of a society, and under the pressure of social sentiments and social needs. That is what makes them essentially harmonious. *There is, then, a social life outside the whole division of labor, but which the latter presupposes . . .* there are societies whose cohesion is essentially due to a community of beliefs and sentiments, and it is from these societies that those whose unity is assured by the division of labor have emerged (Durkheim, 1933:277, italics added).

Here, Durkheim, who exposes the flaw in the argument that society resulted from aggregating isolated but differentiated individuals, himself argues fallaciously that, *therefore*, the division of labor could only have emerged from an earlier form of association where it was absent. Durkheim failed to consider that the ineluctable condition of humankind is interdependence and that the division of labor could have existed all along.

We can attribute the error in part to his adopting the then newly popular evolutionary model from biology. Essentially, this disposed him to believe that what is manifest could not have existed at the outset.

But the evolutionary aspects of Durkheim's argument prove to be the weakest part of his discussion. Indeed, to accept his view that social solidarity depends first on the adhesion of those of similar sentiments, and later on functional interdependence, would not necessarily imply that functional

interdependence didn't also prevail at the outset. Durkheim's logic here was supported by bad data, as Merton (1934:324) pointed out in his lengthy review of the first English edition of Durkheim's work on the division of labor. Schwartz and Miller (1964) more recently cast doubt on Durkheim's argument by analyzing legal sanctions in societies with varying degrees of divided labor (see also Bendix and Roth, 1971:80).

Yet, Durkheim must have regarded the capstone of this argument against the Utilitarians to be that their concept, the division of labor, was only the evolutionary product of a prior sociological foundation. They were to be, thus, classically hoist by their own petard. But, when a thinker relegates a concept to secondary status, he is not likely to develop its analytical implications. I suggest that Durkheim's preoccupation with winning the battle of metatheory against the Utilitarians led him into his own fallacy and exhausted his interest in the concept.

Contrast Durkheim's treatment and use of the division of labor with that of the concept of suicide. His theoretical, as opposed to polemical, interest in suicide led him at least to substruct the concept (Barton, 1955). He finds four types of suicide and relates these to four types of societal integration and regulation. The division of labor did not have the same analytic interest for him. He does assert that it creates social solidarity; but that proposition, I suggest, was crucial principally for its role in the polemic against the Utilitarians.

If taken seriously, Durkheim's position on the secondary significance of the division of labor is a barrier to analytic attention to the concept. Indeed, for many sociologists (perhaps best exemplified by Talcott Parsons, 1937, and the "value" position he espouses) Durkheim's significance is precisely that he treated "the community of beliefs and sentiments" (Durkheim, 1933:277), that is, common values, as "the fundamental fact of all social life." But if Durkheim's argument is flawed, then we may bypass his view that the division of labor is a secondary form of social organization, and we may examine the division of labor as a concept worthy of analytical development in its own right.

AN ANALYTICAL APPROACH TO THE
DIVISION OF LABOR

I have suggested that Durkheim's interests and arguments, valuable as they may be for some purposes, have obscured a view of the division of labor that may prove highly useful in sociological analysis.¹ In this section I will suggest a way to undertake a conceptual analysis of the division of labor which moves beyond Durkheim.

This discussion assumes that voluntary human association always and everywhere implies interdependence (Hawley, 1950:177) and divided labor. Interdependence means that humans cannot supply their wants and needs exclusively by their own efforts; thus they are inescapably joined with others to augment their strengths and purposes. To attain the common goal, or complete the common task, each actor necessarily performs a different portion of the common effort. Human activities as varied in scope, character, and purpose as conversation among friends, two men rolling a log up a hill, a tribal hunt for buffalo, going to the moon, and making love, involve interdependence and a division of the task into separate parts. Given the pervasiveness of the division of labor,² what type of conceptual analyses can we undertake that go beyond Durkheim?

First, we can examine different *forms* of the division of labor. Although Durkheim (1933:124 and 212) was aware that it can take different shapes, his major treatment implies that specialization is the only way to divide labor. Yet consider the following two passages:

To cooperate, in short, is to participate in a common task. If this is divided into tasks qualitatively similar, but mutually indispensable, there is a simple division of labor of the

first degree. If they are of different character, there is a compound division of labor, specialization properly called (Durkheim, 1933:124).

... when men unite in a contract, it is because through the division of labor, whether simple or complex, they need each other (Durkheim, 1933:212).

We can only wonder why Durkheim did not attribute the solidarity of early social aggregates to the "simple division of labor of the first degree," which surely characterized such social groupings as much as did a "common conscience." Had Durkheim developed the implications of this passing insight on formal differences, sociological theory might be quite different today.

Others who have considered the problem of the form of the division of labor include Spencer (1896); Bucher (1901); Weber (1947), who was influenced by Bucher; Thomas (1957); Blau and Scott (1962); Gibbs and Martin (1962); Weick (1965); and Thompson (1967). Although their work will not be reviewed here, it points in general to three forms that the division of labor may take: a *parallel* form, in which the actors do similar things and pool their efforts (i.e. Durkheim's "simple" division of labor); a *sequential* form which concatenates the actors' efforts; and a *reciprocal* form in which actors do different things and pool their efforts. In the sequential form actors may do similar things, as in a bucket brigade, or different things, as in an auto assembly line. While this tripartite formulation does not do justice to every nuance of the various discussions of form, it does indicate the general direction of such efforts.

A second approach to the analysis of the division of labor will be taken here. In whatever form, the division of labor entails a set of coactors coordinated with a set of tasks or functions (treated as positions to be filled by actors) whose particular arrangement in the workflow may be viewed as a system. The system itself has properties, and the analytical task will be to discover these properties.

Both form and system analyses are based on the general consideration that as the form and the system properties of concrete divisions of labor vary, the outcomes for the actors will also vary. This should be true whether we consider *global* outcomes such

¹ The division of labor *per se* will be dealt with here, not what has been called "bases of the division of labor" (Gibbs and Martin, 1962), e.g. age, sex, race, etc.

² Coercive interactions, such as robbery or rape, are excluded from this analysis. Even in contexts of voluntary interdependence there is often coercion, as in economic and family relations. Thus, the husband acts unlovingly, and the employer demands more than the contract. Despite such breakdowns—Durkheim's rubric for such cases is the "forced division of labor"—usually these relationships involve much voluntarism and interdependence.

as the probability of winning a war, the rate of urbanization, or the rate of modernization; *structural* outcomes such as the distribution of authority in a group, the allocation of status, or the potential for mobility to elite positions; or *individual* outcomes such as interest in one's work, preferred type of leisure and recreation, or the probability of using physical punishment on one's children.

It will be useful now to offer a provisional definition: *The division of labor is an arrangement of actors whose coaction is required to complete a common task or attain a common goal.* It is seen as a social ordering of human effort where the actors are guided in their contributions by their conception of the common goal or task and by the activities of their fellow actors (see Weber, 1947:88, for the definition of "social" action). For heuristic purposes the definition imposes few restrictions. For example, nothing is specified about the degree of task specialization of the actors.

If actors are engaged in coaction to complete a task, there is inevitably communication between them. Durkheim (1933:276), for example, writes that for the division of labor to exist "it is indispensable that the two specializing parts be in constant communication." For that matter actors engaged in the "simple" division of labor must also

communicate at some point. Otherwise, tasks could not be specified or actors allocated to the activities required by the task. The messages need not be verbal. They may be tactile, as in sexual intercourse; but they nevertheless allocate and integrate the necessary activities. While actors may communicate about more than the given task, their communication includes the task at a minimum. Thus, the arrangement of actors in the division of labor coincides in an important sense with the pattern of communication between them, and this fact will be seen to have an heuristic value.

Communication Networks and Graph Theory

A body of literature, beginning with Bavelas (1948), and including Leavitt (1951), Heise and Miller (1951), Shaw (1954), Mulder (1960), Harshbarger (1971), investigates the empirical consequences of different arrangements of actors in the flow of communication. Much of the work examines networks of five positions; and, since this number permits sufficient variation for our purposes, several networks of this size are shown in Figure 1.

The visual depiction of a communication network translates easily into a system of divided labor, with each point representing a particular actor, or set of activities defined

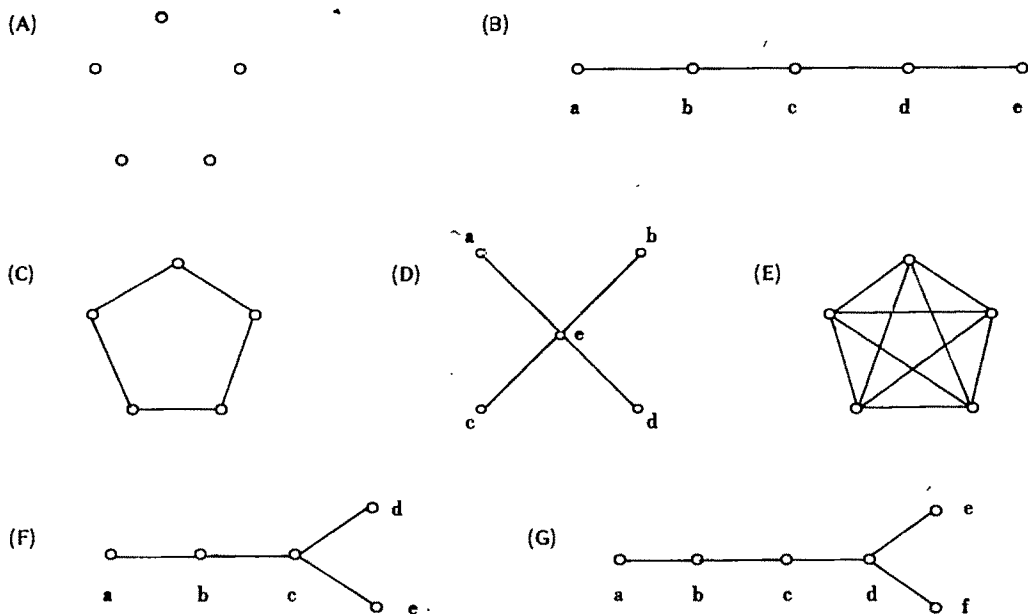


FIGURE 1. SOME TYPES OF FIVE AND SIX POSITION NETWORKS.

as a position in the network, and each line defined as a flow of work. Note also that the visual depiction brings into focus certain properties of the division of labor as a system which might be difficult to detect or describe by verbal means alone. Thus, it will be easier to discuss the dimension of centrality (to be treated below) with the aid of these network depictions than without them. These graphic representations have yet another advantage. A type of mathematics exists devoted to analyzing just such networks.

Graph theory treats the abstract properties of sets of distinct points, the joining of any two of which produces a line.³ By coordinating empirical entities, such as actors or functions, to the points, and relationships, such as flow of messages, or work, to the lines, one can develop mathematical models of empirical systems (see Harary, Norman and Cartwright, 1965:1-4). A growing literature attests that problems of "structure" and "organization" in social science are amenable to analysis by means of graph theory.

Even relatively straightforward mathematical treatments now include pages on applications to "human science" (Busacker and Saaty, 1965) or "psychosociology" (Kaufman, 1967), or "social science and psychology" (Marshall, 1971). Flament (1962) and Harary, Norman and Cartwright (1965) are specifically concerned with social science applications, as are Cartwright (1959), Evan (1960), and Harary (see references to many papers in Harary, 1969). Yet it is remarkable that while these authors and others have dealt with status measurement (Harary, 1959a; Oeser and Harary, 1962; Friedell, 1967); clique formation (Holland and Lienhardt, 1970); hierarchy and authority in a group (Oeser and Harary,

1962); preferences in ordering objects (Harary, Norman and Cartwright, 1965); the psychological theory of balance (Harary, 1959b); models of urban problems (Marshall, 1971)—none has explicitly viewed the division of labor itself as worthy of graph theoretic consideration.⁴

I suggest, for heuristic purposes, that since a graph may formally represent a concrete system of divided labor, a study of the abstract properties of graphs should also suggest empirical properties of the division of labor. The results of the work to be presented are not technical in the mathematical sense—no postulates are offered, nor theorems derived—nor are they theoretical in the sociological sense, since no testable propositions are presented.⁵ Rather, they are *systematically* conceptual, because the attributes of the division of labor to be discussed may be treated as part of a system *at a single level of abstraction*. This result is an important basic step in developing systematic theory, since the properties to be discussed are not merely *ad hoc*, as is often the case with sociological concepts, but are attributes of a system with a single deductive base.

SOME ANALYTICAL PROPERTIES OF THE DIVISION OF LABOR

For analytical purposes, I have considered only the technical workflow in this examination of properties of systems of divided labor. I have omitted an important, but nonetheless analytically separate aspect of concrete groups whose labor is divided, namely the structure of authority. I do not mean to slight the concept of authority, but to treat

³ In addition to *graphs*, which are collections of points with or without lines connecting them, there are directed graphs, or *digraphs*, which are graphs having directional arrows added to the lines; signed graphs, or *sigraphs*, in which the lines are numerically weighted according to some criterion, e.g. distance, flow capacity, etc.; and *trees*, which are graphs in which all points are connected to at least one other point by a line, and, further, in which one cannot move to another network point and then return to the starting point without passing through all previously traversed points. Thus, in Figure 1, networks A, D, F, and G are trees, while B and E are not.

⁴ This is all the more remarkable because a number of outgrowths and developments from the division of labor have been analyzed by graph methods, but not the division of labor itself. Even in disparagement, one author recognizes the primary character of the division of labor in his remark that "it is treated first not because it is believed to be the most important mechanism, but because it somehow seems to be logically prior" (Price, 1968:45).

⁵ Only a non-technical treatment in discursive language will be given here, since a rigorous discussion would require technical definitions for which space is not available and which would defeat the purpose of demonstrating to the preponderance of sociologists not mathematically trained the wide analytic scope which even a simple graph theoretic analysis permits.

the pattern of technical workflow as analytically primary (cf. Price, 1968; Lanzetta and Roby, 1956:95).

Furthermore, the analysis is insensitive to the micro-macro distinction. Both levels, however, are comprehended here in that the actors in a system of divided labor may be individuals, corporate groups, or even societies. Also, following the useful method of Oeser and Harary (1962), I distinguish, at first, between a set of actors and a set of tasks. The particular way of mapping actors into tasks gives rise to various forms and properties of the division of labor. The properties suggested are not considered exhaustive. Harary (1969:11) explicitly says of "invariants of graphs" (these are properties which indicate whether one graph is isomorphic with another): "No decent complete set of invariants for a graph (with more than three points) is known." Let us consider now some system properties of the division of labor as suggested by the properties of graphs.

(1) *Number or Size*

Probably the most obvious property of a graph is how many points it contains. Since we begin with a set of actors and a set of tasks, two numbers are generated, namely *size of group* and *number of tasks*. The manner in which the number of actors and the number of tasks are related gives rise to several properties of the division of labor.

(a) *Non-specialized division of labor*: Even if each actor is ostensibly doing the same thing, as in Thomas's (1957) illustration of picking apples in an orchard, each actor does something which contributes to the overall goal. This non-specialized type of divided labor is shown in Figure 2(A). The points representing actors are connected, indicating

their interdependence; but the points representing tasks are unconnected, since the technical workflow is immaterial and there is no relationship between doing one task and doing another. Despite this, and despite the fact that each actor does approximately the same unspecialized task, his participation is necessary for the whole task to be accomplished; e.g., the apples must be picked before the frost.

(b) *Specialized division of labor*: When the graph of actors and the graph of tasks are coordinated as in Figure 2(B), we have moved to a case of specialized division of labor. Two differences exist between Figure 2(A) and Figure 2(B). First, directed lines connect the task points, indicating the contingency of the technical workflow. Second, both the actor and the task points are labeled. The latter tells us that there is one-to-one correspondence between the graph of actors and the graph of tasks with respect to points with the same labels (Harary, 1969: 10). In sociological terms we have here *actor specialization* and *task differentiation*. When specialization-differentiation has occurred, we can represent the division of labor by graphs such as those in Figure 1, in which the graphs of actors are absorbed into the graphs of tasks with which they are coordinated.⁶

Specialization, however, is a matter of degree. Thus, each actor may perform a unique task in the division of labor, as shown in Figure 2(B); or the degree of specialization-differentiation may be less than complete. This is represented in Figure 2(C), where

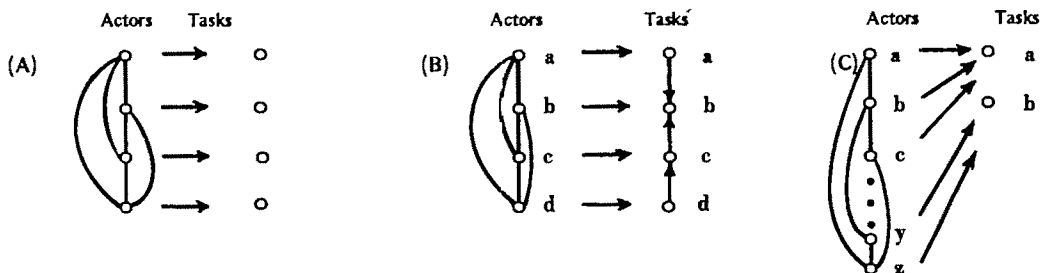


FIGURE 2. THREE POSSIBLE COORDINATIONS OF ACTORS AND TASKS.

⁶ The networks of Figure 1 are shown as graphs, which is their most general form. Where it becomes necessary to view a network as a digraph this will be apparent from the context in which it is discussed.

it may be seen that a number of actors are coordinated with task *a* and a number are coordinated with task *b*. This potential variability in specialization-differentiation has given rise to a number of conceptualizations.

Childers *et al.* (1971) distinguish between two forms of what they call "structural differentiation." First is "external structural differentiation," which is simply the number of different tasks, activities, or occupations, in a group. This is very frequently used as a measure of the division of labor (e.g. Blau, 1970; Mayhew *et al.*, 1972). Childers *et al.* also propose "internal structural differentiation," which is the ratio between the number of different tasks and group size. This measure has been used very little, but it is akin to the concept of "functional dispersion" or "concentration" proposed by Gibbs and Martin (1962); and further used by Labovitz and Gibbs (1964), Gibbs and Browning (1966), Rushing (1968), and Samuel and Mannheim (1970). Functional dispersion is measured by the ratio between actual and maximum possible variation in the assignment of actors to differentiated tasks (see Rushing and Davies, 1970; and Samuel and Mannheim, 1970, for derivations of this measure).

It can be seen that a good deal of sociological interest in the division of labor has centered in the relationship between size of group and number of tasks, with a specific interest in explaining the exact degree of specialization, either in the absolute sense of internal structural differentiation (Childers *et al.*, 1971) or in the relative sense of functional dispersion.

Spencer (1896) proposed that as the number of actors in a group increases, specialization tends to occur, i.e., the famous transformation from "homogeneity" to "heterogeneity." Durkheim (1933) took issue with Spencer for suggesting that increase in group size could alone account for actor specialization. He proposed the concept of "moral density," which in conjunction with population density, produces actor specialization to moderate the effects of increased competition for a share of the output of the task system. Durkheim also proposed three factors which enhance moral density and thereby foster actor specialization: (1) concentration of population; (2) formation

of cities; and (3) improvements in communication and transportation.

With regard to the first two factors, which may be subsumed under the general rubric of *urbanization*, the available research seems to support a reversal of Durkheim's formulation. Gibbs and Martin (1962:677) report that "a high degree of urbanization depends on the division of labor, technology, and organization." This conclusion, based on data from forty-five countries, was further supported by U.S. data for the period of 1900 to 1950 (Labovitz and Gibbs, 1964). Land (1970) also finds that changes in the degree of urbanization depend more on occupational differentiation than does differentiation on urbanization.

With regard to the factor of improved communication and transportation, Durkheim would appear to be on firmer ground. Yet, it is difficult to see how such improvements could have occurred without some prior change in the set of tasks, specifically through the development of technological improvements which permit greater productivity through specialization, therefore greater capacity to sustain the increase in group size (cf. Hawley, 1950:193).⁷

Although the initial theory relating group size and actor specialization was developed at the level of societies, much interest in these two variables has prevailed at the level of organizations, in which both size and differentiation are presumably more subject to the actors' rational discretion, particularly those with authority. A relatively consistent set of results from recent work suggests that differentiation tends to increase rapidly when small groups increase in size, but then the rate of increase diminishes. These data produce a logarithmic function curve. Blau (1970) reports the logarithmic relationship

⁷ An implicit question here is the degree to which actor-specialization *per se* contributes to greater productivity, as opposed to the increment due to improved technology. It would appear that technological development is more important by far, as even Adam Smith (1937) recognized. He moved very quickly in his discussion of the advantages of the division of labor from "improved dexterity" and "saving of time" to "the application of machinery." We also have here the curious suggestion that what explains specialization is specialization itself, in this case in the area of communication and transportation.

when he plots size of local office against the number of occupational positions in the universe of 1201 local offices of all state agencies in the U.S. dealing with employment security. Mayhew *et al.* (1972) also find an adequate fit between size and task differentiation in three types of U.S. Coast Guard units by the same logarithmic curve—based in this case on a derivation from the “principle of least effort” proposed by Zipf (1949). While Blau (1970) and Mayhew *et al.* (1972) reach the same empirical conclusion concerning the shape of the relationship between size and differentiation, an outstanding theoretical issue is which variable is causally prior, with Blau, and recent work by Meyer (1972), supporting size, and Mayhew *et al.* supporting differentiation. Thus, Durkheim’s view that increases in group size lead to task differentiation and actor specialization have received more support in studies of organizations than in studies of society. This poses the question as to whether different theories are required to account for the results at the two levels (cf. Mayhew *et al.*, 1972:754–5).

In this regard, it is useful to consider that in organizations, as opposed to societies, it is likely that the empirical relationship between the number of actors and the number of differentiated tasks results more from discretionary decisions than from crescive accumulation. Indeed, organizational decisions are often made by those who perhaps follow what they have read to be “good practice.”⁸ This may involve both technological as well as ideological considerations (cf. Bendix, 1956). Thus, within certain limits no *necessary* relationship exists between group size and the degree of specialization. For example, task differentiation could proceed up to the limits of group size. When this degree of specialization also includes a minimal set of activities as part of the task—e.g. the eighteen different specializations that Adam Smith (1937) reports in the manufacture of

pins—new considerations emerge about the actor, as distinct from the output of the actor. A large literature attests to the alienating and demoralizing effects of extreme refinements in the division of labor (e.g. Smith, 1937; Marx, 1964; Bucher, 1901; Weber, 1947; Friedmann, 1964). “Job enlargement,” (Walker and Guest, 1952) is one of the solutions proposed, i.e. de-differentiating the task and de-specializing the actor. Marx (1964) was perhaps the first to propose a radical de-specialization of actors as a general solution to problems of stratification as well as the division of labor.

(2) *Centrality*

The networks of Figure 1 may be examined for a pattern which has received perhaps the most attention from students of communication, namely *centrality*.⁹ It is apparent that networks B, D, F, and G in Figure 1 have what may be called “central” positions, if we imagine the workflow as moving from the more peripheral points toward the less peripheral points. In networks C and E, by contrast, all points appear to be equally peripheral. Centrality, which has an immediate, intuitive appeal, and appears to fall quite easily in view, is a somewhat more difficult dimension than it seems.

First, there is what has been called a *central point* (Harary, 1969:35). This is the point whose distance from the point farthest from it in the graph (measured by the number of lines to be traversed) is shorter than the corresponding maximum distance for any other point in the graph. There may, in fact, be more than one such point. For example, in Figure 1(G), point c is central, since it is two units distant from the farthest point from it; while points b and d, for example, are three units distant from their farthest points. In Figure 1(F), by contrast, points b and c are both central, because the farthest distance for both is shorter than the farthest distance for any other point.

Harary (1969:36), however, introduces another distinction, namely the *centroid point*. This is defined as the point from

⁸ Starbuck (1965a:500), observing that certain empirical studies in the organizational literature may have “descriptive value,” suggests that “one reason for this may be that businessmen believe in the ‘principles of management’ (Newman and Logan, 1955:20–1).” The longterm value of theories of organization which are based on descriptive studies of practice is thus questioned.

⁹ The remaining properties to be discussed will assume absorption of actors into tasks so that only graphs representing “positions” will be necessary, as in Figure 1.

which the maximum number of lines branching from it is smaller than the maximum number of lines branching from any other point. As with central points, centroid points may be or may not be unique. In Figure 1(G), points c and d are centroid points; while in Figure 1(F) only point c is a centroid point. Centroid and central points need not coincide, but, if not, will more than likely be adjacent. Beauchamp (1970) has proposed a measure for a system's relative centrality, based on the central-point notion, which equals one when all members of a system are directly in contact, and less than one, when they are not.

Another graph property having important empirical implications especially when associated with centrality is the *cutpoint*. This is a point which, if removed, would cause the graph to decompose into separate graphs. Thus in Figure 1(B) points b, c, and d are cutpoints which could lead to decomposition into separate graphs; while in Figure 1(D) removing point c would lead to the graphs decomposing into a set of unconnected points as in Figure 2(A). Complex multiposition networks may have numerous cutpoints. Positions which are both central and cutpoints are especially important. We would expect them to be concerned with problems of the system as well as with the task being undertaken. One such system problem is authority, and the evidence is strong that centrality is strongly related to authority; specifically, the more central the position the more often it is seen as the locus of authority (Hopkins, 1964).

Much experimental literature examines the consequences of centrality for such dependent variables as productivity, satisfaction, knowledge of group structure, emergence of a leader and the like (e.g. Leavitt, 1951; Heise and Miller, 1951; Bavelas, 1953; Shaw, 1954; Guetzkow and Simon, 1955; Lawson, 1965; Watson, 1965). Hopkins (1964) in his review of the literature finds centrality highly associated with influence and leadership. Emerson (1970) also examines centrality as a determinant of high position in the stratification system. The results of Grusky's (1963) study of baseball teams, in which he finds that infielders are chosen as manager at greater than expected frequencies, is also in this pattern.

Infielders are, by far, more central in the interaction network than outfielders.

Leavitt (1951), in addition to showing that groups whose division of labor has a central position are more productive, finds that satisfaction with one's position in groups with a central position is much lower if one is in a peripheral position in them, as well as lower than the satisfaction of workers in groups having no central position. From another perspective, Berkowitz (1956) shows that occupying the central position in a division of labor imposes requirements which appear to override the effects of personality. After the third trial in a cooperative task, individuals rated low in ascendancy and leadership performed as well as individuals rated high in ascendancy. This outcome was unexpected, because the central position would appear to impose demands for behavior which is characteristic only of high ascendant persons.

Lest some "macro"-oriented sociologists regard centrality as a concept useful only in small-group analysis, I would stress that since centrality is an attribute of a system of divided labor, *regardless of type of unit*, it should be of general interest to sociologists. Geographers, for example, have noted the influence of centrality on urban concentrations and applied graph theoretic models to this problem (see Olsson, 1965, for a review). The work of Gibbs and Martin (1958; 1962) also implies centrality as an underlying feature of how location and technological efficiency are associated with urbanization. Labovitz and Gibbs (1964) discuss the need to overcome the "friction of space," a concept which seems directly connected with the location of important activities in minimally distant, or central positions.¹⁰

¹⁰ Centrality, which is clearly an analytical property of a system of divided labor, was not introduced into social scientific discourse by a sociologist, but rather by an M.I.T. psychologist named Bavelas (1948), after his study of the work of Kurt Lewin, another M.I.T. psychologist. Most work done with the concept of centrality has been done by psychologists working in the small groups area. The history of ideas must record that an important analytical dimension of the division of labor, which is squarely within the conceptual domain of sociology, was launched and largely investigated by psychologists.

(3) *Isolation*

Continuing the analysis of points in a graph, let us consider the number of what are called "endpoints" in a system, relative to the system size. An endpoint (Harary, 1969:14-15) is a point or position having only a single line leading to or from it. Thus, in Figure 1(B), points a and e are endpoints. A system with a relatively large number of endpoints, such as network D in Figure 1, is also more likely to have more marked centrality than a system without endpoints, such as Figure 1(C) or (E). The graph property of endpoint strongly suggests the more general concept of isolation, which is of interest since it has been identified as a component of alienation (Seeman, 1959). Positions in the division of labor may be more or less isolated from each other. Some consequences of isolation are found in the work of Blauner (1962) who examined four different technological systems, or divisions of labor, for the degree to which isolation characterizes positions in each system. In both craft and continuous-flow technologies, worker isolation was found to be relatively low, and this condition was associated with greater satisfaction with work and integration with the work-setting than in more isolating systems.

Isolation *per se* need not diminish satisfaction. Some (e.g. Mills, 1951) have waxed rhapsodic on the gratifications of craftsmanship—a relatively isolated activity. Rather, it may be hypothesized that it is being an endpoint in a relatively connected system which is potentially dissatisfying. Occupants of endpoint positions in Leavitt's experiments (1951) were least satisfied with their work. These would include such positions as a, b, c, and d, in Figure 1(D) and positions a, d, and e, in Figure 1(F). By contrast, occupants of any of the non-endpoint positions in Figure 1(C) expressed greater satisfaction with their tasks.

But we must ask also whether the task is bounded and entirely internal, or whether it contacts external elements. For example, travelling salesmen are endpoints, as is intended here; but they continually contact clients and prospects. Nelson Foote (private communication) suggests that an intuitive sense of the potential difficulties of

being an endpoint—even though one was in contact with clients—led to the early Christian practice of sending apostles to proselytize in pairs.

(4) *Connectedness*

Thus far, I have discussed only the points of a graph. But lines also have implications for the division of labor. As with points, the most obvious question is simply how many there are, in this case relative to the number possible. To answer this involves the concept of connectedness (Harary, 1969). Connectedness is differently defined, if one is dealing with a graph or a digraph (Harary, 1969). For graphs, the term denotes the minimum number of lines whose removal results in a disconnected graph, i.e., simply a collection of disconnected points, as in Figure 1(A). In a digraph (see footnote 3), connectedness may be *strong* or *weak* (Harary; 1969:199). Strong connectedness means that each pair of points are mutually reachable through a path consisting of a sequence of points and properly directed lines. Weak connectedness, on the other hand, means that every two points are mutually reachable, because there is a sequence of points and lines that may be traversed between them; but the arrows appended to the lines need not face consistently in one direction.

Strong connectedness apropos of the division of labor implies that the total workflow engages all actors. Figure 1(E) is such a system, and a group discussion is a concrete example, whether in a conference room or a political legislature. All actors' input is available to all other actors in the course of such discussion. Degree of connectedness has been a critical variable in many studies of task efficiency. Leavitt's original findings (1951), which suggested that minimal connectedness via a central position leads to more efficient group performance, have been supplemented by the work of Heise and Miller (1951) and Shaw (1954) which showed that efficiency varied with connectedness as a function of the type of task. Guetzkow and Simon (1955) and Guetzkow and Dill (1955) have also found that the completely connected network was as efficient as the minimum-channel network, if

given an opportunity, before task operations, to devise a suitable workflow plan. These results have been replicated by Carzo (1963) and Shure *et al.* (1962).

Blau's (1956) delineation of types of group goals (tasks) involving the need for efficiency, satisfaction, or policy, suggests variable requirements for strong connectedness. Highly structured problems requiring efficient, rapid solutions seem best handled by systems with low degrees of connectedness, especially mediated by central positions. Such problems are the most amenable to "bureaucratization" (Thompson and Tuden, 1958). Less structured problems, e.g., legislative decisions, or scientific research, invite systems with a higher degree of connectedness, less mediation by central positions (Lawrence and Lorsch, 1967). Groups devoted to providing "intrinsic gratification" (Blau, 1956) and to policy formation, seem to invite especially high degrees of connectedness. For the latter, Robinson (1962) found, in studying relations between Congress and the U. S. State Department, a positive correlation of $r = +.41$ between a congressman's satisfaction with the information flow (i.e., was he receiving enough information) and his favorable vote on a State Department appropriation. (We must include the possibility that the content of the communications was also an influence). More recently, Blain (1971) has proposed "interaction chain length," a path-length measure related to connectedness, as an alternative to the Parsonian Four-Function Paradigm.

CONCLUSION

I have suggested that Durkheim treated the division of labor less as an analytical construct with properties to be investigated, than as a unitary construct, specialization, whose metatheoretical significance for him possibly exceeded its substantive import. To move beyond Durkheim's analysis, it is necessary to view the division of labor as a concept worthy of development in its own right. I have suggested here a mathematically-based method of deriving properties of the division of labor treated as a system in which actors and tasks are coordinated. The interrelations between group size and num-

ber of tasks were found to comprehend such properties as actor specialization, external and internal structural differentiation, and functional dispersion. In varying degrees these properties have received attention from sociologists, since they all turn on the notion of specialization emphasized by Durkheim. Other properties derived by the method, such as centrality, number of cut-points, isolation, and connectedness, have been relatively neglected, since they are not measures of specialization, but, rather, reveal how specialization itself may be organized.

The properties derived here do not, of course, exhaust the analytical dimensions of the division of labor, but do show the value of trying to model it by graph methods. Even the relatively non-technical approach taken here has produced not only a number of common properties of the division of labor, but also several not usually treated by sociologists. Of fundamental value for systematic theory development is that all the properties are derived from a model with a single deductive base.

If, as Price (1968) suggests, the division of labor is "logically prior," then the greater the number of its analytical dimensions available to us, the more likely we are to explain subsequent outcomes such as the structure of authority, the distribution of status, the satisfaction of participants, and the productivity and effectiveness of the division of labor as a whole.

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DIFFERENTIALS IN MARITAL INSTABILITY: 1970 *

LARRY L. BUMFASS AND JAMES A. SWEET

University of Wisconsin, Madison

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Using national data on white ever-married women under forty-five, differentials in marital instability are examined for several of the wife's characteristics at first marriage and for the couple's combined age, education and religion. Dummy variable multiple regression is used to adjust for the effects of differing durations since first marriage and to obtain effects for each variable net of other variables. With other variables controlled, an inverse age at marriage-instability relationship persists; and differences in marital stability by education appear largely attributable to differences in age at marriage by education. Other characteristics we considered are the wife's religion while growing up, whether she grew up on a farm, whether she lived with both parents at age fourteen, whether she was pregnant before her first marriage and whether her first husband had been married before. When we included the husband's variables, we found husbands age at marriage and education to have a negative relationship with marital instability. Higher instability for intermarriage is found among couples divergent in age or religion; only extreme differences in education are associated with higher instability.

DATA from the 1970 National Fertility Study remedy several inadequacies that have long hindered the study of marital instability. For the first time it is possible to examine differentials in marital instability for a national sample with data on the characteristics of both spouses at marriage (including their religion) and to include separations, as well as divorces, in the definition of instability.

The 1970 National Fertility Study differed from its predecessors by extending the sample universe from currently married women to all ever-married women under forty-five. Although this change was made to facilitate the analysis of fertility, it fortuitously provides an appropriate sample for studying marital instability. With these data we can examine the stability as of 1970 of all first marriages to women under forty-five who had married by 1970. To take advan-

tage of this opportunity, a limited number of questions about the respondent's first husband were asked of respondents no longer married to their first spouse. The screening procedure which determined whether separations were the consequence of marital discord enabled us to include separations in our measurement of instability. As Sweet detailed in another paper (1972), differentials in marital instability can be significantly misrepresented to the extent that subgroups differ in probability of divorce after separation, or in the average length of the interval between separation and divorce.¹

Since our dependent variable is a measure of prevalence, cross-sectional differences at the time of the interview are obviously affected by differences in the length of time that has elapsed since the first marriage. We have used dummy variable multiple regression analysis to adjust additively for the effects of differing exposure, and to obtain net effects for each variable considered.

This is the first of a number of analyses we are undertaking with these data. It is restricted to the white population (5,442

* This study is based upon data collected under a contract with the National Institutes of Health (Norman B. Ryder and Charles F. Westoff, Princeton University, principal investigators), with a subcontract to the University of Wisconsin. The analysis was also supported in part by National Institutes of Health Contract NIH-2197, James A. Sweet, principal investigator, and a "Center for Population Research" grant to the Center for Demography and Ecology from the Center for Population Research of the National Institute of Child Health and Human Development.

¹ For example, in these data among couples in which neither spouse has attended high school, the proportion divorced is .21 among whites and .19 among blacks; the corresponding proportions divorced or separated are .25 among whites and .39 among blacks (Ns=922 and 455, respectively).

women) and is divided into three parts. In the first part we consider differences in marital instability by various characteristics of the wife: her age at marriage, education at marriage, religion while growing up, whether she was pregnant at first marriage, whether she grew up on a farm, whether she lived with both parents at age fourteen, and whether her first husband had been married before. After examining these variables in cross-section, net of exposure time and net of each other, we consider briefly the addition of husband's age, education and religion to the model, and then examine the question of the effects of heterogamy on marital stability. In this latter part, we examine differentials in stability for marriages that are homogamous or heterogamous with respect to age at marriage, education and religion.

This first report is an overview of these data. In subsequent analyses we will extend the present analysis to the black population, and, for both racial groups, focus more attention on the process through which differentials occur. In addition we will examine a number of important variables not included here: husband's occupation, and wife's employment and education after marriage, as well as such fertility variables as the length of birth intervals and the planning status of pregnancies. We also plan to analyze the separation-divorce process, including differentials in the timing of separation.

Characteristics of the Wife at Marriage

Table 1 presents the results of a regression analysis of marital instability in relation to a number of characteristics of the wife. Fifteen percent of our sample were separated from or had divorced their first husbands. The first column of Table 1 shows the unadjusted or crude rate of separation and divorce expressed as a deviation from the total sample mean. The second column presents the net effect for each variable after controlling in an additive regression model for the length of time between the first marriage and the interview. (Duration is entered as a set of dummy variables categorized as shown in the bottom panel of the

table.) The third column shows each variable's net effect after additively controlling for duration and all other variables in the table.² Thus we are trying to sort out the effects on marital disruption of such correlated variables as age at marriage, education, premarital pregnancy, and religion.³

Age at First Marriage. Differentials in marital stability by age at marriage have often been reported (see, for example, Glick, 1957; U.S. Bureau of the Census, 1971). In the first panel of Table 1 it is clear that women who marry before age 20 have substantially higher rates of marital disruption than women who marry at older ages. This differential is not simply the result of the correlation of age at marriage with the other variables. With these other variables controlled, there is a difference of about ten points between the rates for women marrying at ages 14-17 and 18-19, a five point difference between ages 18-19 and 20-21, and a three point difference between ages 20-21 and 22-24. Women marrying for the first time at ages thirty and above have very low rates of marital disruption.

The fact that there is little difference between the unadjusted rates and those adjusted for duration and the other variables indicates that the lower stability of early marriages is not due simply to their association with low education or premarital pregnancy. These results suggest that role factors directly associated with age may be a fruitful area for further sociological research. Two issues in particular seem relevant: 1) the impact on young marriages of the struggle for independence from the family of orientation; 2) The effect of subsequent changes in young adult role perceptions on the stability of young marriages. To the extent that adult role patterns are tentative in the late teens and tend to stabilize with increasing age, post-marriage divergence in the spouses' role expectations may be more likely for young marriages.

² We tested a number of two-way interactions that seemed to be of substantive interest and found them to be unimportant. These include interactions between: age at marriage and education; pregnancy status and age at marriage; pregnancy status and education; and pregnancy status and religion.

³ For reviews of the literature in this area see Goode (1966).

Table 1. Differentials in Marital Instability^a by Selected Characteristics of the Wife, for White Ever-married Women under the Age of 45 in 1970: Unadjusted and Adjusted Deviations from the Grand Mean.

Characteristic	Deviations from Grand Mean			Number of Cases
	Unadjusted	Net of Duration ^b	Net of Other Variables in Model ^c	
<u>Wife's age at marriage</u>				
14-17 years	.132	.126	.109	1111
18-19	.000	-.002	.006	1791
20-21	-.057	-.050	-.042	1278
22-24	-.083	-.078	-.076	814
25-29	-.060	-.054	-.069	320
30+	-.096	-.080	-.129	52
<u>Wife's education at marriage</u>				
0-8 years	.120	.096	.041	397
9-11 years	.084	.078	.011	1174
12	-.025	-.024	-.009	2538
College, 1-3 years	-.047	-.037	-.002	876
College, 4+	-.090	-.078	-.010	454
<u>Status of first pregnancy and birth</u>				
Premarital birth	.068	.093	.088	171
Premarital conception	.022	.034	.002	602
Post-marital conception	.000	-.018	-.018	3851
No first birth	-.042	.043	.062	790
<u>Wife's religion while growing up</u>				
Protestant:				
Fundamentalist	.057	.059	.030	379
Baptist	.051	.046	.031	1170
Lutheran	-.038	-.036	-.028	455
Episcopalian	.040	.055	.077	144
Other Protestant	-.013	-.015	-.011	1556
Roman Catholic	-.031	-.026	-.017	1363
Jewish	-.117	-.115	-.066	80
Mormon	-.014	-.014	-.013	135
Other	-.011	-.012	.002	84
None	.056	.054	.018	76
<u>Wife's family status at 14</u>				
Lived with both parents	-.012	-.012	-.006	4403
One or both parents had died	-.001	-.011	-.017	458
Other	.092	.096	.056	581
<u>Wife's residence while growing up^d</u>				
Deep South:				
Farm	.033	.017	-.037	155
Non-farm	-.016	-.020	-.067	217
Other South:				
Farm	.021	.002	-.036	349
Non-farm	.019	.022	-.002	915
Other U.S.:				
Farm	-.010	-.015	-.016	709
Non-farm	-.005	-.001	.016	2883
Not U.S.:				
Farm	.179	.160	.206	21
Non-farm	-.074	-.078	-.036	162
<u>First husband's marital history</u>				
Not previously married	-.013	-.013	-.013	5029
Previously married	.156	.159	.154	413

Table 1—Continued

Time since first marriage				
Less than 2 years	-.130	--	-.172	443
2-3 years	-.080	--	-.088	538
4-5	-.017	--	-.010	510
6-7	-.036	--	-.025	480
8-9	.008	--	.007	433
10-11	.002	--	.022	457
12-14	.053	--	.062	641
15-19	.038	--	.051	918
20+	.051	--	.040	992
Grand Mean = .154				N = 5442

^aProportion with first marriage ending in either separation or divorce.

^bTime since first marriage at interview.

^cAll variables in the table were included in this model. Each of the variables makes a statistically significant addition to the explained variance at the .05 level. All except wife's education are significant at the .01 level.

^dBy taking the weighted average of the effects of the rural and urban women within a region, we can see the effect of region, ignoring farm origin.

Characteristic	Deviations from Grand Mean		
	Unadjusted	Net of Duration	Net of Other Variables in Model ^c
Deep South	.004	-.004	-.055
Other South	.019	.016	-.011
Other U.S.	-.006	-.004	.010
Not U.S.	-.045	-.051	-.007
(Similarly, for type of origin residence, ignoring region):			
Farm	.004	-.006	-.024
Non-farm	-.000	.003	.007
Not U.S.	-.045	-.051	-.007

Education. Consistent with earlier studies (Udry, 1966; U.S. Bureau of the Census, 1971), we find a pronounced negative relationship between education and marital instability when duration is controlled. However, with other variables such as age at first marriage controlled, the relationship is greatly attenuated, leaving only a five point difference between the grade school and college graduate categories. Furthermore, the majority of this remaining difference (three points) results from the higher rates of women with 0-8 compared to those with 9-11 years of education. Large education differences in the crude and duration adjusted rates between 9-11 and 12, and 13-15 and 16 years (the so-called "Glick effect") are virtually eliminated in the complete model. Clearly most of the effect of

education on marital disruption is mediated by education differences in age at marriage; little effect remains to be directly ascribed to education differences in "resources" brought to marriage as the literature sometimes suggests (for example, Goode, 1966: 110).

Status of First Pregnancy. Premarital pregnancy, when it results in an unintended marriage, is believed to raise the risk of marital disruption (Christensen and Meisner, 1953). We have classified all women in our sample into one of four categories with respect to their first birth:

1. Those who reported a first birth before their first marriage, i.e., whose first birth was illegitimate,
2. Those who reported a first birth between marriage and the end of the seventh

- month after marriage, i.e., who were premaritally pregnant,
3. Those who reported a first birth after the beginning of the eighth month of marriage,
 4. Those who have not yet had a first birth by the time of the interview. This group does not include all women childless in their first marriage. Those whose first child was born in a second or subsequent marriage would be included in group 3.

Controlling only for duration, we find that the level of marital disruption for women who reported an illegitimate first birth is eleven points higher than for those with a post-marital conception. Among women who were premaritally pregnant, marital disruption is five percentage points more likely than among those who conceived after marriage. However, premarital conception is associated with young age at marriage and the early termination of schooling. With these factors included, premaritally pregnant women have disruption rates only two points higher than those of women whose first conceptions occurred after marriage; only women whose first births were illegitimate continue to have substantially higher levels of disruption.

Women who have never had a child by the time of interview have a disruption rate (with all other factors controlled) that is eight points above that of women whose first conception occurred after marriage. We place no particular interpretation on this finding since the causal relationship between fertility (particularly childlessness) and marital disruption is so complex (see Cohen and Sweet, 1971). Whether the lower fertility is due to the marital disruption or the higher rate of disruption to childlessness is unknown.

Wife's Religion While Growing Up. The effects of religious differences on marital instability may derive from differing theological and subcultural views on marital breakdown and the strength of family ties. While religious differentials have been documented for specialized samples (Landis, 1949; Burchinal and Chancellor, 1963), until now there has been no evidence for the U.S. as a whole. In our data, we find wide variations in crude rates of marital disruption by religion, ranging from twelve points below the mean for Jewish women to

six points above the mean for women who reported that they had no religious preference during childhood. With other factors controlled the religious differential contracts considerably. It is of particular interest that Roman Catholics and Protestants as a whole differ only three points and that the high crude rate of persons reporting no religion is reduced to only two points above the mean. Lower levels of marital disruption among Jews persist in the complete model at seven points below the mean.

Within the Protestant group there are wide variations. Episcopalians have the highest rate (+8 points) followed by Baptists and fundamentalists (+3 points). Lutherans and "other Protestants" have rates similar to those of Roman Catholics.

Family Status While Growing Up. Our data support the possibility that there may be a social inheritance of marital instability. Women one or both of whose parents were deceased have disruption rates which differ little from those of women from intact families. On the other hand, women whose parents had experienced divorce or separation have a disruption rate that is ten points higher than that of women who lived with both parents. Only about a third of this difference is eliminated with other variables controlled. We have not, however, controlled on other aspects of origin status such as parental education and occupation, which may also be correlated with marital instability.⁴

Wife's Residence While Growing Up. Irrespective of region, there is no difference in the unadjusted rate of marital disruption between persons with farm and non-farm origin. However, with all other factors adjusted for, the farm origin women have a rate three points below that for women with a non-farm origin. There are no regional differences in the crude rates of disruption, but after adjusting for all other variables, we find that women from the deep South are six points less likely than non-southern women to have experienced marital disruption.

⁴ The apparent conflict between our findings on this point and those of Duncan and Duncan (1969) is probably in large part a product of the fact that our data measure directly the stability of first marriages, whereas their findings were based on current marital status.

tion. This difference is particularly pronounced for the non-farm group.

First Husband's Marital History. While our focus is on the first marriages of the women in our sample, the first husband of approximately eight percent of these women had been previously married. Such marriages were considerably less stable than those which were first marriages for both spouses. The crude difference of about sixteen points does not contract when the other variables are controlled.

*Husband's Age at Marriage,
Education and Religion*

Table 2 presents the results of adding husband's characteristics one at a time to our complete additive model. Including the husband's age at marriage does not alter the effects of wife's age, though unadjusted differences in marital stability by husband's age at marriage are in part attributable to other factors including wife's age at marriage. Husband's education also has a nega-

tive relationship with marital instability with all other variables controlled (husbands completing college were six points lower in marital instability than husbands not completing high school). This persistent influence of husband's education in our complete model may well reflect the effects of economic resources as well as those of education per se. We will be able to check this point in our future analyses by including husband's occupation at marriage in our model.

When we add husband's religion to the model, two findings in particular deviate from the effects observed for wife's religion. The most surprising of these is the markedly high level of instability observed for Jewish males with other variables controlled. Detailed analysis suggests that this estimate results from the high instability of Jewish males married to non-Jewish wives (one quarter of the Jewish males in the sample) and the fact that these males were selected from subgroups otherwise likely to have lower rates of instability.

Table 2. Differentials in Marital Instability^a by Age at Marriage, Education and Religion of Husbands and Wives, for White Ever-married Women under the Age of 45 in 1970: Unadjusted and Adjusted Deviations from the Grand Mean.

Characteristic	Husbands		Wives
	Unadjusted	Net of All Variables in Model ^b	Net of All Variables in Model ^b
<u>Age at marriage</u>			
14-19	.070	.029	.099
20-21	.002	.009	.003
22-24	-.030	-.008	-.038
25+	-.021	-.017	-.066
<u>Education at marriage</u>			
0-11 years	.062	.018	.006
12 years	.001	.003	-.011
College, 1-3 years	-.063	-.031	.014
College, 4+ years	-.096	-.040	.013
<u>Religion while growing up</u>			
Protestant:			
Fundamentalist	.008	-.045	.047
Baptist	.021	-.030	.044
Lutheran	-.052	-.019	-.013
Episcopalian	.020	.025	.069
Other Protestant	.005	.004	-.013
Roman Catholic	-.029	-.004	-.017
Jewish	-.055	.105	-.156
Other	.049	.062	-.033

^aProportion with first marriage ending in either separation or divorce.

^bAll variables in Table 1 included in addition to this characteristic of husband.

The second major difference by husband's religion is the negative effect observed in the total model for fundamentalist and Baptist husbands. Lower levels of marital instability for husbands and higher levels for wives in these conservative religious groups may reflect differing experiences of stability in outmarriages in the context of traditional patterns of male dominance.

Homogamy and Heterogamy

We turn now to a consideration of the effects on marital dissolution of similarity or dissimilarity between spouses at marriage. Theories of mate selection emphasize the role of homogamy as a structural mechanism for the identification of potential mates with similar values and expectations, and the marital adjustment literature suggests that the probability of satisfactory adjustment is lower among couples with dissimilar backgrounds. When marriages are not homogamous with respect to significant social characteristics, it is expected that added tensions will be reflected in both lower rates of marital adjustment and higher rates of marital dissolution. Measures of homogamy are widely used in tests for predicting marital adjustment (Bowerman, 1964), and Goode (1966) lists "general dissimilarity in background" among factors associated with greater "proneness to divorce." The higher probability of success for homogamous marriages is usually attributed to the greater likelihood of value consensus between the spouses on basic life goals and priorities, and to similarity of expectations for marital roles. Differences between spouses may be emphasized and reinforced to the extent that relationships are maintained with friends and relatives from the divergent groups.

However, heterogamous marriages may be selective of certain types of persons. On the one hand, persons who intermarry may be less committed to the values of their respective groups. Such selectivity would moderate the effect of group differences. On the other hand, intermarriages may be selective of persons less able to compete within the prescribed market. To the extent this occurs, marital success rates might be lowered for heterogamous marriages, not so much through the differing values of the

groups involved as through the selection of personal characteristics not conducive to marital adjustment.

Empirical tests of these expectations have been limited because of the scarcity of appropriate data.⁵ Blood and Wolfe (1960) reported lower levels of marital satisfaction among Detroit couples who were dissimilar on age, religion, or education (among other characteristics). Analyses of differing failure rates for homogamous and heterogamous marriages have focused primarily on religion; and most have been limited to the states which record religion on marriage certificates: Iowa and Indiana (e.g., Christensen and Barber, 1967). Though limited in scope, the data we present are, to our knowledge, the first national data on differences in marital stability for marriages homogamous or heterogamous on age, education and religion.

To estimate the effects of combinations of husband's and wife's characteristics at marriage we have included a composite husband-wife variable in our regression model and then compared the resulting deviations with those expected if husband's and wife's characteristics were additive in their effects. The composite variables are reported in Panels 1 and 2 of Tables 3-6, and the additive effects are based on Table 2. The differences between these two sets of deviations are reported in the third panel of Tables 3-6. Cell values greater than zero indicate that the level of marital instability is higher than we would predict if the characteristics of spouses were additive.

Ages of Spouses at Marriage. Age dissimilarity might be thought to affect marital stability in several ways. In the first place, value consensus may decrease as age differences increase. Age differences do not represent specific sets of values and expectations as may religious or educational differences. Nevertheless, in an era of rapid social change, persons born only five years apart may formulate values and expectations in quite different contexts. Secondly,

⁵For example, in his review of the literature on these topics Levinger (1965) cites only three articles on education differences written in 1929, 1937 and 1938, and two sources on age differences written in 1939 and 1951.

age differences may produce an imbalance in the family power structure. Such an imbalance may tend to create tension in general, but it may do so especially when the wife is older than her husband. And finally, as we suggested above, some couples marrying at widely divergent ages may have personal characteristics that lower their probability of marital success.

Marital instability varies widely with varying combinations of age at marriage, ranging from fifteen points above the mean

when the wife is under seventeen and the husband under nineteen to nine percentage points below the mean when both are over age twenty-two (Table 3). While the variance contributed by interaction is not statistically significant at the .05 level, particular cell effects in Panel 3 are of considerable substantive interest. When we examine the deviation of the composite variable from an additive model, we find higher than expected instability when age differences between spouses are large and when wives are

Table 3. Differentials in Marital Instability^a by Ages of Spouses at Marriage, for White Ever-married Women under the Age of 45 in 1970: Unadjusted and Adjusted Deviations from the Grand Mean, and Differences between Interactive and Additive Effects.

Age of Wife at First Marriage	Age of First Husband at Marriage			
	14-19	20-21	22-24	25+
Unadjusted Deviations				
14-17	.147	.116	.064	.224
18-19	-.014	-.016	-.002	.070
20-21	-.031	-.052	-.065	-.057
22-24	**	-.032	-.090	-.093
25+	**	**	.033	-.085
Net of Other Variables in Model ^b				
14-17	.141	.100	.032	.135
18-19	.009	.002	-.002	.031
20-21	.003	-.024	-.043	-.075
22-24	**	-.005	-.068	-.105
25+	**	**	.024	-.100
Differences between Interactive and Additive Effects ^c				
14-17	.013	-.008	-.059	.054
18-19	-.023	-.010	.002	.045
20-21	.012	.005	.003	-.020
22-24	**	.051	.006	-.022
25+	**	**	.097	-.017
Number of Cases				
14-17	515	292	193	111
18-19	421	628	494	246
20-21	73	400	504	300
22-24	15	74	346	378
25+	10	10	48	304

**N less than 20.

^aProportion with first marriage ending in either separation or divorce.

^bAll variables in table were included.

^cCell estimates resulting from composite husband-wife variable (Panel 2 of this table) minus cell estimate resulting from addition of husband and wife effects, both net of all other variables in model.

older than their husbands. When wives aged 14-17 married husbands 25 and older, the level of instability is five points higher than we would expect on the basis of these ages, all other variables controlled. Furthermore, instability is five points higher than expected when wives aged 22-24 are married to husbands 20-21; and when wives over 25 are married to husbands 22-24, instability is ten points higher than expected on additive assumptions. It is noteworthy that the stability of the marriages of the youngest brides increases with the age of their husbands up to the last category, where the age difference is large enough to be socially significant. These data, then, are consistent with theoretical expectations about the effects of age differences at marriage.

Education of Spouses at Marriage. The interpretation of the effect of educational differences is complicated by the many social meanings of education. Increasing levels of education represent both increasing prestige and access to resources, and varying socialization experiences. On the one hand, the tie of education to the stratification system means that prestige and financial resources increase with the education of either spouse, though particularly so with the male. On the other hand, we might expect higher value consensus among spouses with the same education. To the extent that formal education influences world views, goals and priorities, we might expect educational differences between spouses to influence probabilities of marital success.

While there are considerable differences in marital instability for various education combinations, these are greatly reduced when other variables are controlled (Table 4, Panels 1 and 2).

Again, the overall contribution of the interaction to the explained variance is not statistically significant though several cell deviations are of interest. When we consider the difference between interactive and additive effects (Panel 3, Table 4), we find no regular pattern for off-diagonal cells adjacent to the diagonal—of six such cells, three are higher than expected and three are lower than expected on additive assumptions. However, for greater differences in education, higher than expected instability ap-

pears in three of four cells. Among wives not completing high school married to husbands who had attended college, estimates of instability are three points higher than expected; among high school graduate women married to college graduate husbands, instability is four points higher than expected; and among college women who married high school dropouts, the level of dissolution is seventeen percentage points above the mean net of all the variables, and fourteen points higher than expected on additive assumptions, all else controlled. These latter marriages may represent the most extreme cases of both conflicting expectations about the female role in marriage and divergence from general social expectations about the male economic role. On the other hand, the fact that high school graduate husbands married to college graduate wives do not have a higher than expected level of dissolution suggests that the former large deviation may reflect the lower earning power of high school drop-outs. We can also check this interpretation in future analyses including husband's occupation.

The most important conclusion from these data is that they do not generally support the association of educational heterogamy with high marital instability.

Religion of the Spouses. Much literature on intermarriage has focused on religion. Religious differences are thought to be closely related to differences in basic world views and values, often quite apart from theological, liturgical or organizational differences. Value consensus is considered less likely with intermarriage, especially when religious backgrounds are highly dissimilar. For many, differences in socialization may continue to be reinforced by social interaction with parents and friends from divergent communities. One important dimension of such differences may be the relative value placed on family relationships vis-a-vis other values. The religious identification of children has often been discussed as a particularly likely source of tension in interfaith marriage, especially when one or both of the religious groups involved has strong normative expectations. In brief, religious intermarriage is expected to add additional tensions to marriage because the basic non-

Table 4. Differentials in Marital Instability^a by Education of Spouses at Marriage, for White Ever-married Women under the Age of 45 in 1970: Unadjusted and Adjusted Deviations from the Grand Mean, and Differences between Interactive and Additive Effects.

Education of Wife at First Marriage	Education of First Husband			
	0-11 Years	12 Years	College 1-3	College 4+
Unadjusted Deviations				
0-11 years	.094	.073	.046	**
12 years	.009	-.028	-.065	-.069
College, 1-3 years	.154	.014	-.080	-.103
College, 4+ years	**	-.052	-.062	-.105
Net of Other Variables in Model ^b				
0-11 years	.022	.012	.006	**
12 years	.002	-.012	-.034	-.015
College, 1-3 years	.169	-.040	-.034	-.049
College, 4+ years	**	.002	.002	-.024
Differences between Interactive and Additive Effects ^c				
0-11 years	-.003	.002	.031	**
12 years	-.006	-.005	.009	.036
College, 1-3 years	.137	-.057	-.016	-.024
College, 4+ years	**	-.015	.020	.003
Number of Cases				
0-11 years	922	533	65	16
12 years	509	1339	413	175
College, 1-3 years	52	232	377	215
College, 4+ years	14	49	109	289

**N less than 20.

^aProportion with first marriage ending in either separation or divorce.

^bAll variables in table were included.

^cCell estimates resulting from composite husband-wife variable (Panel 2 of this table) minus cell estimate resulting from addition of husband and wife effects, both net of all other variables in model.

rational values of spouses are apt to differ, and significant others will reinforce these differences.

Higher rates of marital instability have been reported for interfaith marriages by Landis (1949) based on data collected on the parents of his students, and by Burchinal and Chancellor (1963), Christensen and Barber (1967) and others based on Indiana and Iowa vital statistics.

We will first consider differences in marital instability for intra- and interfaith marriages among Protestants, Catholics and

Jews, and then turn to interdenominational marriages among Protestants.

Our national data support the findings of the previous studies based on specialized samples. In intra-faith marriages, Jewish couples have the lowest levels of instability, Protestant couples the highest, with Catholics intermediate (Table 5). Net of duration differences (table not shown), the difference between Jewish and Protestant intra-faith marriages is ten percentage points. Mixed Protestant-Catholic marriages are less stable than religiously homogenous marriages.

Table 5. Differentials in Marital Instability^a by Religion of Spouses at Marriage, for White Ever-married Women under the Age of 45 in 1970: Unadjusted and Adjusted Deviations from the Grand Mean, and Differences between Interactive and Additive Effects.

Religion of Wife While Growing Up	Religion of First Husband			
	Protestant	Catholic	Jewish	Other
Unadjusted Deviations				
Protestant	-.003	.045	**	.127
Catholic	.052	-.061	**	-.016
Jewish	**	**	-.110	**
Other	.072	.028	**	-.030
Net of Other Variables in Model ^b				
Protestant	-.012	.049	**	.122
Catholic	.054	-.038	**	-.002
Jewish	**	**	-.054	**
Other	.050	.027	**	-.021
Differences between Interactive and Additive Effects ^c				
Protestant	-.012	.041	**	.048
Catholic	.083	-.016	**	-.048
Jewish	**	**	-.003	**
Other	.095	.064	**	-.051
Number of Cases				
Protestant	3003	582	13	306
Catholic	316	915	9	123
Jewish	3	1	68	8
Other	84	33	1	177

**N. less than 20.

^aProportion with first marriage ending in either separation or divorce.

^bAll variables in table were included.

^cCell estimates resulting from composite husband-wife variable (Panel 2 of this table) minus cell estimate resulting from addition of husband and wife effects, both net of all other variables in model.

They are about five percentage points more unstable than Protestant-Protestant marriages and eleven points more unstable than Catholic-Catholic marriages. Control for other variables in the model accounts for some of the low level of instability observed for Jews but not for the higher levels of instability of interfaith marriages.

The religious interaction is statistically significant and all but one of the off-diagonal cells is higher than expected on additive assumptions. (The exception is for Catholic wives married to "other" husbands.)

Interdenominational marriages among Protestants are presented in Table 6. Al-

though cell sizes unfortunately limit the comparisons we can make, there is no general support for higher instability in interdenominational marriages. In the deviations from values expected given additive assumptions (Panel 3), of the twelve reported interdenominational marriage categories (off-diagonal cells), eight are higher and four are lower than expected. However, some interdenominational marriages are considerably less stable than marriages between spouses of the same denomination, and some of these differences are as large or larger than those found for Protestant-Catholic marriages. For example, outmarriages involving funda-

Table 6. Differentials in Marital Instability^a by Denomination of Spouses at Marriage, for Protestant White Ever-married Women under the Age of 45 in 1970: Unadjusted and Adjusted Deviations from the Grand Mean, and Differences between Interactive and Additive Effects.

Denomination of Wife While Growing Up	Denomination of First Husband				
	Fundamentalist	Baptist	Lutheran	Episcopalian	Other Protestant
Unadjusted Deviations					
Fundamentalist	-.060	.059	**	**	.136
Baptist	.074	.013	.107	**	.078
Lutheran	**	-.063	-.068	**	-.042
Episcopalian	**	**	**	-.067	.132
Other Protestant	.030	.016	-.075	-.000	-.039
Net of Other Variables in Model ^b					
Fundamentalist	-.083	.011	**	**	.078
Baptist	.026	-.023	.121	**	.054
Lutheran	**	-.074	-.045	**	-.033
Episcopalian	**	**	**	-.010	.183
Other Protestant	-.008	.002	-.049	.035	-.034
Differences between Interactive and Additive Effects ^c					
Fundamentalist	-.086	-.006	**	**	.026
Baptist	.027	-.036	.095	**	.006
Lutheran	**	-.025	-.008	**	-.019
Episcopalian	**	**	**	-.104	.110
Other Protestant	.049	.045	-.018	.023	-.026
Number of Cases					
Fundamentalist	159	75	10	3	69
Baptist	79	688	23	13	207
Lutheran	16	22	233	8	71
Episcopalian	3	14	5	46	28
Other Protestant	65	141	76	39	910

**N less than 20.

^aProportion with first marriage ending in either separation or divorce.

^bAll variables in table were included.

^cCell estimates resulting from composite husband-wife variable (Panel 2 of this table) minus cell estimate resulting from addition of husband and wife effects, both net of all other variables in model.

mentalists are over ten percentage points more unstable than marriages between fundamentalists. This difference is the result of lower than expected levels of instability in marriages in which both partners are fundamentalists. Such differences for fundamentalists seem consistent with the strict theological and social ethics stance of these groups. On the other hand, comparable lower stability for outmarriages is observed for Baptists and Episcopalians (when outmarriages are averaged for the latter).

In summary, we have documented from national data important differences in marital instability by characteristics at marriage. Age at marriage is predominant in affecting stability, though other variables have important net effects. Theories of the role of homogamy in marital stability are supported, particularly for religion. Much work remains to be done in empirically "explaining" the processes through which these differences arise.

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STATUS PERCEPTIONS *

C. NORMAN ALEXANDER, JR.

Stanford University

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This study proposes that the perception of status in any group is a function of the perceiver's status position. The lower the perceiver's actual, imagined, or aspired-to status, the smaller the dispersion of his status judgments. Those of lower status diminish the distance between persons or positions in the system by raising the status of those in the lower regions. Data supporting this argument come from a national sample of occupational prestige perceptions and from a group of high school senior males. Data on popularity judgments by members of a school class supported the general nature of the phenomenon. The importance of these perspective differences for theoretical concepts of status-related behavior is emphasized, and the possibility of affecting aspirations by changing status cognitions is discussed.

EVER since Marx, ideological theorists have insisted that one's position in the social structure profoundly influences his perspective on the system. Sociology is full of studies showing that social class makes important differences in attitudes, values, and life-styles. But in one area consensual perceptions are emphasized across social strata. This research deals with status perception itself—occupational prestige judgments. Davies (1952) searched in vain among prestige perception studies for differences in status perceptions owing to the perceiver's social position; but he had to conclude that more than a quarter-century of research revealed a "remarkable consensus."

His impression has been reinforced by research reports in the two decades following his review. Studies stress correlations of +.98 and better between status judgments of different strata within and between most industrialized societies. The impression that social position does not influence status per-

ceptions is not only inconsistent with most social theories, it conflicts with a vast amount of evidence in psychophysics and social psychology (Sherif and Hovland, 1961; Helson, 1964) which shows that a person's position ("anchor standard") on any judgment scale influences his perception of stimuli in that area. Finally, commentary has so over-emphasized consensual ranking across strata that it has obscured the important fact that the *absolute* status-rating scale-scores of persons or positions do differ.

The problem of differing status perceptions is important and needs sociological scrutiny. Aside from its theoretical relevance, many research questions presume knowledge of and require measurement of status perceptions. For instance, status incongruity assumes perceived distinctions in status-orders affecting a person's self-regard and others' treatment of him. Problems of distributive justice necessarily involve placing oneself and others in status positions. And, of course, any concern with levels of aspiration and achievement orientation implies such status distinctions.

Since status is a variable governing the rights and rules by which interactors mutually orient themselves, differing assessments can importantly affect the flow of social interaction. This paper argues that systematic perceptual differences in status structures are a function of the perceiver's position; it suggests that all status systems contain these perceptual differences; and it illustrates that these differing perceptions

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are associated with phenomena of interest to social scientists. After a brief review of the major literature on status perceptions, I will present new research evidence and discuss its implications.

COMMUNITY AND OCCUPATIONAL PRESTIGE STUDIES

The pioneering and now-classic series of studies by W. Lloyd Warner (1949) remains the most comprehensive body of research on community prestige. Unfortunately, Warner was so concerned to find the "true" number of classes and correctly place individuals in them that he tended to view as "annoying," any signs of differing perceptions—a bias for which he was later criticized (Kornhauser, 1953). Yet, subsequent work was never systematic or broad enough to answer the questions raised.

However, the Warner series did contain a cogent statement of class-related differences in status perceptions. Davis, Gardner, and Gardner (1941) discussed such variations as well as variations in judgment associated with status distinctions. They noted two tendencies: (1) Prestige distinctions decrease in fineness with increasing "social distance" between judge and judged; and (2) people tend to enhance their relative prestige in a system by using many or few judgment categories. The number of class groupings perceived increases with the judge's status.

In general, too, individuals visualize class groups above them less clearly than those below them; they tend to minimize the social differentiation between themselves and those above. . . . In view of this situation it is not surprising that individuals in the two upper strata make the finest gradations in the stratification of the whole society and that class distinctions are made with decreasing precision as social position becomes lower (Davis, Gardner, and Gardner, 1941:72).

Lewis (1964) noted an inconsistency in the "social distance" and "ego-enhancement" generalizations, and suggested an appropriate reformulation. People should be able to make the finest distinctions about areas of the social structure most familiar and relevant to them, but doing so above one's position tends to lower self-evaluation. Thus, it seems most likely that prestige dis-

tinctions will be made as social distance decreases and as the ego-enhancement implied by the distinction increases. The proposition has never been adequately tested, but it seems consistent with observations from community prestige data.

One would expect to find such propositions systematically explored with the data from occupational prestige studies, especially since these studies have employed more sophisticated sampling and methodological techniques. However, investigators in this area have been more impressed by the rank-order consensus among respondents than interested in exploring differing prestige perceptions. In an extensive analysis of the 1950 NORC data, Reiss commented: "The most obvious difference in the ratings of occupations by economic level, both in terms of rank and prestige-increment criteria, is the tendency for poor people to rate almost all occupations . . . higher than do the prosperous, with the middle class generally intermediate in position" (Reiss, 1961:175).

Reiss did little more than note this, and others have also dismissed or disregarded such "scale response" phenomena as relatively unimportant or reflective of theoretically irrelevant "response tendencies." However, we should take these responses seriously, because (1) they reproduce patterns found in community prestige studies where respondents were free to select their own categories of judgment, and (2) they seem fairly constant whenever people are asked to judge any social-stimulus objects along a consensually evaluative scale (Sherif and Hovland, 1961; Alexander and Geisinger, 1971).

In this paper I will argue that prestige perceptions are patterned by the position of the perceiver and that the patterns are the same in any social structure. A status structure is defined as a consensual ordering of persons or positions along a similarly-valued dimension by knowledgeable members of a group. Thus, for there to be a status structure, rank-order consensus must exist among members with respect to an evaluative dimension. Members of the group are considered competent judges if they know (1) the meaning of the dimension and (2) are familiar enough with the objects of judgment to

know how to place them. The definition is general enough to include any group which ranks individuals, positions, or status-attributes consensually.

First, I will show that the dispersion of status judgments increases as the status of the judge increases in different types of structures with different types of populations. I will also show that this proposition is true, not only for the judge's actual status, but also for his anticipated, imagined, or aspired-to status. I will briefly discuss the implications of this lawful relation between the perceiver's position and his prestige perceptions patterns. Finally, I will examine occupational prestige judgments of a national sample, popularity judgments among students in a high school, and the prestige perceptions and educational aspirations of high school senior males.

I. OCCUPATIONAL PRESTIGE: THE NORC DATA

Reiss presents the original NORC data from 1950 (1961:198-217, 276-95) in such a way that one can reconstruct the data in appropriate form. However, a problem is raised by the "knowledgeable respondent" criterion of judgment, because the study contains substantial numbers of "don't know" responses. The evidence indicates that knowledge about occupations decreases as (1) the status of the position being rated increases and (2) the status of the rater decreases. Furthermore, the NORC study contained a disproportionate number of high status occupations that are fairly technical, if not esoteric. Data from the study show two things: First, the absence of accurate knowledge about an occupation does not dissuade people from rating it (Reiss, 1961:17). Although only 49.6 percent of respondents rated the occupation of nuclear physicist, a later question revealed that 61.3 percent of those rating it did not know what it was (they had "no idea," "vague" or "wrong" responses about what a nuclear physicist did). Second, knowledge affects ratings. Those who were correct in knowing what a nuclear physicist did ($N=78$) rated the position as an "excellent" one in 71.8 percent of the cases; whereas, only 31.3 percent of those having no idea what he did ($N=400$) gave excellent ratings.

Although we cannot determine from Reiss' summary data which respondents were relatively ignorant, we can eliminate occupations that were relatively unknown. Arbitrarily, twenty-seven of the ninety occupations were removed, because less than 95 percent of the respondents knew enough about them to rate them. Table 1 presents the percent of respondents at each status level who rated the remaining sixty-three occupations as "excellent" or "good," with the occupations divided into equal thirds according to their overall prestige scores.¹ The data show great differences by rater-status in the prestige scores assigned the lower two-thirds of the hierarchy, but few differences in ratings of the top third. It is obvious from the table that the dispersion of judgments decreases as the status of the judge decreases: lower status respondents are less inclined to use the "poor" category and more often use positive ratings. It is possible, however, that the category labels did influence these results. It is also possible that these data are peculiar to occupational prestige ratings or large social groups. Thus, to explore the general nature of the phenomenon, we must consider a very different type of "prestige": popularity in high school.

II. HIGH SCHOOL POPULARITY RATINGS

I chose to investigate popularity status judgments in a high school class in order to minimize methodological problems. The

¹ The occupational classification of occupations is based on raters' status categorization of respondents as Prosperous, Middle Class, or Poor. These data are reported in full, including percentage bases for "don't know" responses, in Reiss (1961:198-217, 276-95).

Table 1. Percentage of Excellent-Good Ratings of "Known" Occupations by Status of Judge and Occupation Judged

Status of Judge	Status of Occupation Judged			
	High	Medium	Low	N
Above average	82.6	33.6	00.6	514
Average	84.6	39.2	08.6	1,531
Below average	85.4	50.6	17.4	856

choice eliminated entirely the difficulties of sampling—especially acute in selecting occupations to be judged—by permitting the inclusion of the entire universe of judges and judged. Knowledge (who knows whom) could be established directly. Furthermore, an informal group has no historically established barriers to communication and information nor any specialized task-functions, association patterns, or structurally determined visibility channels. Finally, a high school class is enough different from community or national occupational prestige structures that claims of a general nature based on it are more plausible.

A school in a middle-income suburb was selected on the basis of homogeneity in socioeconomic composition, moderate class size, and limited geographical dispersion of students. The 161 students whom the school classified as seniors were asked to judge the popularity of the same-sex others in their class. Popularity was judged on a nine-point, unlabeled continuum, ranging from LOW to HIGH. Listings were mimeographed in alphabetical order, with the pages randomly ordered for presentation. The poles of the judgment continuum were not alternated, since constant response bias was unimportant compared to a risk of increased response error.

To insure that respondents were knowledgeable and the judgment universe "known," judgments by and about students were removed if they did not judge or were not judged by 80 percent of their classmates.² A person was judged unless the re-

spondent "never heard of him" or knew "nothing about him at all." Fourteen students were eliminated, seven of whom had been in the school less than two years. The popularity ranking for each sex was determined from the responses of the remaining 147 seniors. They were divided into approximately equal thirds (25-24-25 for males, 24-25-24 for females). All data were first analyzed separately by sex (Alexander, 1965:161-87); but they are averaged here, since it simplifies presentation and does not change the conclusions.

Since consensual rank-ordering is required for a status structure to exist, we computed correlation coefficients between each individual's popularity judgments and the consensually defined mean values on popularity for each person rated. Though rank-order agreement is all that is required, it was more convenient and interesting to compute the Pearsonian coefficients. By equal-thirds of the popularity order for judges, the average coefficients were between +.70 and +.79, with a slight tendency for higher status respondents to be more "accurate." It might be more theoretically appropriate to discard cases who did not perceive the popularity ordering in accord with their peers; but we did not do so, since everyone had an accuracy correlation above +.30 and any cut-off point would have been arbitrary. Obtaining average Pearsonian correlations above .70 indicates adequate consensus, especially since these are undoubtedly lower than rank-order coefficients would be.

Table 2 presents the mean popularity judgments of those in each popularity third about those at each third, and the data show the expected pattern. As with occupational judgments, respondents do not differ much in their ratings of those at the upper end of the prestige-popularity continuum; but they progressively differ as the status of the judged object decreases. The dispersion of

² In examining the NORC data, a 95 percent level of reported knowledge about a job was arbitrarily selected as an intuitively reasonable criterion for eliminating positions about which respondents knew little or nothing. It happens that this removes some of the acknowledged bias of that sample of occupations, which tended to favor heavily the upper status professional and technical positions (e.g., nuclear physicist). It is also the case, as evident in the ratings of nuclear physicist by unknowledgeable respondents, that people who are ignorant, but give a rating anyway, tend to give moderate or middle-range responses. Since most lack of knowledge comes from those of low status, our hypothesis of judgment constriction by perceiver status is more strongly supported if we do not remove positions about which people are uncertain or ignorant.

In the case of a knowledge criterion for student popularity in the high school, we wanted to set our required level of knowledge as high as possible;

but this time we were dealing with an unstable system. Thus, we had to lower our criterion to 80 percent, again arbitrarily. This percent is probably lower than would be necessary in another school, but our high school was new (two years old) and was located in a rapidly growing suburb. Many students were recent transfers, who could not be expected to know or be known by their fellow students.

Table 2. Mean Popularity Judgments--by Popularity of Judge and Judged

Popularity of Judge	Popularity of Judged			(N)
	High	Medium	Low	
High	6.7	4.7	3.0	(49)
Medium	6.7	4.9	3.3	(49)
Low	6.8	5.3	3.8	(49)

popularity judgments decreases with the popularity of the judge. Again, this is due to the fact that lower status judges rate objects of judgment more highly as the positions of those objects decrease in status.

Since we asked our respondents to rate themselves on the popularity dimensions along with their ratings of classmates, we can examine the prediction that judgment dispersions are associated with anticipated, imagined, or aspired-to status positions. The standard deviation of popularity judgments was computed for each respondent; and, by sex at each of the three status levels, they were divided at the median. Mean self-placements were then computed. Table 3 presents the mean self-placement judgments of respondents by actual popularity ranking and high or low standard deviation of popularity judgments. The table shows that objective and self-perceived popularity standings are associated with judgment dispersions in the same way: dispersions increase with actual and imagined popularity ratings. This result has interesting implications which we will consider after presenting some data gathered

Table 3. Mean Self-placement Ratings, by Judge's Popularity and Standard Deviation of Popularity Judgments

Popularity of Judge	Standard Deviation of Popularity Judgments	
	High	Low
High	7.1(23)	6.2(24)
Medium	5.6(23)	5.2(23)
Low	5.1(24)	4.7(25)

on high school senior males' prestige perceptions and educational aspirations.

III. MALE SENIORS' OCCUPATIONAL PERCEPTIONS AND EDUCATIONAL ASPIRATIONS

In an upper status suburban area we surveyed high school senior males about their college plans and perceptions of occupational prestige. They rated the prestige of fifty occupations (using the NORC list as a core and trying to approximate the distribution of occupational statuses to that in the population) on an unlabeled, nine-point continuum. Based on their responses to a question about father's occupation, we classified them as either white collar or blue collar. We eliminated those who did not give adequate responses to the prestige-judgment question (failed to discriminate among occupations, didn't answer, or answered in obviously idiosyncratic ways) and those who, in a separate question about knowledge of fifteen jobs, indicated they knew little or nothing about most.

Table 4 reports the mean occupational prestige judgments by status of judge and judged and the percentage of respondents at each status level whose standard deviation of prestige judgments was above the sample mean. The mean judgments show the expected pattern, and the standard deviations of white collar students are significantly higher than those of blue collar respondents. Turning now to their aspirations following graduation from high school, we found that almost all had plans for further education; so we classified them as "high aspirers" if

Table 4. Mean Occupational Prestige Judgments--and Percentage of Judges Above Mean Standard Deviation--by Status of Judge and Occupations Judged

Status of Judge	Status of Judged Occupations		Percentage Above Mean Standard Deviation in Judgments*
	High	Low	
High	6.7	4.3	61.1%(422)
Low	6.5	4.7	50.6%(166)

*Differences by status level are significant beyond .05 level by chi-square test with 1 d.f.

they planned to attend a four-year college or university and "low aspirers" if they planned anything else.

Table 5 presents the percent with high aspirations—by status level and the standard deviation of their occupational prestige judgments. As with the popularity judgments shown in Table 3, actual status has the most impact on aspired-to status; but prestige-judgment dispersion and aspirations are also associated. For those with high status, prestige dispersion has a significant relationship with educational aspirations; and for those with low status, the association is almost statistically significant. The same trends can be seen in data relevant to these respondents' occupational aspirations; however, we asked for those responses in an open-ended question, and almost one-third of the sample either failed to answer or gave responses too vague to be coded.

CONCLUSIONS

Status perception differs greatly and systematically with the perceiver's status in the system—as it is, as he sees it, and as he hopes for it to be. The higher the judge's status, the greater his judgment dispersion, due largely to the fact that he places lower status persons or positions lower on the scale. This pattern has been revealed in occupational prestige judgments from a national sample in 1950 and an upper-middle-class suburban sample of high school seniors in the late 1960s, as well as in the popularity judgments of members of a high school class. The same pattern also appeared in earlier studies of community prestige. We are thus led to propose that this differentiated per-

ception of status is a general characteristic of any group. The independent influence of self-perceived or aspired-to status is an interesting and important finding rarely considered, and inadequately explored (Blau, 1957).

Differing status perceptions may prove important in such areas as the shaping of occupational and educational aspirations. Weinstein (1957-58) found that high status youngsters lumped everyone from skilled craftsmen to private household workers in the same low job category; whereas, low status children classified skilled craftsmen with professionals in their top category. If objects are similarly classified, they are usually responded to similarly. Thus, it may be that the skilled crafts seem equal to domestic service for upper status respondents; while lower status students might regard the skilled-craftsman and professional positions as equally difficult to attain. Furthermore, if lower status persons systematically diminish the perceived prestige differences among all occupations, prestige may not influence them much in choosing an occupation.

If a lawful relationship exists between prestige perceptions and aspirations, one could perhaps manipulate aspirations. Most research on the determinants of aspirations has focused on variables almost impossible to manipulate—family factors, innate ability, underlying motivation, and value structures. It is frustrating to attend to such variables that we cannot control. The perceptual-pattern variable emphasized here is cognitive and depends largely on the availability of information. Those who advise and counsel could manipulate the information bases for prestige perceptions and, by that process, perhaps affect student aspirations.

If we explore this line of reasoning, we may find that people respond to their perceptions of different status worlds rather than respond differently (for motivational reasons) to similar perceptions of consensually defined status structures. Apart from learning more about the cognitive impact of social structures on participants from different positions, we will undoubtedly refine theories which assume that status-objects have similarly defined values throughout the structure. Since this assumption of "consensual definition" pervades sociological

Table 5. Percentage with High Educational Aspirations--by Judge's Status and Standard Deviation of Prestige Judgments

Status of Judge	Standard Deviation of Prestige Judgments		Chi-square Test
	High	Low	
High	59.7% (258)	47.0% (164)	$P < .01$
Low	38.1 (84)	26.8 (82)	$P = .08$

thought, recognizing perspective differences by social position and assessing their effects should be critical to the study of status-related social phenomena.

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COMMENTS

FEMALE INTERGENERATIONAL OCCUPATIONAL MOBILITY: COMPARISONS OF PATTERNS? *

This note is addressed to two major problems with "Patterns of Female Intergenerational Occupational Mobility: A Comparison with Male Patterns of Intergenerational Occupational Mobility" (DeJong, et al., 1971:1033-42). (1) The findings, and hence the conclusion of "no differences" between male and female patterns, are questionable. (2) The theoretical inferences about female status attribution and role conflict cannot be derived from the empirical analysis.

(1) Although the authors (1971:1033) state "the expectation that occupational mobility of females should be different from that of males," they employ a technique of analysis not designed for comparing populations with differences. They do not attend either to important documentable differences between the occupational distributions of females and males or to the differential rewards males and females receive for occupational activity.¹ In addition, the authors do not attend to specific differences by sex which can be observed in the data they present. All of these oversights have implications for their findings and for their conclusion that there are "essentially no differences between female and male patterns of mobility" (1971:1033).

The authors use mobility ratios as their base for comparison. However, this statistical procedure is appropriate only if one assumes that the two populations have the same marginals. Blau and Duncan (1967:94) state that mobility ratios do *not* remove differences. The "supposition that the mobility ratios 'remove the differences,'" making populations comparable with respect to occupational structure leads to inaccurate conclusions. If two populations have different marginals, their mobility ratios are not comparable (see Blau and Duncan, 1967:90-7). In the 1960 U. S. Census data, major differences are apparent in the relative

distributions of females and males among gross occupational categories.² Since the authors state (1971:1041) that the occupational distribution of their female sample is "virtually identical to the 1960 census occupational distribution of working females" and that their male sample "shows equally high agreement," we assume their data would reveal similar differences. Yet, the differential distribution (different marginals) for males and females who come from similar origins³ indicates that the use of social distance mobility ratios to compare these populations is inappropriate. Therefore, the conclusion that their occupational mobility is similar is unwarranted. For example, Table 1 shows that two populations with the same distribution of origins, but with very different destination patterns, can have similar mobility ratios (at least as similar as those presented by DeJong, Brawer, and Robin).⁴ Indeed, this example illustrates that such "similarity" might be found between any two populations, e.g., ethnic populations, even those whose destination distributions are opposite.

In the 1960 Census data, major differences are also apparent in the relative distributions of females and males *within* gross occupational categories. Females are disproportionately over-represented in the lower status and lower paid

² The following major differences were found in our comparison of the relative distributions of females and males among gross categories: 58.6 percent of the females were located in the clerical and service categories, but only 14.1 percent of the males; 1.6 percent of the females were located in the craftsmen category, compared to 20.6 percent of the males; 17.1 percent of the females were situated in the three most remunerative categories of professionals, managers and craftsmen, compared to 46.3 percent of the males. The data for these percentages come from the U.S. Bureau of the Census (1963: Tables 16 or 30). The percentages are based on those who worked fifty to fifty-two weeks; therefore, the comparisons are of females and males most likely to have similar occupational patterns.

³ The authors state (1971:1041) that "the distribution of fathers by occupation for the Blau and Duncan males is also in close agreement with the occupational distribution of fathers of both male and females in our sample DeJong 1969:42-9).

⁴ Along the major diagonal in the authors' Table 1, six of the ten differences are greater than as shown in our example.

* We are indebted to Norval Glenn and Elton Jackson for comments and advice.

¹ On these differences see for instance: Bancroft (1958); Bogue (1969:213-30); Epstein (1970); Knudsen (1969); National Manpower Council (1957); Oppenheimer (1970); Smuts (1959); Women's Bureau (1969).

Table 1. Illustrative Mobility Ratios for Two Populations with Similar Occupational Origins.

Males					Females				
Son's Occupation					Daughter's Occupation				
	H	M	L			H	M	L	
Father's Occupation	H	1.0	1.0	.60	Father's Occupation	H	.60	1.0	1.0
		(70)	(25)	(5)			(5)	(25)	(70)
	M	.90	1.3	.80		M	.80	1.3	.90
		(180)	(100)	(20)			(20)	(100)	(180)
	L	1.1	.50	1.5		L	1.5	.50	1.1
		(150)	(25)	(25)			(25)	(25)	(150)
		400	150	50			50	150	400
				600					600

occupations within major categories.⁵ This differential distribution suggests that the "masking effect" so readily dismissed by the authors (1971:1040fn) operates throughout their classificatory scheme and may partly account for their "convergence of findings."

Another factor which may influence the authors' findings of similarity in occupational inheritance and in the direction of mobility is the use of a male occupational ranking to index the positions of origin and destination for both males and females. Such a scheme ignores the possibility that occupations are ranked differently for females and males.⁶ This possibility

makes questionable the basic procedure of comparing father's occupation to daughter's to indicate mobility or inheritance. A different ranking for males and females would also make the authors' comparisons between son and daughter mobility questionable. For instance, suppose the sales category is ranked differently for males and females. If a daughter has a sales father and is destined for a sales position would this be inheritance or a case of mobility? Would it mean the same thing for a daughter as for a son to move into the father's occupational category?

The error of inappropriate data analysis is compounded by the authors ignoring between-sex differences in their findings.⁷ For example, regarding similarity in the concentration of supply, three differences are notable in Table 2, column 1. The difference for the clerical category is +15.1, for the service category +12.4, and for the craftsmen category -12.1. These differences are for the most sex-typed occupational categories.

Finally, regarding similarity in the nature of barriers to mobility and in the distance of mobility, analyzing mobility in terms of the white-blue collar distinction hides more than it reveals of mobility patterns; and this may be

⁵ For example, using a ratio of percent female in the professional category to percent male, females are underrepresented in the following occupations (among the highest paying jobs in the category): physicians and surgeons, dentists, lawyers and judges, and architects. However, females are overrepresented in these occupations (among the lowest paying jobs in the category): social workers, elementary teachers, librarians, nurses, and medical and dental technicians. Similar patterns exist within the clerical, sales, operative, and service categories. These data come from the U.S. Bureau of the Census (1963: Tables 16 or 30). The analysis is based on those who worked fifty to fifty-two weeks. We have not included a tabular presentation of our findings, but the tables are available for any who desire them.

⁶ For example, using 1960 U. S. Census data, a rank ordering of gross categories by the criterion of median earnings revealed important discrepancies for females and males. The major discrepancy was for the sales category which ranked 7 for females and 4 for males. Furthermore, the most remunerative occupational category for females (professional) corresponded to the seventh ranked

category for males (laborer). Similar differences were found for intra-category comparisons. The data on earnings come from the U. S. Bureau of the Census (1963: Tables 16 or 30). Earnings are for those who worked fifty to fifty-two weeks.

⁷ Since no marginal frequencies are reported for any of the tables, the reader cannot interpret the data for himself. Therefore, we cannot determine how important these unconsidered differences are.

especially true for female patterns (for a discussion of the weaknesses of the manual-non-manual dichotomy see Duncan, 1966:83fn). 1960 Census data indicate that for females three "blue-collar" gross categories rank above the sales category by the criterion of median earnings.⁸ Particularly in the analysis of female occupational mobility patterns, the traditional white-blue collar division may thus conceal much important and relatively *long-distance* movement.

(2) The relationship between the theoretical framework and the empirical analyses seems to reflect two types of errors. While the theoretical framework deals with questions of causes for males and females participating at different levels in the occupational structure, the empirical analyses deal with intergenerational mobility patterns with much differential participation held constant. While the findings pertain to macro-level intergenerational mobility patterns, the hypotheses and inferences are phrased in terms of micro-level phenomena such as the "role-conflict" and "status attribution" of females. The analyses do not deal with differential participation, and the findings do not pertain to questions of female status attribution and role conflict.

The authors' primary hypothesis appears to be that certain factors, e.g., role conflict, affect female participation in the occupational structure. As they (1971:1033) state: "This role conflict may result in females working at lower occupational levels than would be predicted from their own education or father's occupation." In other words, hypothesis one asserts: If males and females differ in role conflict, then females and males will participate differentially. Females will participate at lower occupational levels. The authors (1971:1034) also suggest a second hypothesis: "If American females must function in the occupational role in a manner different from that of males, then we would expect these differences to produce divergent occupational mobility patterns." In other words, hypothesis two asserts: If males and females participate differently, then they will have different intergenerational occupational mobility patterns.

The authors only offer findings relevant to the "result" (see Zetterberg, 1965:64) of their second hypothesis. They never treat the first hypothesis empirically. They simply infer backwards, i.e., from intergenerational mobility patterns to participation to role conflict. Since they find "no major differences" in the intergenera-

tional occupational mobility patterns of male and females they reason that no major differences in participation exist, and therefore, nor exist in role conflict and status attribution.

Such reasoning occasions major problem for example, the perceived anomaly (1971:1041):

There seems to be no ready explanation for the anomaly that other research shows that females are recruited into lower level jobs, while the research shows that they have the same occupational mobility patterns in relation to the fathers as do males.

Inferring backwards leads them to conclude that documentable differences between male and female participation do not exist. However, females and males were expected to participate differently in the occupational structure, the data should have been examined to see if they do participate differently. The question of differential participation is empirical—indeed, constitutes the "result" of the authors' first hypothesis. Yet, while the "result" of similarity in participation is only inferred, it is used to "indicate" further similarity in role conflict and status attribution.

Finding "no major differences" in the intergenerational occupational mobility patterns of males and females, the authors (1971:1041) conclude that female role-conflict has been exaggerated or is lessening and that females may not derive their social status from males. The findings do not support their inference about participation. And, their data are not pertinent to conclusions about female role conflict and status attribution.

We are at a loss as to how "the accuracy and applicability of prior theory and analyses of female roles are called into question" (DeJon Brawer, and Robin, 1971:1041) by the authors' analysis. A dangerously large analytic leap is required to make inferences about variation in psychological variables such as sex role conflict and status attribution from findings about patterns among structural variables such as intergenerational occupational mobility.

One might surmise that level of participation is related to considerations of "sex-appropriate jobs and to discrimination, as well as to role conflict per se and the discontinuity thought to result from it. However, since no data relevant to issues of differential participation are presented, one can only speculate.

In conclusion, we find it an interesting possibility that when the traditional gross occupational categories are used, father's occupation classification is "about" as good for predicting occupational classification for working daughters as it is for sons. Note however that we do not

⁸ Source: U. S. Bureau of the Census (1963: Tables 16 or 30).

's occupation is related to a
into the labor force, i.e.,
s or does not enter the labor
question the general conclusion
ce" exists in the occupational
s and females, and the inferences
aw, ostensibly from their data,
respect to role conflict and status

ELIZABETH M. HAVENS
JUDY CORDER TULLY
University of Texas at Austin

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REPLY TO HAVENS AND TULLY

Professors Havens and Tully raise a series of questions about our analysis, conclusion and theoretical bases. We will respond to their points in the order they present them:

Professors Havens and Tully are more ambi-
tious for our research than are we. We, too,
chafe at the unaddressed and unanswered ques-
tions in this area, but have refrained from the
specific comparisons and implications for which
we are castigated. We address ourselves only
to *patterns* of intergenerational occupational
mobility. Thus, we agree that males and fe-
males receive "differential rewards" for "occu-
pational activity," and we do not posit notions
of equality or equal psychic or monetary re-
ward on the basis of our findings. Such issues
raise a series of important questions which our
limited research was not designed to explore.

The same observation may be applied to our
strongly criticized use of the mobility ratio. We
analyze and interpret the mobility ratios only
in terms of the general mobility patterns each
matrix suggests for the sexes; we do *not* at-
tempt the more specific cell by cell comparisons
pronounced unreliable by Professors Havens
and Tully. We do not suggest that the volume
of mobility into and out of each occupational
category is the same for females and males,
only that male patterns are reproduced by work-
ing females in our national sample.

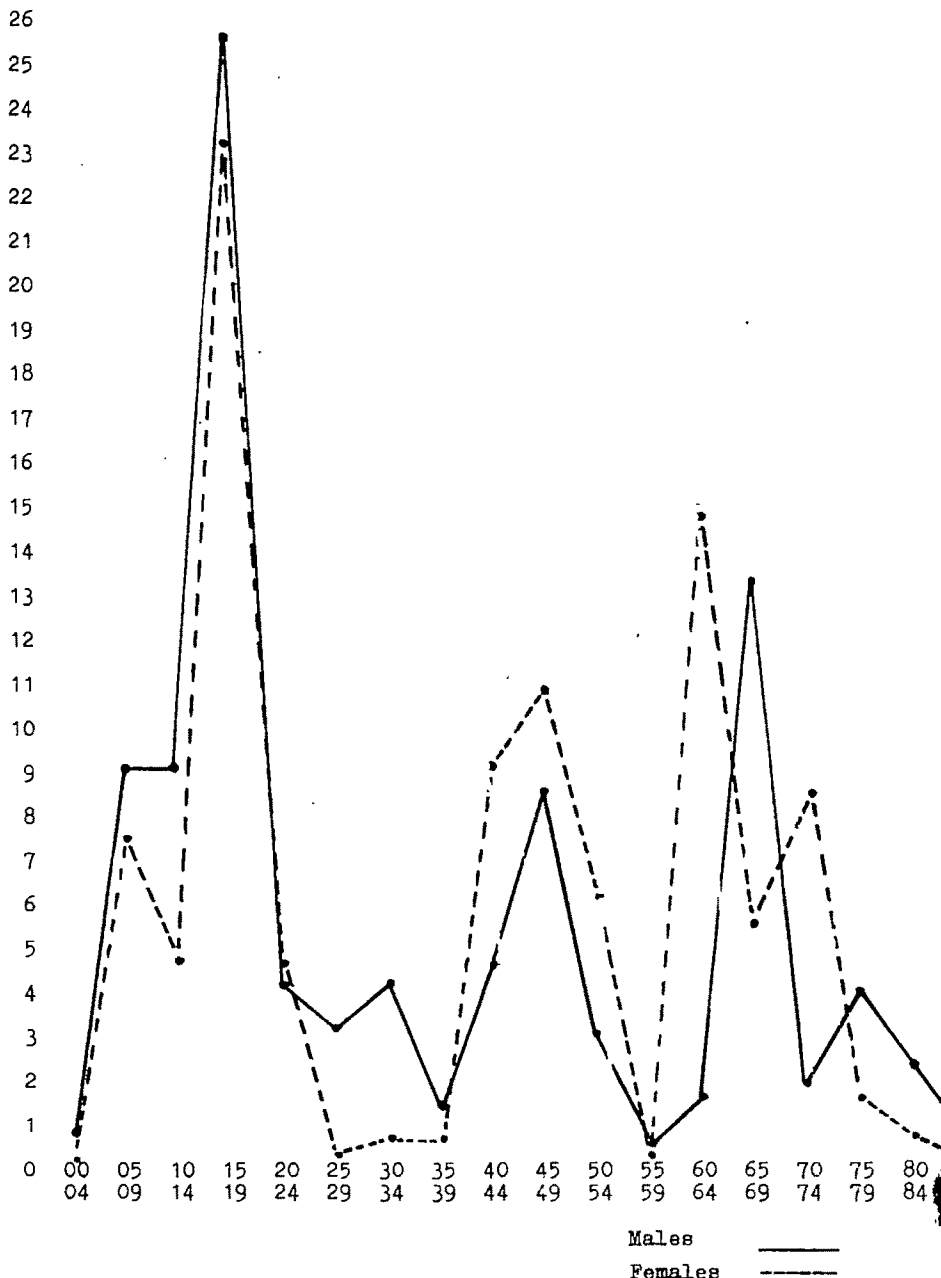
However, Havens and Tully's criticism of the
use of mobility ratios may be overstated. Blau
and Duncan, while noting the limits on mobility
ratios for comparing populations with different
marginals (Blau and Duncan, 1967:90-7), do
make such comparisons (with appropriate cau-
tions) across nations with different occupation
distributions (Blau and Duncan, 1967:433-4).
We are more cautious, confining our interpreta-
tion to patterns. Blau and Duncan's use of mo-
bility ratio matrices (pp. 433-4) can perhaps be
further justified by an examination of Havens
and Tully's hypothetical mobility ratio tables.
Comparison of males and females in their Table
1 shows that their focus on cell by cell com-
parisons apparently blinded them to the fact
that the patterns of occupational mobility do
indeed differ for their hypothetical populations
of males and females. For males, the diagonal
cells (1.0, 1.3, 1.5) support a conclusion of
greater than chance occupational inheritance.
Such a conclusion for females (for whom the

diagonals are .6, 1.3, 1.1) would not be warranted. Similarly, their females show a pattern of greater downward mobility than their males. Thus, even in Havens and Tully's Table 1, the patterns reflect the differences in the marginals.

Havens and Tully are also concerned (as were we, De Jong, et al.:1040) about the possibility of a "masking effect" caused by ours and others' use of "gross" occupational categories. The figure below contains a further analysis of our data by occupational status weights (Blau

and Duncan, 1967:119-23) to that point.

The patterns of status weight pronounced similarity for males and tend to confirm our convergence of patterns is not categorizing. While there are differences, they do not negate the similarity of these data, though buttressing our interpretations, were not available to us at the time of our presentation.



More serious is the criticism that "... the authors' findings of similarity in occupational inheritance and in the direction of mobility is (influenced by) the use of a male occupational ranking to index the positions of origin and destination for both males *and* females." They are completely justified in questioning this procedure. The ranking of occupational categories for females may differ from that of males. Their suggestion, however, that this possibility would necessarily decrease similarity of patterns between males and females can be questioned. The issue raised calls for future research.

We are not certain we understand the relevance of Havens and Tully's comments about supply concentration findings. At best their conclusions about sex-typed occupational differences is inaccurate: note the similarity of indices for females and males for farmers—an exceedingly sex-typed category (De Jong, et al.:1037). More to the point, the connection between sex-typed occupations and supply concentration is unsupported by the nature of the measure and especially the removal of inheritance from the calculation (see Blau and Duncan, 1967:64-7 and De Jong, et al., 1971: 1037-8).

Havens and Tully object that the analysis by categories of white collar-blue collar-farmer may "conceal much important and relatively long distance movement." This possibility exists for any summary measure. The charge that this is particularly so for females is inadequately

supported by their citation. Data strongly support the convergence of barriers for males and females. Why this convergence may be not so could be examined *ad infinitum*, but ultimately we must reach a tentative conclusion supported by reasonable if not perfect data.

At this point in their commentary Professors Havens and Tully misquote, misread and apparently misunderstand both our theoretical rationale and generalizations:

(1) They confuse our summary of others' conclusions with the hypotheses actually advanced.

(2) The designation of the rationale as micro-level is in error. There is nothing necessarily psychologistic or micro-level about social roles and status attribution.

(3) The charge that we never treat our first hypothesis would be correct if it were "our" hypothesis. It is not.

(4) Finally, the series of questions and anomalies raised at the conclusion of our article are treated as assertions. Havens and Tully would have us interpret what we have said we cannot interpret and have us conclude what we have left in question.

PETER Y. DE JONG

Calvin College

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Western Michigan University

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ITEMS—(Continued)

■ **William H. Form**, Professor of Sociology at the University of Illinois, is also associated with the university's Institute of Labor and Industrial Relations. A monograph (*Technology and Social Behavior*) is in preparation, focusing on the impact of technology on the integration of workers in various social systems. A monograph (*Income and Ideology*) has recently been completed, and another (*Political Stratification*) is in preparation, both with Joan Huber.

■ **Theodore D. Kemper** is Associate Professor of Sociology at Queens College. Currently he is working on a general theory of social relations focusing on dimensions of power and status relations.

■ **Larry L. Bumpass** and **James A. Sweet** are Associate Professors at the University of Wisconsin, where Sweet is also Director of the Center for Demography and Ecology. Both are involved in further analysis of fertility and marital instability.

■ **C. Norman Alexander, Jr.**, Assistant Professor of Sociology at Stanford University, is working on a new approach to defining situational meaning and social activity. His formulation of "situated identity theory" operationalizes symbolic interactionist concepts and employs these experimentally.

The issue ends with an exchange concerning a previously published article, and the inevitable (apparently) erratum. Our apologies for the latter and our thanks to all our contributors.

J.F.S.

ERRATUM

Footnote 13 on page 372 of Professor John H. Barnsley's comment, "On the Hobbesian Problem of Order: A Comment," which appeared in the June, 1972 issue of *ASR*, should read as follows: "That is, simply, the falsity of ' $a \neq > b$ ' does not entail the truth of ' $b \neq > a$.'" The two signs here represent, respectively, the possibility and impossibility of logical implication; hence, the oblique stroke through the second "equals" sign is necessary.